A FRESH ESSAY ON DUTY AND RESPONSIBILITY

Evidence suggesting that the alleged “original manuscript” of which a purported “translation” appeared in David Selbourne’s *The City of Light: An Authentic Traveler’s Tale* is either a nineteenth- or twentieth-century fake or, more likely, has never existed\(^1\)

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Abstract

The main part of *The City of Light: An Authentic Traveler’s Tale*, first published in 1997 by David Selbourne, is what he claimed to be his English “translation” of what he claimed to be the account of a trip from Italy to China and back which he claimed was written in the thirteenth century by someone whom he called “Jacob of Ancona,” who, he asserted further, made that trip. At least thirty people have stated for the record (in reviews, letters to the editor, and letters to the author of the present essay) that in their opinion either the alleged manuscript containing the alleged account does not exist (if they are right, what Selbourne claimed was his “translation” is an original English text, presumably written by him) or, if the alleged manuscript does exist, it is a fake probably dating to the nineteenth or, more likely, the twentieth century.

Inasmuch as only Selbourne claimed to have seen the alleged “original manuscript” (on many occasions between September 1991 and June 1996) though he refused to say precisely where he saw it or reveal anything about its alleged “present owner,” we have (a) no way of verifying its existence, (b) no way, if it exists, of determining its authenticity, and (c) no way, if it exists and is authentic, of determining its degree of factualness.

Strong circumstantial evidence of several kinds leads us to conclude that either the alleged “original manuscript,” if it exists, is a fake or, more likely, that it does not exist. To the evidence brought forth by earlier critics the author of the present essay adds more, gathered from his examination of three imprints of *The City of Light* (Selbourne 1997a, Selbourne 2000, and the French version of his book), from Selbourne’s few letters to the editor in which he tried to defend himself, and from the few defenses which others have published.

*Reaction to Selbourne’s book was mostly positive in the People’s Republic of China and mostly negative elsewhere, a cleavage attributable to three factors:

A. Overall, standards of research are much higher outside that country. For instance, whereas producing the alleged “original manuscript,” authenticating it, and determining the degree of factualness of its contents are in that country not felt to be important, elsewhere those three steps are deemed essential before any text purported to exist and be factual can be considered further. Indeed, the normal procedure in the highest research circles when claims like Selbourne’s are evaluated is to consider the alleged “original manuscript” not to exist until it is proven to exist; if it is proven to exist, it is not considered authentic until it is proven authentic; and if it is proven authentic, its degree of factuality has to be determined. See subsection K of section 22.

B. Either Selbourne’s supporters in that country were unaware of the extent of the criticism elsewhere (for example, none gave any sign of knowing that many non-Chinese aspects of the book too are judged to be unauthentic) or they did not know that the criticism was often based on damning evidence.

C. Since a large number of people in that country belief that anything positive that you say about them or where they live, whether right or wrong, is worthy of praise, the fact that the alleged “Jacob of Ancona” allegedly visited China and allegedly described what he allegedly saw in the alleged account was sufficient warrant to accept the claims that such an account exists, is genuine, and is factual. That belief is absent elsewhere.
The earliest criticism of Selbourne’s book was in the form of attempts to prevent its publication (apparently, the publisher of the first imprint had sent galley proofs to researchers in certain relevant fields in an attempt to get blurbs but instead got condemnations). Whereas Selbourne saw those attempts as an infringement on his right of free speech (he spoke of the critics’ opposition to his disseminating what he termed “new knowledge”), the critics see the book as such an affront to the truth and as such a perversion of scholarship that it deserves suppression, for, though they agree that free speech is important, they also believe that it is not absolutely free. As Oliver Wendell Holmes, Jr., said, “The most stringent protection of free speech would not protect a man in falsely shouting fire in a theater and causing a panic.” Therefore, one should not make claims about an “original manuscript” and so forth if one has not first proven the existence of that document, then proven its authenticity, and then determined its degree of factualness, especially if overwhelming evidence suggests a fabrication of some kind.

The present essay is relevant to these fields: Arabic, Asian studies, Chinese studies, East-West relations, ethics in publishing, ethics in reporting, ethics in research, etymology, Indian studies, Italian studies, Jewish studies, Latin, lexicology, linguistics, medieval studies, Mediterranean studies, Persian, standards of proof, travel literature, and more.
“Everyone is entitled to his own opinion, but not to his own facts” (Daniel Patrick Moynihan, 1927-2003)

“Don’t believe the man who tells you there are two sides to every question. There is only one side to the truth” (William Peter Hamilton, editor of the editorial page of The Wall Street Journal in the early twentieth century).

“It is incumbent on every person who presents a statement of important events to the public, to unfold the sources from which he derives his information” (Robinson 1820:iii)

“Extraordinary claims require extraordinary evidence” (Carl Sagan, 1934-1996)

“In recent years it seems that there have been more and more blowups over the question of whether book publishers actually care about the veracity of the material in the nonfiction books they publish. Or to put it another way: there appears at times little to indicate that the culture of the industry requires what could be described as a predisposition among editors and publishers to be ferocious at authenticating” (Arnold 2000)

“The reception given to a writer by his contemporaries and near-contemporaries is evidence of considerable value to the student of literature. On one side we learn a great deal about the state of criticism at large and in particular about the development of critical attitudes towards a single writer; at the same time, through private comments in letters, journals, or marginalia, we gain an insight upon the tastes and literary thought of individual readers of the period. Evidence of this kind helps us to understand the writer’s historical situation, the nature of his immediate reading-public, and his response to these pressures” (Southam 1971:v)

“‘A Death in Belmont’ must be read with the appropriate caution that should surround any work of nonfiction in which the author is seeking a literary or dramatic payoff” (Dershowitz 2006)

“A historian who finds it so difficult to distinguish truth from fiction [...] is best advised to lay down his pen” (Abulafia 2007 referring to Ariel Toaff)

“Anachronisms are the rock on which counterfeit works almost always run most risk of shipwreck” (Farrer 1907:2)

“O sir, to wilful men / The injuries that they themselves procure / Must be their schoolmasters” (William Shakespeare, King Lear, act II, scene 4)

“Oh, what a tangled web we weave, When first we practice to deceive!” (Walter Scott, 1771-1832)

Wherever there is a secret there must be something wrong (an English saying)

What you do in the dark will come out in the light (an Alabama saying)

“Die Weisheit ist nur in der Wahrheit” (Johann Wolfgang von Goethe, 1749-1832) ‘Wisdom is found only in truth’

“Ethical inquiry is the foremost task of mankind” (Walton 2001)
1. Introduction

2. The City of Light: An Authentic Traveler’s Tale

3. Italian anticipatory anachronisms in the alleged “original manuscript”


5. A Latin anticipatory anachronism in the alleged “original manuscript” (*domini canes*)

6. A Hebrew anticipatory anachronism and other unusual Jewish-related items in the alleged “original manuscript”:
   A. “Heshvan”
   B. “Amen, Amen and Amen”?
   C. “peace upon him” and “peace upon them” after names of living people?
   D. “the Unnameable One,” “He, may His name be unspoken,” and “His unspoken name”?
   E. “land of Sinim” ~ “Sinim” ‘China’?

7. Selbourne will not have the last word: a Moroccan Arabic anticipatory anachronism and ectopism in the alleged “original manuscript” and his lame responses

8. A. No Jews known in twelfth- and thirteenth-century Florence, Arezzo, Fano, or Sinigallia
   B. Still more unusual Hebrew
   C. The non-existent Jewish “Book of Forgiveness,” and a misunderstood Jewish custom

9. A. The Gemara in Hebrew?
   B. The Mishna and the Talmud separate works?

10. Aquinas but nothing about the controversy over Maimonides?

11. A. “San Giovanni d’Acri” and “the holy fast of Ramadan” from a Jew as Jewish and as anti-Christian as the alleged “Jacob of Ancona”?
   B. A misuse of the glottonym *Frankish*
   C. Another First for the Fictitious “Jacob”: Barcelona is in Aragon
   D. “Lazzaro Ha-coen”?
   E. “Ioshua”?
   F. “Menorah (hébreu): chandelier à sept branches utilisé à Hanoukkah”?

12. Selbourne’s suggestion that the alleged “original manuscript” might not be the real “original manuscript”

13. The Jewish and Christian calendars, monsoons, and winds

14. Selbourne will not have last word: a triple-barreled smoking gun: “Toutson”

15. A “translation” into English before publication in Italian? (On the possible relationship between Ancona, Urbino, and Florence)

16. The uncanny resemblance between (1) the alleged “Jacob of Ancona”’s political, social, ethical, and philosophical preoccupations, and (2) David of Urbino’s

17. A. A newborn Jewish male’s circumcision before the eighth day after birth?
B. The Sanctification recited at a wedding?
C. The Sanctification recited at the end of the Sabbath?
D. The Torah commands Jews to invite everybody to a wedding?
E. The alleged “Jacob”’s unusual way of marking the thirty-third day of the Counting of the Sheaf
F. A suspicious excess of Jewish-related details

18. The imaginary “Jacob,” the real “Jacob,” Marco Polo, Benjamin of Tudela, the Jews of Cochin (again), and allegedly “lost” books -- is there not a connection?
19. Selbourne will not have the last word: again on “Baiciu” ~ “Banciu” ~ “Bae-Choo” and Huang Chao

20. Wang Lianmao’s “Remarks on The City of Light”

21. Eleven reasons why the present essay did not appear earlier

22. Final remarks (for the time being):
   A. What Selbourne should have done
   B. A dissection of Selbourne’s “acknowledgments”
   C. Did Selbourne’s helpers and supporters have any relevant credentials?
   D. Will Selbourne open his files and can he leave us an ethical will?
   E. The reaction to Selbourne’s book in the highest relevant scholarly circles (the only reaction that counts)
   F. The more likely possibility: no “original manuscript” has ever existed
   G. Selbourne later admitted that he could have written “Jacob”’s tale
   H. Selbourne’s deficient knowledge of Latin
   I. Does Selbourne now regret undertaking his enterprize?
   J. Outdoing Al Capone
   K. Let us bring the matter before a court of law
   L. In sum
1. Introduction

Imagine not only that you were a publisher offered a translation of a hitherto unknown, unpublished late-thirteenth-century 280-leaf manuscript containing an unusually detailed, unusually reflective first-person account of a journey undertaken by an Italian Jew from Ancona to China and back but also that although he and Marco Polo started out from Italy almost at the same time, the Jew left earlier (allegedly on 16 April 1270; Polo left in 1271) and the Jew got there first (allegedly on 25 August 1271; Polo, in 1274 or 1275). Your reaction would probably be the strongest desire to publish the original manuscript in facsimile, with a transcription if lay readers could not easily read the original, with a translation into some widely used language of today, and with the entire critical apparatus that the highest requirements of today’s research demanded for such an extraordinary find. Because such a project would require deep knowledge in many disciplines and nobody could be expert in all of them, you would want to put together a team of the best relevant researchers to carry it out.

Before taking a decision, however, on whether to proceed, you would feel an equally strong desire to have the manuscript authenticated from every possible angle (provenance, paper, ink, handwriting, language) and, if it turned out to be authentic, to determine how factual it was.

Yet imagine that the person offering you his alleged translation of the alleged manuscript, his commentary on the alleged text, and the rest of his critical apparatus (all in English, with a transcription of just a few words or phrases in the alleged manuscript) said that, although he saw the alleged manuscript many times between September 1991 and June 1996, he was now (late in 1996 or early in 1997) not at liberty to show you even a single line of the original manuscript (much less the entire manuscript), not even a photocopy of a single line (much less a photocopy of the entire manuscript), because, as the person offering the alleged “translation” claimed, the owner of the manuscript feared that if he allowed copying of any kind, his “anonymity” would be “broached” (Selbourne 1997d).

Furthermore, the person offering the “translation” said that he was not at liberty to reveal the present location of the manuscript, not at liberty to disclose anything about its anonymous present owner (because that person feared that the Italian government or someone else might try to assert rightful ownership of the manuscript), not at liberty to say anything about its provenance except that a Jewish family in Italy, whom he did not name, had kept it for generations and that the present owner was a non-Jew living “near Urbino” (thus, not far from where the person offering the alleged “translation” lived), and not at liberty to provide anything more than what he had brought you, namely his translation and his commentary.

Thus, you would be asked to take on faith every detail of the little which the offerer chose to reveal.

On hearing that story, you would suspect a hoax: saying that one cannot produce the evidence, in this case the alleged “original manuscript,” often means that the evidence does not exist; saying that one cannot produce even a photocopy of even one page of an allegedly 280-leaf manuscript is even more suspicious (how could a photocopy of even just a page, you would ask yourself, compromise the “present owner”’s anonymity?) -- and you would show that person the door, as Arnoldo Mondadori Editore S.p.A., a large Italian publishing house, rightly did after Selbourne, so he alleged, offered it the manuscript of his book but said that he was unable to produce the “original manuscript” or reveal precisely where it was or who owned it. If you have not already concluded that David Selbourne’s story was preposterous, read on.
2. The City of Light: An Authentic Traveler’s Tale

Several publishers were not so scrupulous as Mondadori and the publishers mentioned in note 2. Since 1997, The City of Light: An Authentic Traveler’s Tale has appeared in at least sixteen versions in at least nine languages (see the entry for Selbourne 1997a in the References). The book consists of what Selbourne, a resident of Urbino and self-styled “editor” and “translator” of the alleged “original manuscript,” asserted was his English “translation” of what he claimed to be an “original manuscript” written, he claimed, largely in “Italian vernacular” by someone he dubbed “Jacob of Ancona,” who, he said, lived in the thirteenth century and, Selbourne asserted further, traveled from Ancona to China and back in 1270-1273, the purported manuscript being the alleged “Jacob”’s purported account of that purported trip (Selbourne 1997a).

Numberless elements of what we may call Selbourne’s enterprise (= Selbourne’s story [= his “explanation” of how the alleged “original manuscript” came into being, how it allegedly survived for over seven hundred years down to our times, how he allegedly learned about it, how he allegedly came to “translate” it, how the alleged “present owner” allegedly got angry and therefore broke off all contact with him when Selbourne told him that he had been to Mondadori, and so on] + “Jacob”’s tale [= the alleged text in the alleged “original manuscript”] + Selbourne’s critical apparatus [= his comments on the form and content of the tale] + Selbourne’s defense [= his self-defense + the statements of his supporters]) lead us to conclude that no evidence supports the story, that the story is improbable or preposterous in many ways, and that either the “original manuscript,” if it exists, is a twentieth-century fake or, more likely, no such manuscript exists (in which case Selbourne’s “translation” is an original English text, presumably written by him).


Among the few believing in the existence and the authenticity of the “original manuscript” purportedly translated in The City of Light: An Authentic Traveler’s Tale were:

A. Tudor Parfitt, who said, “He told me, looking me in the face, that this is genuine. I tend to believe in what people tell me. I believe him an honorable man, but some of the criticisms are fairly difficult to counter. What we’re all waiting for is him to come out of his corner with his manuscript” (quoted in Carvajal 1997). We return to him in section 7.

B. Henri Gambourg, whom Selbourne, in one of his frequent flights of fantasy, described as “a French scholar of the participation by Jews from biblical times onward in the trade with the Orient in silks, spices, and other things, tells me that the account by Jacob of Ancona ‘confirms [his] work of 20 years’” (Selbourne 2001). Note 143 brings Selbourne back to reality.
C. “Professor Chun-shu Chang of the Department of History at the University of Michigan [...] ‘found no conclusive evidence to support the theory that the book was a forgery’ -- believes it to have been written in the late 1290s” (Selbourne 2000:442). The same could be said of realistic fiction written in the twentieth century by a champion of literary realism and set in the thirteenth. In the present case, moreover, we have no evidence that the alleged “original manuscript” exists whereas we do have much evidence that the alleged “translation” is so bulging with inauthenticities that, even if the existence of the “original manuscript” were proven, it would have to be dismissed as a fabrication of some kind (though the likelier possibility is that the only “original manuscript” is the first draft of an original piece of writing by Selbourne in English, namely what he called his “translation”). Chang thus started with the question of factualness (stage 3) when the alleged document has not been produced (stage 1) and authenticated (stage 2). Just as reputable zoologists would not try to describe an animal they had never seen (not even its tiniest bone, not even the thinnest sliver of its flesh, not even in a photograph) if the only evidence for it was the affirmation of one eyewitness, whose story, moreover, was riddled with contradictions and other anomalies of such high number and seriousness that most judges dismissed him as an unreliable witness. Chang, like Gambourg and Selbourne’s few other supporters (see section 20 and note 143), did not espouse rigorous standards of evidence -- because they were unaware of them.

D. “So far, Dr. Luc Kwanten is one of the few academics embracing it. ‘Authentic it is,’ he declares in a fax. But the former University of Chicago professor is far from impartial--it turns out that he’s brokering the Chinese-language rights for Little, Brown U.K.” (Chang 1997). We wonder how he could pronounce “it” authentic if he never saw “it,” we have no proof that “it” exists,” and we in fact doubt that “it” does exist. What, moreover, were Kwanten’s credentials in the analysis of paper, ink, handwriting, and so on (to say nothing of credentials in the many non-Sinological disciplines relevant to Selbourne’s book)?

E. Several supporters in China, of whom the most outspoken was Wang Lianmao, director of the Maritime Museum in Quanzhou (formerly called at least Ch’üan-chou, Chuanchow, Tsinkiang, Zaitun, Zaytun, and Zayt_n in English) and secretary-general of the Research Association for the History of Chinese Contacts with the Outside World of the People’s Republic of China. His enthusiastic support derived presumably in part from the fact that the alleged “Jacob of Ancona” thought he arrived in the city of “Zaitun” (the name which Selbourne chose to use in his “translation”) and proceeded to give a detailed description of the place (thus, Wang’s city) and presumably in part from a desire to emphasize “Chinese Contacts with the Outside World.” However, though the alleged “Jacob of Ancona,” according to Selbourne’s “translation,” indeed believed himself to be in that port, his description was actually that of a different Chinese port, Zhangzhou (formerly called at least Changzhou, Chang-chou, and Longxi in English).

The clincher here is that, so far as we know:

A. Confusion of the two cities dates only to the end of the nineteenth century.

B. The confusion originated with one Sinologist, G. Phillips, active at that time, who apparently had no first-hand knowledge of either city.

C. Any later confusers copied either from Phillips or from anyone who might have copied from him.

D. Anyone acquainted with at least one of the two cities (as the alleged “Jacob” was) would not confuse them.
We therefore conclude:

**A.** if one is not charitably disposed toward Selbourne, that the alleged “Jacob of Ancona”’s confusion of the two cities is another bit of evidence that the purported “original manuscript” either does not exist or, if it does, that it is a latter-day fake from the pen of someone who, unacquainted with both cities, copied, either directly or indirectly, from Phillips.

**B.** if one is charitably disposed toward Selbourne, that “Jacob of Ancona” confused the two cities way back in the thirteenth century on his own and nobody, so far as we know, was to do so again until the end of the nineteenth century, this time G. Phillips, whose confusion is independent of any earlier confusion.

However, even if one decided to be charitable, one would then realize that whereas G. Phillips’s mistake is easily explained as a result of his lack of first-hand knowledge of either of the cities, “Jacob of Ancona”’s confusion could not be explained away in that fashion because Selbourne’s “translation” states that “Jacob” visited Quanzhou. If you’ve lived all your life in Mongolia and you confuse Philadelphia and New York City by writing a description of one city that is actually that of the other, you’re guilty only of misusing your sources, but if you claim to have visited at least one of the two cities and your description of it turns out to be that of the other, it is clear that you in fact visited neither and, instead, either copied from some unreliable source that was itself based on lack of first-hand knowledge of both places or you used accurate sources for each city but mixed them up. In the end, therefore, whether or not we are charitable, Selbourne came out the loser.

Furthermore, since “Jacob”’s confusion of the two cities is one which Wang should have for two reasons caught immediately (he is director of the maritime museum of one of the two cities and he is secretary-general of an association studying contacts of foreigners with China), he should have been among the first to spot the confusion. Since he did not, we wonder whether Wang read Selbourne’s book carefully, whether he read all the negative reviews and letters to the editor (as we see from note 6, it was publicly stated in 1998 that “Jacob” confused the two cities), whether his standards of research were high, and whether he is a qualified student of Quanzhou and its history. What would you say of the director of the Museum of Maritime History of New York City (let us assume that such existed) if (s)he mistook a detailed description of Philadelphia to be one of New York City and what would you say of the director of the Association for the Study of American Contacts with Other Countries (let us assume that such existed) if (s)he enthusiastically endorsed a book about an alleged Spaniard’s alleged visit to the United States that for many compelling reasons did not ring true?

Or is it that de Rachewiltz and Leslie are wrong in saying that “Jacob” confused the two cities? If Wang or anyone else can show their criticism to be unjustified, let us have the details.

We will return to Wang in section 20, which will conclude that he was gullible and that his defense of Selbourne proved only that his standards of research were low.

As for Parfitt’s pious wish that Selbourne “come out of his corner with his manuscript,” even more than ten years after the first imprint of his book appeared we have still seen no “original manuscript.” How many more years does Parfitt suggest we wait? Or has he by now left Selbourne’s camp?

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3. Italian anticipatory anachronisms in the alleged “original manuscript”
Selbourne boasted that “No reviewer, so far as I recall, has remarked on” the several words from the alleged original which he incorporated either into the marginal notes of his purported translation or elsewhere in the book, those words, he claimed, being “an earnest, in the interim, of the authenticity of the text” (Selbourne 1997e).

Taking up his invitation to comment on those words (though wondering how long that “interim” will last), I offer an analysis of some of them in sections 3, 4, 5, 6, and 7, as well as here and there later in the present essay.

In sections 3 and 4, I proceed as follows. *Opera del Vocabolario Italiano*, compiled by the Italian National Dictionary Project, is a concordance to every known piece of writing in Italian up to 1375 and some beyond that year (expectedly and rightly, it has rejected Selbourne’s book as a source of information about the language). Since the concordance, available electronically, is constantly being updated, it is the best work of reference for determining the date of the earliest known use of all known Italian words up to 1375 and sometimes beyond. For words not listed in *Opera del Vocabolario Italiano*, Palazzi et al. 1992 is the single most useful source for dates of first known use, though since it has not been updated, it is less state-of-the-art with respect to dates than *Opera del Vocabolario Italiano*.

Reading Selbourne’s “critical apparatus,” I made a list of some of the Italian words each of which meets these three requirements:

A. Selbourne claimed that the word appears in the “original manuscript.”

B. The word is not in *Opera del Vocabolario Italiano* (thus, so far as we know, the word was coined after 1375).

C. The word is in Palazzi et al. 1992 (therefore, the date given next to the word below is the one in that dictionary).

Six words meet the three requirements (bear in mind that according to Selbourne the alleged “Jacob of Ancona” was born in 1221 and was alive in 1273; Selbourne indicated no year of death for that alleged person):

**List 1**

A. *brasile* ‘brazilwood’: 1555. The appearance of the word in Selbourne’s “translation” is not just a smoking gun (for being a dramatic anticipatory anachronism) but also a smoking cannon (inasmuch as Europeans became acquainted with the woods known in English as *brazil* and *brazilwood*, in French as *bois de Brésil* and *bois du Brésil*, in Italian as *brasile*, and so on in other European languages only in the sixteenth century, when Portuguese explorers in what is now Brazil came upon various species of wood later classified by botanists as belonging to *Caesalpinia* or related genera and Dutch explorers in what is now Malaysia came upon the wood later to be classified by botanists as being the species of *Caesalpinia* which they dubbed *Caesalpinia sappan* [= English *sapanwood* ~ *sappanwood*].

One would therefore have to suppose, if one were naive enough, not only that the alleged “Jacob” came upon sapanwood some three hundred years before any other European is known to have found it but also that, by sheer coincidence, he, like Italian-speakers beginning in the sixteenth century, called it *brasile*.
How curious, by the way, that the alleged “Jacob,” way back in the early 1270s, knew that centuries later botanists would classify sapanwood as one of the brazwoods (we know that “Jacob” possessed such knowledge because he called the wood, as Selbourne told us, *brasile*).

Had Selbourne been acquainted with brazwoods as well as the alleged “Jacob” allegedly was with *Caesalpinia sappan* and had Selbourne been acquainted with their names in various languages, he (Selbourne) would have realized that his best choice (“his” means both ‘Jacob’s’ and ‘Selbourne’s’) and would therefore have noted in the “earnest” not *brasile* but a word resembling Malay *sepang* ‘sapan [the tree]’. How thoughtless of Selbourne, therefore, to pick *brasile*, but how fortunate for us.

**B. cammuca** ‘a certain expensive cloth of Asian origin’: 1484.

**C. maomettano** ‘Muslim’: fifteenth century. The appearance of the word in that spelling in the alleged “original manuscript” is doubly suspicious: not only is our earliest evidence for *maomettano*, however spelled, from the fifteenth century but at first the word was spelled *maumetano*. *Maomettano* also being the current spelling of the word in Standard Italian, a latter-day hoaxer unaware of the need to ascertain the earliest known spelling or unable to determine it of the word would naturally use the current one. See the next entry for a similar state of affairs.

**D. rabarbaro** ‘rhubarb’: 1350. The earliest known spelling is *robarbaro*. No later than 1534 the spelling *riobarbaro* came into use. The spelling *rabarbaro*, which, Selbourne alleged, is found in the alleged “original manuscript,” is thus of a piece with *maomettano*.

**E. rabbinico** ‘rabbinic, rabbinical’: 1745.

**F. ricorrenza** ‘recurrence; anniversary; festivity’: 1640.8

If our list consisted of just one such word, we might ascribe to chance the absence of written evidence from the time the alleged “Jacob of Ancona” supposedly wrote his alleged account to the date indicated. For example, we might conclude that “Jacob” indeed existed and indeed used *brasile* in the 1270s but for some reason we have no later evidence for that word until 1555. Or, we could conclude that one or more other people did use the word in writing or print during that interval but either the relevant text or texts have been lost or they exist but no one has uncovered them and studied their vocabulary.

Yet then we find a second such word, a third, a fourth, a fifth, and a sixth. In fact, we have here not just six lexical anticipatory anachronisms (also called *prochronisms*) but also two orthographical ones. More anticipatory anachronisms will be mentioned later in this essay and other critics have noted even more.

Hence one of these possibilities must be right:

**A.** An “original manuscript” exists; it is authentic; and the seeming anticipatory anachronisms are actually authentic antedatings.9 Which is to say that “Jacob” was such an influential writer of Italian that certain of his coinages were adopted by speakers and writers of the language in later centuries. But that is not believable because Selbourne claimed that the alleged “original manuscript” was hidden for some seven hundred years (hence it could not have had any linguistic influence to speak of); because most of the hiatuses are too long (for instance, why would it take almost seven years for someone to adopt the alleged “Jacob”’s coinage *rabbinico*?); and because Jews in thirteenth-century Italy did not know how to write non-Jewish Italian inasmuch as it was not their vernacular and most of them had no reason to learn to read or write it and no interest in doing so.10

**B.** An “original manuscript” exists but in whole or in part it is a post-thirteenth-century fake.
C. No “original manuscript” exists. If so, the “earnest” came not out of any thirteenth-century text but out of Selbourne’s head and he did not bother to make sure that the words and phrases were attested in Opera del Vocabolario Italiano for the 1270s or before. See the eighth epigraph of the present essay.

Of the three possibilities, only the first one favors Selbourne. Like the other critics of Selbourne’s enterprise, I find it impossible to accept. The second possibility is equally unacceptable. The third one is the likeliest because it is supported by the most evidence. Here are three considerations buttressing it (more evidence is presented elsewhere in the present essay and in other critical reviews and letters to the editor):

First consideration. To be taken into account here is the continuum between underdescribed and well-described languages: Standard Italian, for example, is a relatively well-described language because, as a result of the existence of abundant texts and the investigations of many researchers over many years, we now have fairly good descriptions of Standard Italian vocabulary as it has varied in time and in space (in contrast to, say, Judezmo and Yiddish, which are relatively underdescribed languages, especially the first one).

With underdescribed languages, it is statistically likely that dates of earliest known use can be pushed back toward the dates of earliest use if more older texts are uncovered. With well-described languages, that is unlikely. To take a theoretical contrastive example, say that language X originated around the year 1000 and we know of only three texts in that language: one dated 1500, one dated 1700, and one dated 1900, and say that language Y also originated around the year 1000 and we know of 901 texts in that language, namely one for each of the years between 1000 and 1900. Language X is thus the relatively underdescribed language and language Y the relatively well-described one because we have fewer texts in X than in Y on which to base our descriptions of them.

Now, suppose that we find a fourth text in language X and it is dated 1300. It perforce gives us antedatings for every one of the usages it contains (inasmuch as till now the earliest known text in the language was the one dated 1500, so that hitherto the dates of earliest known use were never from before the latter year). And say too that we now find another text in language Y and it is dated 1300. Since we already have 901 texts each dated a year apart between the years 1000 and 1900, the addition of that second text dated 1300 will probably not change the picture much: the second text from that year might give us a few antedatings (say, if the hitherto earliest known use of a certain word was in the manuscript dated 1395 but now we find it in the newly discovered second text from 1300), though probably not many -- in all likelihood not so many as we would find were the language underdescribed and another old manuscript discovered.

Which is to say that we should not expect dramatic antedatings for Standard Italian vocabulary because Standard Italian is by now a relatively well-described language. Yet here, with Selbourne’s “earnest,” we would have dramatic or fairly dramatic antedatings, as we will now see.

To give Selbourne the benefit of the doubt, let us assume, though it is not realistic to do so, that “Jacob” lived to be a hundred and in his hundredth year wrote his account or wrote the version which Selbourne claimed he saw. If so, we would have a span of 255 years (for brasile), 234 years (for rabarbaro), of 445 years (for rabbiniaco), and 340 years (for ricorrenza) -- to say nothing of the two unexpected spellings maomettano and rabarbaro -- between the date of composition of the alleged “original manuscript” and the dates of first known use given in Opera del Vocabolario Italiano or Palazzi et al. 1992.
Moreover, all those alleged antedatings would come in just one small corpus, namely the alleged “earnest.” Since it is reasonable to assume that the percentage of antedatings in the alleged “earnest” is the same as the percentage of antedatings in the alleged “original manuscript” and since the alleged “earnest” is only a tiny part of the alleged “original manuscript” (by Selbourne’s telling, the latter consists of 280 leaves, most of them written on both sides, each leaf measures 25.5 by 19.5 centimeters, and each leaf contains an average of forty-seven lines), the latter must contain an extraordinarily large number of antedatings. Therefore, if we believed Selbourne, we would have to draw the astounding conclusion that “Jacob” was an outstanding Standard Italian neologist -- at a time when the vernaculars of thirteenth-century Italian Jewry were not any variety of non-Jewish Italian but Jewish Italian, Jewish Greek, and possibly other Jewish languages too (Italy north of the Po River eventually came to have many Yiddish-speakers but I do not know whether any were living there in the 1270s).\textsuperscript{11}

Moreover, even if we found not a single anticipatory anachronism in the “earnest” (to say nothing of the myriad other anomalies in Selbourne’s enterprize, only some of which will be detailed in the present essay), we could not believe that a Jew in thirteenth-century Italy was capable of, or interested in, writing 280 leaves (about 13,160 lines) in non-Jewish Italian (to say nothing of Higher Macaronic).

We thus conclude that here we have not legitimate antedatings in an authentic thirteenth-century text but anticipatory anachronisms either in a fake text allegedly from the thirteenth century or in a list of words and phrases that Selbourne made up and was trying to pass off as excerpts from what he alleged was an authentic thirteenth-century text.

Can we believe, for example, that the alleged “Jacob” way back in the 1270s used the adjective \textit{rabbinico}, yet, despite the abundance of available Italian texts dating from that time to 1745, not a single person during that interval of some 445 years used the word in a written or a printed text that has come down to us?

Or, can we believe that \textit{rabbinico} was indeed used in the interval but not a single text containing the word is now extant or is extant but has not yet been analyzed lexically?

I focus on \textit{rabbinico} not only because it is the most dramatic anticipatory lexical anachronism mentioned so far in the present essay (read on for anachronisms that are even more astounding) but also because that Italian word, according to Palazzi et al. 1992, was possibly modeled on the French word \textit{rabbinique}, the earliest known evidence for which is only from the sixteenth century (see note \textsuperscript{8} for the unsurprising revelation that our earliest evidence for the English word \textit{rabbinical} is only from 1622), so that we are asked to believe not only that “Jacob” used that Italian adjective about 445 years before anyone else, so far as we know, used it, but also that whoever coined \textit{rabbinico} in the thirteenth century or before, modeled it on a French word our earliest evidence for which is only from the sixteenth century. Is it not clear that here we have someone writing in our time who, unaware, that those words probably did not exist in the 1270s, put them into the mouth of a non-existent person of the thirteenth century? What would you say if someone claimed that he had seen a holograph letter signed by George Washington with words like \textit{gas guzzler}, \textit{global warming}, \textit{windchill factor}, and \textit{workaholic}?

Selbourne could counter that those believing that \textit{rabbinico} was modeled on \textit{rabbinique} were wrong, so that the lateness of the earliest known use of that French adjective was no hindrance to the existence of the Italian one in the late thirteenth century. If so, we would retort as follows:

\textbf{A.} In the thirteenth century (and probably for several centuries afterwards) the Italian for
'rabbi' was not *rabbino* (a word first attested for 1598) but *rabbi*.

**B.** Consequently, if any Italian adjective existed in the thirteenth century meaning ‘rabbinic, rabbinical’, it would be derived not from *rabbino* but from *rabbi*. Yet the derivation “Italian *rabbi* + Italian -*nico* = Italian *rabbinico*” would be impossible since Italian has no suffix *-nico,” which we would need to account for the /n/ of the adjective (all the nineteen Italian suffixes used to coin adjectives from nouns begin with a vowel: -*ale, -ano, -are, -arino, -asco, -ato, -esco, -ere, -iano, -ico, -igiano, -ino, -istico, -ita, -itano, -ivo, -izio, -oso, and -uto).

**C.** Which is to say that the /n/ of *rabbinico* belongs to the stem of the word (= the noun meaning ‘rabbi’), not to any adjectival suffix.

**D.** Consequently, the base of the adjective must be *rabbino*, not *rabbi*, yet *rabbino* in all likelihood did not exist in the 1270s.

**Second consideration.** We have to take statistical probability into account. Whereas we might entertain the extremely slight possibility of an unusual gap in the citational evidence regarding just one of the words (say, “Jacob” used it, we have no evidence for its use for the next several hundred years, and then it surfaces again, this time in, say, the 1600s or 1700s), it is improbable that such gaps would occur repeatedly (and for *rabbinico* a gap is not even conceivable because the derivation in **B** is impossible).

**Third consideration.** The statistics weigh even more heavily against Selbourne if we take into account the fact that coiners of words can usually make choices. As an example, let us consider the Italian noun and adjective meaning ‘Muslim’.

If the only way which Standard Italian had of coining a noun and an adjective meaning ‘Muslim’ were by taking the Italian name of the founder of Islam and adding to it the Italian adjectival suffix -*ano*, Selbourne could reasonably argue that the alleged thirteenth-century “Jacob” had no choice but to coin or use the form *maomettano* (and that if someone later coined exactly the same form, that would be coincidental).

Yet that could not be the case, for an Italian noun and adjective meaning ‘Muslim’ need not consist of the Italian name of the founder of Islam and the suffix -*ano*. There are in fact other ways in the world’s languages of forming words having that meaning:

**A.** The stem of the noun and adjective could be the Italian name of Hagar (compare obsolete Spanish *agareno* ‘Muslim’, which consists of Spanish *Agar* ‘Hagar’ -- Muslims consider themselves descendents of the Biblical Hagar -- and the ending -*eno*).

**B.** Or the stem could be something other than a personal name (compare English *Muslim*, which derives from an Arabic common noun meaning ‘submitter [to God], surrenderer [to God]’).

**C.** The Italian suffix need not be -*ano* (see above for the eighteen other Italian suffixes appended to nouns to form adjectives).

Consequently, the coiner of an Italian word meaning ‘Muslim’ has several choices; and the more choices there are, the less likely two people -- say, the alleged “Jacob” in the thirteenth century and someone in the fifteenth century -- are to make the same choice, especially if the temporal gap is, as it is here, big.

“Jacob”’s *maomettano* is therefore triply suspect:

**A.** The alleged presence of the word in the alleged “original manuscript” implies a gap in the
B. Choice of the same device (name of the founder of Islam + -ano) twice, two hundred years apart, is unusual.

C. The spelling maomettano is unexpected for the thirteenth century.

Each of those three animadversions is minor (for one reason or another, there could be a gap of some two hundred years; for one reason or another, the alleged “Jacob” could have used a spelling not known to us to have been used again until the fifteenth century; and for one reason or another, two people could have made the same choice independently of each other), but when the three are taken together, we are faced with an accumulation of coincidences (it just so happened that “Jacob” used the same [...]; it just so happened [...]; it just so happened [...]!), the likelihood of which is next to nil, so that the more realistic conclusion to draw is that either Selbourne saw a fake text or he saw no text at all. And, given the pile of other evidence pointing in the same direction, we are all the more certain that our conclusion in the present regard is the right one: those are anticipatory anachronisms which suggest strongly that either no authentic “original manuscript” exists or, more probably, the “original manuscript” is in fact the first draft of an original piece in writing by Selbourne.

* 

Belonging in a list separate from the words just discussed are nine others.

The dates below are from Opera del Vocabolario Italiano or, if that dictionary does not list the word, from Palazzi et al. 1992 (for some of the relevant texts, the exact year of composition is not known):

List 2

A. analo gia: 1363.

B. bucherame: since one text containing the word was written between 1306 and 1325 and another was written between 1311 and 1313, we cannot determine which contains its earliest known use.

C. bussola: one text containing the word was written between 1324 and 1328.

D. cittidanza: 1321.

E. doglianza: since one text containing the word was written in the thirteenth or fourteenth century and another in 1378, we cannot determine which contains the earliest known use of the word.

F. novilunio: one text containing the word was written in 1313 or 1314.

G. ondeggiare: one text containing the word was written around 1341.

H. schiavo: one text containing the word was written before 1294.

I. tegghia: since one text containing the word was written between 1296 and 1305 and another was written between 1293 and 1306, we cannot determine which contains its earliest known use.

Thus, in that list too we have antedatings in the alleged “Jacob”’s alleged “manuscript,” though
less dramatic ones than those of List 1 and some may not be antedatings at all (it depends on how long we allow “Jacob” to have lived). Consequently, they are not strong evidence against the authenticity of the alleged “original manuscript” and maybe not even weak evidence. Still, the dates in List 2 belong to the catalog of anomalies in the alleged “manuscript” and not to any catalog of signs that it exists and is authentic.

*  

Seeking anachronisms in Selbourne’s alleged “translation,” Larner 1997 notes that “Sailing the Adriatic in 1270, the narrator, ‘Jacob of Ancona,’ says how fast his galley sailed - ‘we made wings of our oars’ - thus anticipating one of Dante’s most vivid metaphors (Inferno, xxvi, 125), first written down some forty to fifty years later.”

To Larner’s criticism, Lazenby 1997 responds that “there is absolutely no reason why someone sailing the Adriatic in 1270 should not have used the expression ‘we made wings of our oars.’ ¶ The metaphorical use of ‘wings’ for oars goes back at least as far as Homer, some 2,000 years before Dante. ¶ Teiresias tells Odysseus to find a shipless people who have ‘no oars, which are like wing feathers to ships’ (Odyssey xi).”

Hawkes 1998, likewise responding to Lazenby, calls attention to the phrase *remigio alarum* in Book vi of *The Aeneid*, thereby implying that “Jacob” could have been inspired by Vergil. Hawkes has in mind the line “volat ille per aura magnum remigio alarum” (where *ille* ‘he’ refers to Mercury).

As we will see, Larner’s argument is weak. Consequently, I could have omitted from the present essay all discussion of the sentence ‘we made wings of our oars’ and thereby implied that I agree with it (criticisms leveled against Selbourne in the negative reviews and letters to the editor which I have seen but do not mention in the present essay are in my opinion right), but to show Selbourne and his tiny band of supporters that I try to keep an open mind, do not accept others’ criticism of him just because it is criticism, and prefer to form an opinion with my own lights, I have kept that discussion in.

Here, now, is why Larner’s criticism is weak:

1. Let us first clear away a misconception, which is that since our earliest evidence for the metaphor is in Dante’s writings, he coined it. The first known use of any linguistic item is definitely its first known use, but it may not be its first use because new evidence may be found showing earlier use.12

2. Thus, the fact the first known use of the metaphor is in Dante’s writings does not exclude the possibility that someone else used it at the same time or before.

3. Larner might then argue that the alleged “Jacob”’s use of the metaphor was too dramatic an antedating to be believable. In that case, we would note that because Dante was born in 1265 and ‘Jacob’ allegedly in 1221, a dramatic antedating would be unlikely. Thus, Larner could at most argue the metaphor belongs in List 2.

4. But then we have Lazenby’s and Hawke’s valid remarks.

5. It seems best, therefore, not to make much about the metaphor.13

A. Bamboo

We are asked to believe that “Jacob of Ancona” saw bamboo in China (Selbourne 1997a:293) and that in the supposed manuscript it is called “bambagio” (idem, p. 385). Bambagio belongs in List 1 in section 3 but since it needs somewhat extended treatment, we will deal with it separately.

The earliest European mention of the wood is, so far as we know, in Naturalis Historia, by Pliny the Elder (c. 23-79 CE): “harundini [...] Indicae arborea amplitude” (16.162). Because harundo Indica ‘bamboo’ literally means ‘Indian reed’, we know that Pliny in particular and maybe the ancient Romans in general, if they knew the wood, associated it with India (a point to which we return presently).

From the fact that we have only one attestation for harundo Indica (namely, in that passage of Naturalis Historia) we assume that Latin-speakers had little if any first-hand knowledge of the wood (the very nature of Naturalis Historia, an encyclopedia, was bound to make the work full of references to exotica). And from the fact that words meaning ‘bamboo’ in other languages of Europe are first known to have been used only in the late sixteenth century, which, not by coincidence, fell during the Age of European Discovery, Exploration, and Colonization, we assume that the wood was largely or completely unknown in Europe for some fifteen hundred years after Pliny’s time, until it was (again?) brought to Europe by European explorers returning from their expeditions:

1. Italian bambù ‘bamboo’ is not listed in Opera del vocabolario italiano; according to Palazzi et al. 1992 the word is first known to have been used in 1585.

2. The earliest evidence for English bamboo is dated 1598.

We could go on listing words for ‘bamboo’ in other European languages, but the results are predictable.

3. The earliest evidence for Portuguese mambu (now spelled bambu) is dated 1563.

Consequently, well before and well after the thirteenth century, when Selbourne claimed that the alleged “Jacob of Ancona” lived, bamboo was in all likelihood unknown in Europe, in which case it would have had no name in any European language of the time (people give names only to what is known to them) and the alleged “Jacob of Ancona” would have learned about the wood and learned a name for it only during some leg of his journey to or from China. If so, Selbourne is faced with this contradiction:

A. The family of European-language words meaning ‘bamboo’ that consists of words like Dutch bamboe ~ bamboes, English bamboo, French bambou, High German Bambus, Italian bambù, Polish bambus, Portuguese, bambu, Rumanian bambus, Russian bambuk, and Spanish bambú (thus, including the purported thirteenth-century Italian word *"bambagio” meaning ‘bamboo’) derive in one way or another from the Malay name for the wood, bambu, which in turn derives from the name for the wood in one or more Dravidian languages, possibly Kanarese, which is a language of southwestern India also called Kannada. That etymology is consistent with the literal meaning of Latin harundo Indica, that is, both the Latin name and etymology of the words belonging to the aforementioned family take us back to India.

B. Ponder now the fact that the entire paragraph which you have just read does not mention
China, the Chinese, or the Chinese language. For good reason. Peter K. Bol tells me that none of the Chinese names of bamboo that he can find for any period resembles, even faintly, any of the words belonging to the aforementioned family of Dravidian-origin words. Which is to say that none of the Chinese names resembles *bambagio* either.

C. Selbourne asserted that the alleged “Jacob of Ancona” called bamboo “bambagio” and, according to his alleged “translation” of the alleged “original manuscript,” the alleged “Jacob” first became acquainted with the wood in China. Therein lies the contradiction:

i. If the alleged “Jacob” first became acquainted with that wood in China (as Selbourne claimed), his name for it should be either a Chinese name appropriate for the time and place or places in question or the name should at least resemble such a Chinese name. Which is to say that it should not resemble the names for the word in Malay or in any Dravidian language. But it does resemble the latter. Or,

ii. If the alleged “Jacob”’s name for the wood resembles its names in Malay or in a Dravidian language (as the word *bambagio* in fact does), he should have first become acquainted with the wood not in China but in some place where in his day Malay or a Dravidian language was spoken. But he became acquainted with the wood, so we are told, in China.

One could bend over backwards by trying to resolve the contradiction in this way: yes, “Jacob” first became acquainted with bamboo in China (as Selbourne claimed) and thus learned a Chinese word for it, but on the return trip he stopped off in some place where Malay or a Dravidian language was spoken, became reacquainted with the wood there, and adopted, instead of the Chinese name that he had picked up earlier, the name which he now heard in Malay or some Dravidian language.

However, to conclude that the alleged “original manuscript” exists and is authentic would require us to bend over backwards so many times to accept Selbourne’s numerous far-fetched “explanations” of the myriad anomalies in the enterprize -- “explanations” which are unsupported by any reasonably factual evidence and which give the impression of having being cooked up by someone who was cornered but could not bring himself to admit defeat publicly -- that our spines would soon snap (read on -- and read too the other negative reviews and letters to the editor -- and you will see just how much is wrong with Selbourne’s enterprize). We prefer, therefore, to conclude that the contradiction involving bamboo and “Jacob”’s name for that wood cannot be reasonably resolved, so that here we have further evidence that the alleged “original manuscript” either does not exist or, if it does, is a fake penned much later.

If, however, you are still willing to bend over backwards, read on for evidence against Selbourne concerning *bambagio* that is even more damning.

D. As we have seen, bamboo did not come to the attention of many Europeans a second time (that is, after Pliny’s day) until the late sixteenth century. Consequently, the alleged “Jacob of Ancona” must have been aware that the wood was still unknown in Europe and he should therefore have explained to his readers what bamboo was. Yet examine Selbourne’s alleged “translation” and you will see that the alleged “Jacob of Ancona” mentions bamboo nonchalantly, without any explanation (as nonchalantly as I have used the word *bamboo* in this essay without any definition and without your wondering what I was talking about), so that we must conclude that he assumed that his readers knew the wood. Obviously, a thirteenth-century European would not have made that assumption.

Selbourne might now once again come up with an “explanation” -- if clever enough, anyone can explain away anything (but would it be believable?) -- namely, that the “original manuscript”
indeed describes bamboo but since he (Selbourne) knew that readers of the “translation” would be acquainted with the wood, he omitted it. If that is his “explanation,” I say, with Horace, credat Iudaeus Apella! ‘let Apella the Jew believe it’.

Are you still willing to bend over backwards? If so, read on for evidence that is even more damning.

**E.** As expected, Opera del Vocabolario Italiano, as noted above, does not list bambagio *‘bamboo’*. But it does list that word in the sense of ‘cotton wool [raw cotton]’, its first attestations being from Libro Guelfo (1276-1279). That Italian word, like English bombast, bombax, bombazine, and bombycid, as well as related words in other latter-day languages of Europe (you can pick up the trail by looking up the etymologies of those English words), goes back to a Persian word (compare Pahlavi pambak ‘cotton’).

Therefore, since cotton wool and bamboo are different things, we have here not just an anticipatory anachronism but a semantically misused one. Because people with a good knowledge of Italian would no more confuse the Italian words bambagio ‘cotton wool’ (or, to cite the form of the word in current Standard Italian, bambagia) and bambú ‘bamboo’ than people with a good knowledge of, say, English would confuse the English words bombax and bamboo, the confuser could have been only someone with less than adequate knowledge of Italian. That person could not have been the alleged “Jacob,” for he was an “astonishing intellectual titan” (see note 35) who composed no fewer than 280 leaves in, as Selbourne told us, “basically educated Tuscan.”

Rather, the confuser must have been a primary speaker of some other language whose knowledge of Italian was deficient. Who might that person have been? Of all the people involved in Selbourne’s enterprise, only one is known for certain to be real: he is not the alleged “Jacob of Ancona”; he is not the alleged “present owner” of the alleged “original manuscript”; he is not any of the other characters alleged to have existed; rather, he is Selbourne himself, who is a native speaker of English. As Sherlock Holmes says in one of Conan Doyle’s stories, “When you have eliminated the impossible, whatever remains, however improbable, must be the truth.”

If Selbourne counter-argued that the alleged “original manuscript” indeed has bambagio, the citations in Opera del Vocabolario Italiano show that the word was in use in the 1270s, and he mistranslated it as ‘bamboo’ instead of ‘cotton wool’, our response would be: the meaning ‘cotton wool’ does not make sense in the passages in which the alleged “Jacob” is alleged to have used it.

Thus, so far we have a triple blunder in one word (maomettano) and double blunders in two (rabarbaro and bambagio). See section 7 for another word the presence of which in the alleged “original manuscript” resulted from more than one blunder.

### B. Earrings

The alleged “Jacob” allegedly gave two pearls to one of his servants for use in earrings, but Kay Lacey, a historian of textiles and dress with a special interest in earrings, says that “In the West they are not worn after the Viking period (c AD1000), and only in the first quarter of the 16th century do they appear again. Only ‘moors’ are represented as wearing solitary earrings in the medieval period. ¶ I have seen a pair of 14th-century Indonesian earrings made of gold. I know of none with pearls” (Lacey 1997).

Selbourne responded in this way (not having Selbourne 1997a at the moment, I quote from the
“[…l’un des critiques assura que les «gens du Moyen Âge» n’avaient pas de boucles d’oreille: en conséquence, Jacob n’avait pu, de retour à Ancône, offrir à sa jeune et stoïque servante, Buccazuppo, «deux belles perles pour ses oreilles», comme il dit. Or la pratique juive -- pour ne dire mot de l’émulation liée à ce que les voyageurs avaient vu en Orient -- dément formellement ce «fait». Un passage du Roman de la rose (fin du XIIIe siècle) donne un surcroît de crédit au récit de Jacob: «... Et met en ses deus oreilletes / Deus verges d’or pendans greletes2...» (p. 569, where footnotes 1 and 2 are respectively “Cf. Exode, 32,2: «Aaron leur dit: Arrachez les boucles d’or qui sont aux oreilles de vos femmes, de vos fils et de vos filles, et apportez-les moi»” and “Le Roman de la Rose, éd. D. Poirion, Paris, 1974, p. 553, vers 20977-20978” [ibidem]).

We have to look at artefactual and linguistic evidence, preferably evidence that is as close as possible to the 1270s and to the places which the alleged “Jacob” allegedly visited; and, as we will see in paragraph D, the linguistic evidence should preferably be nonfictional. I leave the gathering of artefactual evidence, which consists of earrings and depictions of earrings, to the relevant researchers, like Lacey (who, the translator’s “l’un” notwithstanding, is a woman).

As for the linguistic evidence, the little that I have been able to gather is unfavorable to Selbourne:

A. Biblical Hebrew. Biblical Hebrew nezem ‘ring’ means specifically ‘nose ring’ in Genesis 24:47, Isaiah 3:21, Ezekiel 16:12, and Proverbs 11:22 and ‘earring’ in Genesis 35:4 and Exodus 32:2. In Judges 8:24-26 and 25, Hosea 2:15, Proverbs 25:12, and Job 42:11, it is unclear whether one or the other is intended (if the word appears there in the singular) or if one, the other, or both are intended (if the word is in the plural). Since nothing in the context or the context allows us to decide which meaning or meanings are intended in those five verses, it is curious that at least the English translation published by The Jewish Publication Society of America in 1945 has ‘earring’ in the first four of the verses. With respect to Hosea 2:15 and Proverbs 25:12, might the translators have thought that nose rings were primitive and un-Jewish and thus decided on ‘earring’? If so, they were not objective.

From Gesenius 1892:DXLII we see that Gesenius would have agreed with The Jewish Publication Society of America regarding Hosea 2:15 (but not Proverbs 25:12). I do not understand their reason for taking nezem to mean specifically ‘earring’ in that verse.

With respect to Judges 8:24 and 25, the suspicion that the translators of The Jewish Publication Society of America chose ‘earring’ because they thought nose rings were primitive and un-Jewish could not be raised because those two verses refer to Ishmaelites’ rings. We wonder, therefore, what the translators’ motive was for choosing ‘earring’ here instead of ‘ring’.

In Job 42:11, the translators of The Jewish Publication Society of America picked ‘ring’. Note that although ‘ring’ is the word to use if neither the context nor the context allows us to choose either ‘earring’ or ‘nose ring’, it is also too broad a translation because English ring can also designate a finger ring, which in Hebrew is called not a nezem but a tabaat.


Therefore, during a certain early period or certain early periods in the history of the Jewish people earrings were worn (and from Exodus 32:2 we know that at least at one time both males and females did). However, extrapolating from just that verse, as Selbourne did, or even from four verses (he should have mentioned Genesis 35:4, Numbers 31:50, and Ezekiel 16:12 also) to something which,
with his characteristic vagueness, he called “la pratique juive” is just another example either of how he
was unaware of the protocols of high-level research (something hard to believe) or of how, if he was
acquainted with the protocols, he did not hesitate to ignore them. Simply put, the Biblical verses tell us
nothing about the 1270s. How would you react to someone who cited notices of slave auctions in pre-
Civil-War American newspapers as evidence that slavery was widespread in the United States today? For
another example of Selbourne’s disregard, apparently deliberate, of the temporal factor, see the
discussion in section 7 of Selbourne’s half-truth that “There are no salt marshes in Fez” and for a
probable example see the Comment on Quotation H in section 20.

As for “ce que les voyageurs avaient vu en Orient,” let Selbourne document with artefactual
and/or linguistic evidence the wearing of earrings in the 1270s in any of the places in which the alleged
“Jacob” allegedly stopped or stayed.

B. Yiddish. Yiddish has two words meaning ‘earring’: oyring is probably the older word,
probably the usual word formerly, and probably the only word used in Bible translations (if used in
recent times in speech or writing other than Bible translations, the word means ‘big earring’) and oyrl
(formally a diminutive) is now the usual word, at least in Eastern Yiddish, in speech and writing other
than Bible translations They are attested only from the nineteenth century though are probably older,
the lateness of the attestations being due presumably to the paucity of early texts and failure to excerpt
all known earlier texts.

C. English. In the second edition of Oxford English Dictionary, the first three citations in the
entry for earring (however spelled) are dated circa 1000, 1468, and 1535. Since all are from translations
of the Jewish Scriptures (see A), they do not attest to the wearing of earrings in English-speaking areas.
The fourth citation, dated 1600, is from Hakluyt’s Collection of the Early Voyages, Travels, and Discoveries of
the English Nation, by Richard Hakluyt, who died in 1616. It refers to someplace outside Europe (I have
been unable to see the collection to determine precisely where). The earliest citation in the dictionary
pertaining to the British Isles could be from 1468 (I cannot tell from the short citation whether
reference is indeed to that area) or before 1643 (again, the geographical reference is not clear from the
citation). See Gold 2005c for an example of how Oxford English Dictionary often fails to give ample
citations, we now having two more.

D. French. The lines “Et met en ses deus oreilletes / Deus verges d’or pendans greletes,”
written by Jean de Meun around 1280, indeed refer to earrings (verges pendans literally means ‘hanging
rods’), but Selbourne’s inability to find more than one attestation suggests that earrings must have been
rare on French-speaking territory at about that time. Furthermore, since Roman de la Rose is fiction, it
could contain any number of references to exotica, so that the passage which Selbourne quoted is not
necessarily evidence for the wearing of earrings on French-speaking territory around 1280 (in
discussions like the present one, we therefore prefer linguistic evidence that is nonfictional). It is not
surprising that Selbourne could find nothing more than fiction -- and then just two lines in merely one
piece of fiction.

E. Italian. All of the foregoing about Hebrew, Yiddish, English, and French is important for a
comprehensive history of earrings (the literature must be vast), but it does not help us to decide
whether Lacey or Selbourne is right. Italian is more pertinent. The earliest known form of Italian
orecchino ‘earring’ was origino, which Opera del Vocabolario Italiano does not list. According to Palazzi et al.
1992, the date of the first known use of the word (still in the form origino) is 1475. If earrings were worn
in Italy during the previous two hundred years, we would probably have linguistic evidence thereof, but
we do not. We thus now have another anticipatory anachronism, this one being of about two hundred
F. Judezmo. At least Southeastern Judezmo (the Judezmo of the Ottoman Empire and its successor states) has two words for ‘earring’, one being of immediate Greek origin (which is therefore a post-1492 addition to the Judezmo lexicon) and the other, ore_ál ~ uri_ál (phonological variants of the same word), being of Old Spanish origin (and thus a pre-1492 addition).

The evidence adduced so far favors Lacey. Among that evidence is her negative evidence: her finding no artefactual or linguistic evidence of earrings in Europe between around 1000 and the first quarter of the sixteenth century and her finding no evidence for pearl earrings. In light of the Italian and Judezmo linguistic evidence, the second half of the fifteenth century seems to be a better terminus ad quem than the first quarter of the sixteenth century.

The status of verses 20977-20978 in Le Roman de la rose is still unclear: are they evidence for the wearing of earrings on French-speaking territory around 1280?

To Lacey’s argument may be added two more:

A. For the thirteenth century, when social hierarchy was rigid, we do not expect a well-to-do merchant-rabbi-physician (Selbourne claimed that the alleged “Jacob” was all three) to give a mere servant an expensive gift like two pearls inasmuch as largesse, unless some extraordinarily heroic deed was being rewarded (which is not the case here), would have been perceived as a sign that the gifter, of much higher social standing, was demeaning himself by considering the giftee, of much lower social standing, to be on a higher level than she actually was (it was not an egalitarian age). The gifter would have thus been humiliated in the eyes of his social equals or superiors. Here, however, Selbourne had an answer: after allegedly giving his servant the pearls, the alleged “Jacob” says “Dieu me pardonne mon péché” (page 519 of the French version), on which Selbourne commented: “Le «peche», je suppose, était de faire un cadeau personnel à sa servante” (ibidem). Let us therefore consider additional argument A to be a quibble (see note 157).

B. “Jacob” gave his servant just the pearls: “[Je donnai] à Buccazupo encore deux belles perles pour ses oreilles” (page 519 of the French version). She would thus have had to have them set, but it is hard to believe that a servant would have been able to permit herself that luxury. However, that too may be just a quibble (see note 157).

C. Saffron

“Jacob frequently purchases saffron, not known in China at the time, and seldom used even today. Or is this another ‘first’ for Jacob of Ancona?” (Wood 1997). Saffron “is supposed to have been introduced into Cathay by the Mongol invasion” (The New Encyclopædia Britannica, 15 ed., 1991, vol. 10, p. 297). If the encyclopedia is right (is it?), Wood’s criticism could still be valid: the Mongol invasion of China began in the north; the invaders reached Quanzhou after the alleged “Jacob”’s alleged departure from China; so that it would be unlikely that he found any saffron there. At the very least, therefore, the criticism should read “Jacob frequently purchases saffron, presumably not known in Quanzhou at the time, and seldom used even today in China. Or is this another ‘first’ for Jacob of Ancona?”

In any event, we must look at how the alleged “Jacob” mentions saffron:

“Des épices, ils n’avaient pas encore commencé à en acheter, car je leur avais demandé de rechercher la meilleure qualité de sucre, de safran, de gingembre, de galangal, de canéfice et de camphre,
mais aussi d’indigo et d’alun [...]” (page 218 of the French version, which I cite because the English versions are at the moment unavailable to me).

Thus, he does not define the word safran and from the absence of any explanation of what saffron is we infer that he assumed his readers were familiar with the spice. That would indeed have been possible for the 1270s because, although the earliest citation for the Italian word zafferano ‘saffron’ in Opera del Vocabolario Italiano is only from 1387, we have reason to believe that it goes back to the twelfth or thirteenth century, as we will now see.

Oxford English Dictionary has a citation for the English word saffron ‘saffron’ which it dates to “c 1200” and the dictionary notes that the French word safran ‘saffron’ is attested for the twelfth century. The family of words meaning ‘saffron’ in European languages some of whose members are Catalan safrà, English saffron, French safran, Italian zafferano, and Yiddish zafren derives from the Arabic for ‘saffron’, za`far_n -- which is not surprising given the fact that in the Middle Ages the European spice trade with Asia could not bypass Arabic-speaking intermediaries because of Arab control of western Asia.

Bearing in mind the location of English-, French-, and Italian-speaking territories and of Asia relative to one another, we conclude that if English had the word saffron (however spelled) by no later than around 1200, French must have had the word safran (however spelled) earlier (indeed, the earliest evidence for the latter word is from the twelfth-century), and Italian must have had the word zafferano (however spelled) even earlier.

If that reasoning is right, the absence of evidence for zafferano from the 1270s or earlier is no proof that the alleged “original manuscript,” if it exists, mentions saffron.

Willard 2001 seems to contain nothing relevant to the alleged “Jacob”’s alleged encounter with saffron, but from Humphries 1998 we learn that “Tumeric (curcuma longa) is an oriental spice, ground from a root similar to ginger. Marco Polo encountered it in 1280, describing it as, ‘a fruit that resembles saffron, though it is nothing of the kind.’ Tumeric was substituted for real saffron and was traded widely in Europe by the Venetian spice merchants” (p. 31). If, then, Marco Polo used the Italian word for ‘saffron’, we wonder why Opera del Vocabolario Italiano did not quote him. Might it be because it is hard to distinguish what he wrote and what editors interpolated later?

It would be good to know how Wood reacts to the mention of saffron in Polo’s account of his trip. See note 13.

D. Ginger

The passage quoted above from the French version of Selbourne’s book also mentions ginger (= French gingembre). The Italian name of that spice is zenzero, which Opera del Vocabolario Italiano does not list. According to Palazzi et al. 1992, the date of the earliest known use of the Italian word is “a. 1698,” that is, ‘before 1698’. When dating the first known use of an item in that way (several more examples appear earlier in this section), lexicologists mean that they have evidence of some kind that the text was not written on or after the date indicated (hypothetical example: the text is a letter written by someone who died on 1 January 1698). Yet by the same token, the text cannot for one reason or another be much earlier than the date indicated, for instance, if a letter was written by someone who died on 1 January 1698, it cannot date to, say, 1550; or, if the text is in a nineteenth-century hand, it
cannot date to the sixteenth-century; or, if the text refers to the Thirty-Years War as being recent, it cannot date to the ninth century. For an actual example, see the discussion in section 6 of Hebrew-Aramaic sin ‘China’, which, we are certain, arose before the death of Yehuda Halevi.

If we suppose, for the sake of argument, that ginger was known in Italy in the alleged “Jacob”’s day, it is curious that the spice is mentioned in no Italian texts known to us until one dated somewhat before 1698. Consequently, we seem to have here another dramatic anticipatory anachronism, but cannot be certain because Selbourne did not say that zenzero or some other form of that word appears in the alleged “original manuscript.” Indeed, since Kalmens 1943:295 cites a Yiddish text written in Austria in 1307/1308 that speaks of “ingber vos ba zey heyst binstanaro,” that is, ‘ginger, which they call binstanaro’ (‘they’ being Jews in one or more unspecified Mediterranean countries), it is not impossible that binstanaro is (solely older?) Jewish Italian for ‘ginger’, so that Selbourne could argue that the absence of evidence for zenzero much before 1698 does not prove the inauthenticity of the alleged “Jacob”’s mention of that spice -- but, we would retort, Selbourne would be right only if the alleged “original manuscript” had a word other than zenzero or a variant thereof. It would be good to know, therefore, what the alleged “original manuscript” has.

5. A Latin anticipatory anachronism in the alleged “original manuscript” (domini canes)

Selbourne translated a passage in the alleged “original manuscript” as “Thus my bitterness, alas, increased at the thought of our griefs, may the Lord keep us in His hand, so that I was called by him to speak with His breath: ‘O hypocrisy and falsehood, O cruelty and folly, o blasphemous worship of idols and the spilling of blood, your names are for ever Christian. Even here your priests come and go as vagabonds, preaching hatred against us so that there might be no place in the world where we are safe. Truly the Dominicans are the dogs of god, for so they justly call themselves, being of that species of rabid cur which bares its teeth and bites to kill the stranger” (Selbourne 1997a:247 and 2000:300-301). Never mind the boring contents and tedious style -- if you want more, the entire “translation” is in rebarbative Wardour Street English (for more mentions of that style, see notes 43 and 81, section 15, note 94, sections 16 and 17, subsection A of section 17, notes 117, 124 and 129, subsection G of section 22, and notes 157 and 162). Consider, rather, the three features of non-Jewish Western style:


2. apostrophe of a personified abstract quality (whether human or not), a literary device going back to Classical Latin, if not Classical Greek, literature. For instance, in Epitome bellarum omnium annorum DCC, the author, whose name has possibly not come down to us with full accuracy (*Lucius Annius Florus? *Lucius Annaeus Florus?, *Publius Annius Florus? *Iulius Florus?), writes, “Quamvis tum, o pudor, manu servili pugnaret.” Or, to take an example from English, Milton apostrophizes a personified abstract quality in the lines mentioned in the previous paragraph. Or, to take a recent example, in one of his columns on correct French in Le Monde in the mid 1960s, Robert Le Bidois wrote “O pureté de la langue française, où est-tu?” ‘O, purity of the French language, where art thou?’

3. ending the apostrophe with the construction ‘thy name is [...]’, our earliest evidence for which is in Shakespeare’s Hamlet: “Frailty, thy name is woman” (act 1, scene 2, line 146). Since my
attempt to find idiomatic use of ‘thy name is [...]’ in earlier English and in other languages yielded nothing, we assume that Shakespeare may have coined it (see note 13) and that its use in later English is the result of his influence.

How curious that the alleged “Jacob” wrote not like a thirteenth-century Italian Jew but like someone who lived in the non-Jewish British world of, say, the twentieth century.

Or will Selbourne tell us that Shakespeare got the idea for the idiom the name is [...] from the fictitious “Jacob”?

Or will Selbourne tell us that “O hypocrisy and falsehood, O cruelty and folly, o blasphemous worship of idols and the spilling of blood, your names are for ever Christian” is his idiomatic rather than literal “translation,” so that we should not infer that the “original manuscript” contains an example of prosopoeia of an abstract quality or the conclusion of an apostrophe with a construction literally translating as ‘thy name is [...]’? If so, what does the “original” have?

Curiously, Oxford English Dictionary does not enter the idiom thy name is [...]. It would be interesting to see what citations its compilers could find and whether any predate Hamlet. It would also be good to see how translation of that play render the idiom.

See note 95 and the last paragraph of section 17.

*  

Attached to the above-quoted passage is this remark in Selbourne’s “critical apparatus” (the goal of which seemed to be to give his enterprise a veneer of scholarliness and hence authoritative): “Latin in MS.: domini canes, a play on words of great mordancy” (Selbourne 1997a:384 and 2000:514). As we will now see, by telling us that the “original manuscript” has domini canes, he put more ammunition into the hands of his critics, the Scourges of Jacob, a hardy band of knowledgeable, hard-nosed skeptics known in Greek as 'Iakobomastiges (the singular is Iakobomastix), in Latin as Iacobomastiges (the singular is Iacobomastix), and in Higher Macaronic as “academic peashooters” (see note 157).

The official name of the order of mendicant friars founded by Dominic in 1215 translates as ‘Orders of Friars Preachers’. Informally, its members are known as ‘Dominicans’, which alludes to him. The play on words (domini canes ‘the dogs of the Lord, the hounds of the Lord’, but not, pace Selbourne, *‘the dogs of god’ or, as I have seen elsewhere, *‘the Hounds of God’, which would be translations of *dei canes) alludes to the fact that the members of the order saw themselves as the watchdogs of orthodoxy in the Roman Catholic Church. Thus, they were ruthless opponents of other religions (naturally, they were rabid Jew-haters) and hunters of heretics. Verily, the dogs or the hounds of the Lord.

So far as I can tell, our earliest evidence for that example of prosoponomasia is from late in 1365 and it is not linguistic but artefactual, to be specific, pictorial: what is now the chapel of Santa Maria Novella (the Dominican church in Florence) was erected around 1350 as the chapter house; late in 1365, Andrea da Firenze was commissioned to paint floor-to-ceiling frescos on its walls; and there he depicted the Dominicans as the Hounds of God rending the wolves of heresy.19

How curious that a Jew, how curious that a person who was not only a Jew but also a rabbi, how curious that a person who was not just a rabbi but a rabbi of “noble rabbinical lineage” (see part A of section 8), how curious that someone who was not just a rabbi of “noble rabbinical lineage” but also the grandson of “the great rabbi Israel of Florence” (ibidem) would have learned not only Latin but
specifically Church Latin -- and learned it so well that not only could he read Thomas Aquinas but he was also acquainted with a play on words about the Dominicans which in Christian realia that have come down to us is not recorded until 1365, thus, about a hundred years later.

The fact that the earliest known evidence for the pun is pictorial gives us a clue about how the pun may have arisen (the word may is important because we cannot be sure): the pun could have occurred to Andrea da Firenze; if so, he gave it pictorial form; and the frescos popularized the pun. If so, the pun dates to 1365, in which case, we have here another dramatic anticipatory anachronism. In any case, the fact remains that the earliest evidence we have for the pun from a reliable source is 1365 and we cannot believe that the earliest attestation is actually in an alleged but never seen Jewish text of the 1270s.

1. "It is hard to forget Ord-Hume's own encounter in 1958 with a touching elderly Londoner who had made in his little flat a wonderful new aircraft engine that ran reliably at high power without any fuel. The author's natural question was might he see it? The polite reply was even more reasonable. Of course, it had been working just out there in the hall for weeks, but the inventor could not make it stop, and so the other tenants had insisted that he disassemble it completely to end the tiresome noise!" (Morrison 1977:30-31, reviewing Ord-Hume 1977).

2. Northeast University Press and the American branch of Little, Brown and Company also considered publishing editions of Selbourne's book but dropped the idea after getting negative reports from relevant researchers.

3. Quinn 1997 quotes Sarah Crichton, of Little, Brown and Company, as saying that the London branch of the firm would publish Selbourne's book because "They're content with the authentication they've received," though "they" have never revealed the name of the alleged "authenticator" or "authenticators," said person's or persons' relevant credentials, the basis of the alleged "authentication" (a scientific examination of the paper, ink, and handwriting of the alleged "original manuscript"? something else?), or the text of the alleged "authentication" (how reminiscent of the unnamed, unseen, imaginary characters in the rest of Selbourne's enterprise) or explained how an "authentication" is possible in the absence -- Selbourne's critics say, presumed non-existence -- of the alleged "original manuscript."

Since common sense tells us that an alleged document which has not been produced cannot be certified as authentic (or inauthentic) until its existence has been proven, we assume that the alleged "authentication" is as non-existent as the alleged "original manuscript."

The present essay gives more examples of how certain commercial publishers do not care as much as responsible publishers do about the accuracy of their allegedly nonfictional books. See too Krug 2005.

For the sake of the history of the controversy over Selbourne's book, it would be good to make public the names of the alleged authenticators of the alleged manuscript he alleged he translated, their relevant credentials, and the text of their authentication (if they said what they said privately, they should be willing to say so in public too). It would also be good to know whether their authentication is based on an examination of the alleged "original manuscript" and whether they still stand by him. Since not one has so far come forward, whether to support the book or to dismiss it, we wonder whether they exist. In any case, after reading all the negative reviews and letters to the editor, including the present essay, you will understand that any "authentication" could come only from the pen of an incompetent.

4. Carvajal 1997 reports the critics [actually, just the early ones] to be an unnamed professor of Chinese history at the University of London (presumably Timothy H. Barrett, of the Department of the Study of Religions at the School of Oriental and African Studies of the University of London), Benjamin Braude (a professor of history at Boston College), Bernard Wasserstein (then president of the Oxford Centre for Hebrew and Jewish Studies), David Wasserstein (professor of Middle East and African History at Tel-Aviv University), and a reviewer, whom she did not name, in The Times of London (reference is presumably to Abulafia 1997c).

In Chang 1997 we read that other critics are Peter K. Bol (professor of Chinese history and chairman of the
Department of East Asian Languages and Civilizations at Harvard University), Patrick Geary (of the Department of History of the University of California at Los Angeles), and Robert Hymes (of the Department of East Asian Languages and Cultures at Columbia University). After hearing their opinions, the American branch of Little, Brown and Company cancelled its planned version of The City of Light (for details see Quinn 1997 and Smith 1997).

Peter K. Bol tells me that another critic is So Kee-long (of the Chinese University of Hong Kong). Still others are Robert Bonfil (professor of Italian Jewish history at the Hebrew University of Jerusalem), who writes me that "When asked about Selbourne's book, I too expressed the view that it must be a fake" (letter, 5 October 1999), and Jonathan Chaves (professor of Chinese Studies in the Department of East Asian Languages and Literatures at The George Washington University), who wrote me that "it seems clear that this is a hoax" (letter, 27 May 2006).

Selbourne claimed that "cabal-like pressures, originating in American academia, have been exerted against Little, Brown in America, the holders of the subsidiary rights in my translation" (Selbourne 1997e). By "cabal" we are presumably to understand the secret artifices or machinations of a few people united in a close design, but nothing illegal or unethical happened here, for it is legitimate that right-minded people oppose the publication of something which they believe to be an insult to the truth and an affront to research, well-known examples being condemnation of Protocols of the Learned Elders of Zion and of writings of Holocaust-deniers. As for being united in a close design, it is likewise legitimate for such opposition to be either individual or joint. In the instant case, only the Wasserstein brothers (see Wasserstein and Wasserstein 1997) and only Igor de Rachewiltz and Donald D. Leslie (see de Rachewiltz and Leslie 1998) worked in tandem, but even if all the negative reviewers and writers of negative letters to the editor had made a concerted effort against Selbourne, it would have been no less ethical and no less legal than Selbourne's to make an effort with others to defend himself, as he indeed did with Tudor Parfitt (see section 7), a few people in China (see section 20), and possibly others (Robyn Davidson and Melanie Phillips?).

5. Peter K. Bol tells me that a large number of people in the People's Republic of China hold that anything positive you say about them or where they live, whether right or wrong, is worthy of praise.

6. Among the many criticisms to which Selbourne never responded is this one: "We have left till last the most amazing, even miraculous, of all Jacob's suggestions. All modern scholars, Chinese and Western, agree that Zaitun (of the Arabs and Marco Polo) corresponds to Ch'üan-chou, not to Chang-chou further south on the Fukien coast, as G. Phillips erroneously believed at the end of last century. See T'oung Pao 1:1890, p. 231, where he wrote: 'These readings of Zaitun most undoubtedly point to the port of Gay-cong, which is the local pronunciation of Yüeh-kiang, the port of Changchau [i.e., Chang-chou.]

The name of Quanzhou is still causing Westerners a problem: "An art review in Weekend on Friday about 'Cai Guo-Qiang: I Want to Believe' [...] misidentified the southern Chinese city where Mr. Cai was born and raised. It is Quanzhou not Guangzhou" (The New York Times, 26 February 2008, p. A2).

7. The earliest citation for a lexical item in Opera del Vocabolario Italiano is the earliest known use. Similarly, the dates in Palazzi et al. 1992 are those of earliest known use. If earlier evidence is discovered, the dates have to be changed accordingly. Which is to say that dates of first known use are not necessarily dates of first use.

The foregoing notwithstanding, at times we do know for certain that a lexical item could not have been used before a certain date (see section 7). Gold 2005a and in preparation 1 deal with those matters in some detail.

8. Not surprisingly, the dates of the earliest known use of the corresponding English words, if such exist, are in most cases not much different from those of the Italian ones: 1390 (rhubarb), 1559 (brazilwood), 1612 (rabbinic), 1622 (rabbinical), 1646 (recurrence), and 1681 (Mohammedan).

9. An antedating is a citation for a linguistic usage older than the hitherto earliest known citation, say, if the earliest evidence for a lexical item is dated 1920 and tomorrow someone finds evidence dated 1905, the new evidence is an antedating of fifteen years.

Antedatings are dramatic in proportion to the temporal distance between the hitherto earliest known attestation and the new attestation. For example, till now the earliest known evidence for English surprise party was from 9 August 1858,
but here is evidence, presented below for the first time, from shortly after 16 September 1776 (the following passage is quoted from Bolton 1924:258, where part of the Battle of Harlem Heights, which took place on 16 September 1776, is described):

In our general description of attack on Mount Washington, brief reference only has been made to that part of the assault upon the Heights, which was in the British accounts described as the Highlanders' "surprise party."

At first one might think that here we have party in its military sense (as in raiding party and search party). However, surprise party refers in that passage not to a group of soldiers but to an attack. It thus refers to an event. Consequently, this is not surprise party in its literal sense of 'party or celebration planned for someone without that person's knowledge and intended as a surprize for that person in order to heighten the jubilation' but in a metaphorical, ironical sense. Since literal senses are perforce older than metaphorical or transferred ones, surprise party, at least in its literal sense, must be older than the British accounts to which Bolton alluded, those accounts, naturally, not being older than the battle (as one might suppose, they were in fact written shortly after it). Consequently, the literal sense of surprise party must predate the battle, so that we now have an antedating of at least eighty-four years, which for earlier American English is fairly dramatic.

Here is an even more dramatic English antedating, namely for the noun merchandising ~ merchandizing, the earliest known evidence for which was hitherto dated 1932: "I dwell in a neat and cleanly City, among very civil People, have a due Freedom, am indifferentely well supplied, gain knowledge in Arithmetick and Book-keeping, am in a way of attaining the methods of Merchandizing, and Correspondence in sundry parts of the World, and many more Advantages [...]" (Harrison 1730:100).

In contrast, if according to our present state of knowledge the earliest known use of a certain word was in 1583 (to take a hypothetical example) and tomorrow someone finds a text dated 1582 that contains the word, the antedating is of just one year and thus not dramatic.

10. Selbourne claimed that the alleged "original manuscript" is written in "the vernacular Italian of the thirteenth and perhaps fourteenth centuries; it is basically educated Tuscan -- Jacob tells us that his grandfather was a Florentine rabbi -- but with some Venetian words and spellings, and occasional phrases which can be identified as the mediaeval Jewish dialect of Ancona" (Selbourne 2000:8). Later we learn that it also contains Arabic, Chinese, Greek, Hebrew, Latin, and Persian. The Wassersteins succinctly call the farrago "higher macaronic" (Wasserstein and Wasserstein 1997). To underscore its uniqueness, I propose capitalization: Higher Macaronic.

See section 8 for an analysis of the sentence "it is basically educated Tuscan -- Jacob tells us that his grandfather was a Florentine rabbi."

Since the imaginary "Jacob" showed familiarity with the writings of Thomas Aquinas (see section 10), readers might wonder how a thirteenth-century Jew came to know Latin (the language in which Aquinas wrote exclusively and the only one in which his writings were accessible in the thirteenth century and long afterward). My guess is that the goal of Selbourne's claim that the alleged "original manuscript" contains Latin was to suggest to us that yes, "Jacob" did know the language, so that we should not wonder how he could have read Aquinas. In contrast, I am at a loss to explain the presence of Venetian in Higher Macaronic.

As a student of Jewish Italian, I am intrigued by the wording "occasional phrases which can be identified as the mediaeval Jewish dialect of Ancona," a wording which could come only from the pen of someone able to differentiate between (i) Anconitan Jewish Italian and Anconitan non-Jewish Italian, between (ii) Anconitan and non-Anconitan Jewish Italian, and between (iii) medieval and nonmedieval Anconitan Jewish Italian. Since we seem to have not even short samples of medieval Anconitan Jewish Italian (in any known, reliable text, that is), we appear to have no guidelines for making those three distinctions (see the end of section 13 for some details). With astonishment and with admiration, therefore, I congratulate Selbourne for his achievement, but my admiration is conditioned on the fulfillment of one small request: could we get a list of those "occasional phrases" (in the spellings in which they appear in the "original manuscript") and see the step-by-step analysis that led him to conclude that the material was indeed in the "mediaeval Jewish dialect of Ancona"?

The Wassersteins too are astonished: "He does not tell us how he has performed this remarkable prestidigitational feat of identification [of medieval Anconitan Jewish Italian (D.L.G.)] - since the nature of this dialect, and even its existence,
remain unestablished by scholars" (1997:16). The existence of Anconitan Italian, pace the Wassersteins, is not in doubt. What we do not know is whether its existence stretches back to the 1270s: one possibility is that during that decade the Jews of Ancona were speaking not Jewish Italian but Yevanic (the end of section 13 for some details); another possibility is that all the Anconitan Jews at the time were speaking only Jewish Italian; and a third possibility is that they were then using both languages (the only point not in doubt being that their language of prestige was Hebrew-Aramaic, which at that place at that time was not a spoken language). So far as I can tell, no extant documents known to us allow us to decide what the vernacular or vernaculars of Anconitan Jewry were at the time. Consequently, it is amazing that someone like Selbourne, who did not have the benefit of any training in linguistics in general, any training in Jewish linguistics more particularly, or any training in Jewish Italian or Yevanic most particularly, succeeded in making that determination. Naturally, he gave no examples of "mediaeval Jewish dialect of Ancona," to say nothing of a complete list, with a detailed analysis, of the items in question. More than ten years after making his astounding announcement, he is presumably now readying his analysis for publication. We cannot wait to see it.

We will again mention Anconian Jewish Italian in sections 6, 8, 11, and 15.

11. It is not without significance that Jews did not begin to contribute to Italian literature until the twentieth century, the chief contributors at that time being Italo Svevo (1861-1928; his mother was not a Jew), Carlo Levi (1902-1975), Alberto Moravia (born 1907), Giorgio Bassani (born 1916), Natalia Ginzburg (born 1916), Elsa Morante (1918-1985), and Primo Levi (1919-1987).

12. "[...]

The myth about Shakespeare's neology is a favorite of teachers of English: "Re 'Spelling Champion Crowned' (news item, June 2): ¶ It was a sad spectacle to watch eager children reciting the correct spelling for words that are ridiculously arcane and utterly useless in the course of human events and often purloined from other languages far afield from English. ¶ Much more edifying would be a contest to make up new words undreamed of by dictionaries and of real use. ¶ Look at what William Shakespeare, the champion neologist of all time, was able to accomplish. Just from the first three letters of the alphabet, he came up with the first known use of the following words: accused, arouse, assassination, bandit, barefaced, bedroom, besmirch, bloodstained, cater, champion, circumstantial, courtship, countless and critic -- just to name some of his inventions. ¶ Shakespeare didn't care about his 'Ursprache'-- and more power to him" (Gary Schmidgall, "professor of English at Hunter College, CUNY," letter to the editor, The New York Times, 6 June 2006, p. A20).

The letter elicits these comments:

A. Words like ridiculously and utterly are dead wood, which does not strengthen an argument.

B. Who is to say which words are arcane and useless?

C. Synchronically, *ursprache* and *weltschmerz* are English words.

D. The word *purloined* not being objective, replace it by *borrowed*.

E. How is distance (compare "far afield") to be measured? Physically? Culturally? According to the genetic relationship (or absence of such) of the lending language and English?

F. Why should distance be a criterion anyway? The source of a word, the "distance" of the source, and how the word arose are no measures of its usefulness.

G. If we did eliminate "purloined" words, a large number of those which Schmidgall used in his letter and a large
number of those he was sure were Shakespeare's coinages would have to be cast out. To take merely the first sentence of just his letter: *spectacle, recite, correct, ridiculous, arcane, use, course, human, event, purloin, and language*. It would be interesting to see how the blanks could be filled in with "pure" English words: "It was a sad... to watch eager children... the... spelling for words that are ...ly ... and utterly ...less in the... of ... ...s and often ...ed from other ...s far afield from English."

H. As for "a contest to make up new words undreamed of by dictionaries and of real use," *The Atlantic Monthly* has had a column on language for years, written by Barbara Wallraff, that is sometimes called "Word Court" and sometimes "Word Fugitives." The instalments headed "Word Court" are always useful. For instance, the one for September 2005 quotes a reader who asked, "People often say *all told* when referring to the sum of damage, ills, members of an audience, etc. Wouldn't the correct spelling be *all talled?*," and Wallraff aptly responded, "*All talled* probably does make more intuitive sense nowadays--but with so-called set phrases that's scarcely ever the point. The *Oxford English Dictionary* makes clear that one meaning of *tell* is, or used to be, roughly, 'count.' [...] It's from this sense of *tell* that we get the expression *all told.*" To her apposite reply this may be added: *tell *'count' is still found in the set phrase *tell one's beads* and with a direct object referring to votes or animals, as in "we are telling the votes" and "the shepherd will tell her sheep" (in the latter function, the verb may take the form *tell over*: "we are telling over the votes" and "the shepherd will tell over her sheep"). *Tell out* 'count out' *is* still used when reference is to counting out a player in a game ("Two players were told out"). And we have the agentive noun *teller*, as in "*They* work as *tellers* in a local bank" and "*Five* tellers were appointed to count the votes."

In the instalments headed "Word Fugitives," readers suggest neologisms to fill unusual lexical gaps. For example, in the issue of March 2005, the column minutes suggestions about what to call 'someone who, in looking up a word in the dictionary, is compelled to look across the page for another, equally interesting entry' and what to call 'saying in an e-mail that a document or file is attached and then sending the message before remembering to attach the file'. The neologisms proposed by readers and published in "Word Fugitives" are often almost cuter or too clever by half (the neologizers try to be witty) and almost always are stumper compounds (an overused device in latter-day middle-brow and high-brow coinages); they have little or no chance of catching on (of all the hundreds proposed in "Word Fugitives" I cannot remember one which has gained acceptance); and the column, therefore, is of no social consequence and hence a waste of space.

Therefore, rather than hold the contest which Schmidgall proposed, it would be better to require that teachers of English take at least a few courses in linguistics. And "Word Fugitives" should be replaced by *Expand Your Vocabulary."

I. Even if the earliest known use of any word is in Shakespeare's works, that does not necessary mean that he coined it.

J. Shakespeare "the champion neologist of all time"? Just in English or in any languages? Either way, it would be good to see the proof.

K. After *accused*, add "(the noun)"; after *bandit*, add "(possibly)"; and after *champion*, add "(the verb)."

L. In sum, Schmidgall's letter contains nothing right.

The same issue of *The New York Times* also carries a letter to the editor from Alfred S. Posamentier ("dean of the school of Education, City College, CUNY"), in reaction to the same recently held spelling bee, also with mistakes about English, also such that he would not have made had he had some linguistics under his belt.

*The New York Times* has for over a hundred years claimed to print "all the news that's fit to print." Sometimes, it prints letters that should never see the light of day.

13. When Samuel Johnson was taken to task for criticizing William Shakespeare, he said, "We must confess the faults of our favourite, to gain credit to our praise of his excellences. He that claims either for himself or for another the honours of perfection, will surely injure the reputation which he designs to assist." In the present essay, I have taken the converse approach: pointing out any criticisms of Selbourne that are unfounded in order to show that I strive to be objective (see too section 4 on how I strengthen Selbourne's argument about earrings, though in vain, by pointing out that he should have cited four Biblical verses instead of just one). By the same token, being trained in the law and admitted to the bar, Selbourne will understand that the prosecution's arguments need not be flawless for its case to be just, it being the preponderance of the evidence that counts:

"The standard of proof given by the federal law defining research misconduct is 'preponderance of evidence'--the evidence that has the greater weight or produces the stronger impression in the minds of triers of fact. In other words, had Churchill been federally-funded, the hearing examiner would ask: Is it more likely that Churchill has deliberately falsified his sources or fabricated evidence, or is it more likely that Churchill's misrepresentations were accidental or otherwise
justifiable? This legal standard of proof does not require one side of the case to conclusively defeat the other side. Instead, it requires only that the triers of fact determine which case is stronger. Keeping this standard of evidence in mind, the triers of fact might ask: Are there any plausible defenses to the charges outlined in the essay above? And if so, what evidence would Churchill need to bring to overcome the evidence arrayed against him? (Brown 2006:123).

Selbourne must also be defended against this charge: "The Jewish townspeople in the Muslim port of 'Cormosa' are described as paying a tax 'which in Saracen they call kharadj'. In fact, Jews and the Christians under Muslim rule would have paid a poll-tax called jizya. Kharaj was a rural land tax" (Irwin 1997). At first, that Arabic word designated a capitation tax paid by unbelievers; later, a tax paid on landed property. Irwin was thus unaware of the earlier meaning and Selbourne cannot be faulted here for using the word kharadj (though see section 7 on the probability that Hormuz [= "Cormosa"] had no Jews in the 1270s).

Whether certain other charges against Selbourne are justified is not clear (see the second and third paragraphs of section 13 on whether one of the Wassersteins' criticisms is valid and subsection C of section 4 on whether one of Wood's is).

14. So far as I can tell, in ancient times only two attempts were made in Europe and China to establish contact, one unsuccessful and the other not: "In AD 97 Chinese envoys were frustrated in an attempt to visit the western part of the world, but a mission from Rome reached China by ship in 166" (Dull and editor 1991:80).

15. For example, the Chinese for 'Bamboo Annals' -- Chinese annals written during the Chou dynasty (770-221 BCE) -- is Chiu-shu Chi-men (Wade-Giles) ~ Zhushu Jinian (Pinyin).

16. See subparagraph i of paragraph A of note 157. The few times that Selbourne tried to defend himself (mostly, he ignored the criticism), he made brief general statements, consisting of nothing substantial or verifiable, often including irrelevancies, "evidence" that was not evidence, or "proof" that was not proof, so that either we had to continue taking his claims on faith (only the gullible would do so) or we had to conclude that he was bluffing (paper will take any ink you put on it) and thus in fact had no evidence or proof acceptable in a court of law or to the most stringent researchers. His "defense" was on the same low level as Wang Lianmao's (see section 20).

17. Even though the citations for bambagio in Opera del Vocabolario Italiano do not define the word, it takes only an elementary knowledge of Italian to know that it does not mean *'bamboo'. Consider, for instance, this one:

"Bambagio di Romania, bambagio di Puglia, bambagio di Calabria, bambagio di Cicilia, bambagio di Malta."

Thus, mention is made here of Romania 'Romagna', Puglia 'Apulia', Calabria 'Calabria', Sicilia 'Sicily', and Malta 'Malta' -- four places in Italy and one nearby in the Mediterranean. Bamboo, even Selbourne would agree, does not grow in any of those places, so that it is clear that bambagio does not mean *'bamboo'.

Or, to take two more citations, Opera del Vocabolario Italiano contains attestations like "bambagio filato bianco" and "bambagio filato tinto," which make clear that we have here something that is spun (filato bianco means 'white spun' and filato tinto means 'dyed spun'). Bamboo is not white or dyed and it cannot be spun. Cotton wool is white, it can be dyed, and it can be spun.

In the other citations which Opera del Vocabolario Italiano gives for bambagio, the meaning 'cotton wool' always fits and *'bamboo' never does.

18. The 1991 imprint of the fifteenth edition of The New Encyclopædia Britannica says that "In the Orient, earrings were worn by both sexes; in the West (including ancient Israel and Egypt) as a general rule, they have been considered exclusively female ornaments" (vol. 4, p. 320). That sentence has to be modified so that it takes account of Exodus 32:2.

The entry earrings in Achtemeier et. 1985:233, by Roger S. Boraas, reads as follows: "jewelry worn at the ear. Earrings were listed together with brooches, signet rings, and armlets (bracelets) as gifts brought to the tabernacle in the
wilderness (Exod. 35:22). They were offered with beads and other items as atonement for defiling the dead (Num. 31:50), and Ishmaelite men gave them as material from which Gideon made an ephod (Judg. 8:24-26). They were viewed by Ezekiel as part of the garb of the exquisitely clothed woman (Ezek. 16:12) and reflected God's treatment of Jerusalem. In archaeological evidence, earrings are most commonly found in burials, and the forms of single and multiple pendant designs frequently incorporated the lunar motif as a most popular expression. Materials used included silver and gold, as with rings and bracelets. Filigreed gold is sometimes extremely fine work in these examples, and the numerous burials with single earrings reflect the practice of Roman times.

19. Reginald Foster, one of the Vatican's Latinists, wrote me on 1 July 2006 that he too could find no evidence, whether linguistic or pictorial, earlier than the fresco (his last sentence in the following letter alludes to the last sentence of mine, which was either "Musae circum te saltent cantentque" or "Omnes Musae in officina tua saltent cantentque" -- I cannot remember which I wrote):

Romae ipsis scribebam Kalendis Quintilibus MMVI.

Davidi suo Reginaldus salutem.

Perlibenter tibi latinis rescribo verbis id cum humaniter poposceris officium.

Non ipsemet arbitror posse te documentum vetustius reperire quam picturam notissimam illam Florentinam ad "Domini canes" spectantem, quandoquidem Praedicatorum Ordo novus eo tempore fuerit, quandoquidem titulus sive appellatio illa potissimum "Inquisitione vigente" percrebruerit.

Nihil addere igitur huc possum quo tuo adiuveris in studio, de quo gratulor tibi ex animo et in quo ut sedulus perseveres hortor.

Valebis interea nosque Romae amabis in schola atque officina saltantes et cantantes. -- Reginaldus

20. Here's an explanation that has just popped into my mind, one just as ridiculous as any that Selbourne has offered in all his other lame attempts to defend himself: "Jacob," though a rabbi, neglected his Jewish studies (which is true, for, as we will see later, he made numerous blunders of Jewish interest) in favor of Christian studies (which is true too, for, as we will see in section 10, he possessed accurate knowledge about Thomas Aquinas). Being therefore versed in Church Latin, he coined the pun himself, which, since he was a rabidly anti-Christian Jew (as we will see later), he shouted at every Dominican he ever met. The Dominicans liked the pun and therefore adopted it as a self-designation (this was the earliest known instance of neutralization or melioration of opprobrious terms by adoption as self-designations) and asked Andrea da Firenze to paint an appropriate fresco. Selbourne 1997a could be subtitled A Book of Fictitious Firsts.