Disputing the value of former research is a constant temptation. It can be useful, scientifically speaking, or it can be useless, when it is solely an attempt to destroy. The paper by David Luders (2001:247-87), bearing the title given to this retort, offers a number of unfortunate criticisms showing little foundation in fact.

Luders lacks an understanding of the contexts of Garanger’s and my own fieldwork. Both were part of a common project, carried out over a number of years, which aimed at providing a more or less complete inventory of the oral traditions and the social structures of a given cultural area, from Efate to Southern Epi, which until then had been little studied.

All of the data collected by me was collated, then coded and transferred to a computer (they were rather big and cumbersome in those days). The computer analysis, carried out by Jean-Claude Gardin and the staff of his computer laboratory at the Centre National de la Recherche Scientifique has shown that the global social and symbolic system of the area was entirely coherent, internally that is. This means that no logical fault could be found within the total data. Each informant was sufficiently conversant with the system as a whole so as to give what fitted exactly into it.

I dare say that this was the very first result of this type and of this magnitude. We dealt with more than a thousand informants. The representatives of every lineage, in every village of each island, had been listened to, mostly inside discussion groups. Women were always present, not least to provide a check on their men-folk’s temptation to aggrandise themselves and their lineage through prestige-seeking utterances. Thus this information stands until somebody has the courage to undertake the same work in greater detail, with as many hundreds of different informants as sources. Criticism on the basis of only very few informants is not valid. The information they provide can only be added to the total; it cannot be used as substitution. The claim that it is better than the results of our earlier work has no scientific basis whatsoever.

Such claims come as a constant temptation, which should be resisted. José Garanger obtained new details on oral tradition when dealing with his different and so very rich archaeological material. This data is neither better nor worse. It is what he was told on a certain day, at a certain place. It was not used for the computer analysis, because it did not exist at the time that analysis was done. My work was carried out earlier than Garanger’s, although its publication was at a later date.

Archaeology as a means of checking on the oral tradition was not only “alluring”, but quite a new idea at the time. It was carried out in a logical way. The workers on the excavations were solely those who had the traditional right to touch the graves and, thus, had the knowledge to be able to give precise commentaries on anything which came to the light. No part of the find was taken away, except for very few pieces, and these only with special permission. Subsequently, these were all returned. All the graves have been filled in, with everything left in place.
For Pacific archaeology this was the first time that researchers acted in such a way. One of the beneficial results was the wholehearted co-operation of the local communities and an atmosphere of traditional religious revival. This method had been built into the project, thus changing in a way what had been the accepted rationale of archaeology in the area. The value of it was then duly recognised, but little followed. During the whole time of the project, no excavation was done other than where the people decided they would allow it and offered the means to do it. This consisted of their constant presence and their co-operation, with both their hands and minds.

The same can be said of the ethnography in the field. The systematic research from place to place was soon understood and created an atmosphere where no local resistance was encountered. The very public aspect of the method allowed for control by the people themselves of what had been recorded anywhere; the existence of the information we sought as a formalised oral tradition helped each research participant follow the detail of the research. There was no thought of a mysterious information being hidden by the white man. What I collected was recorded in such a way that any literate Melanesian could have access to it during the whole time of the work, thus making sure that nobody had entered any fanciful material. It did happen, mostly on Tongoa and Tongariki, that some people, displeased with what had been said, or of what they had felt obliged to say in public, came on the side and offered a very different story, which was duly recorded too and given the same treatment.

David Luders thinks he can criticise the apparent incoherence of some traditions and writes about our “assertions”. We did not assert anything, we did not pass judgment on any part of our data. We did not choose which part we took and which part we left. We did not attempt to correct the apparent incoherence of this or that story. We took the data as it was and never attempted to rewrite it on the basis of other information elsewhere. But this is precisely what Luders does and that is where he is very wrong.\(^3\)

He also does not take into account the situation at the time: a time when social anthropologists were decrying and denying the value of oral tradition, on the basis of Radcliffe-Brown’s well-founded critique of the scientifically very unsatisfying work by S. Percy Smith, especially Smith’s writings about “The Fleet”, which, he claimed, brought the Māori people to New Zealand more or less as a whole group, a claim based on Māori texts that were later shown to have been manipulated and, in some cases, entirely rewritten by Smith himself.

Archaeology has been shown, in this Roymata instance, to have the capacity of validating oral tradition up to a point. In the case of Roymata, we do know today that a person of some importance lived in a certain part of Efate, and was given a burial second to none. The oral tradition offers a justification for this importance, for the burial and even for the details found in the excavation. This tends to validate the local tradition, as no other burial of that type has ever been found in the area.

The fact that there is a different tradition elsewhere, the one of Roymata and Roymuri, is quite another thing. That cannot be validated by any archaeology however well done. Rather, it follows the general Pacific Islands tradition of the quarrel between elder and younger brother. It is thus a complete methodological mistake to try and fit it inside the Leleppa tradition of the person whose burial was such an unknown
event in the area. There is no proof whatsoever that the Roymata who was found inside the excavation is the same as the one who is said to have left Kuwae at a time before this island erupted.

One characteristic of the Efate to Epi system of titles is that the same names are carried over by different men over many generations. The consequence is that there was more than one Roymata over time and very probably there were many. To try and fit all the versions of this particular tradition into a single one is thus scientifically naïve.

David Luders does not understand that there are no true and untrue oral traditions, authentic or non-authentic. We hear of and record different variants, each being as true or authentic as the other. We have no scientific criteria with which we could avail ourselves for distinguishing between them and deciding which is better than the other.

Oral tradition obeys a certain number of logical laws in its formal aspects, a point on which Claude Lévi-Strauss elaborated. His structural analysis brings about a certain number of hidden aspects, which translate into new knowledge about them, none of which, however, allows for any pseudo-historic reconstruction.

That any variant of a tradition is contested is a quite normal situation. Contestation of the authenticity of the other people’s declarations are part and parcel of prestige competition world wide. There is no proof of any new-fangled theme to be found there. More or less everyone contests everyone else, in more or less subtle ways, or lets a potential contestation hang over the other person’s social status (for instance if he has been adopted as a child, or as a fully grown man, and can be assumed not to be a true descendant of his present lineage). If specific informants come to the fore and claim that the tradition of Roymata has been manipulated to certain ends, this means that they themselves are just participating in the same centuries-old game, and do themselves fit into the prestige competition which is the constant and real foundation of all Pacific islands societies.

Manipulating oral tradition is part of this game and has been for ages. Archaeology cannot validate this or that tradition as a whole. It can only confirm that a certain person, bearing a specific prestigious name, truly existed at a certain date in time, and therefore was a real person and not a mythical figure. This does not prevent the same, single or multiple, person to be illustrated in all kind of myths which fit into wider traditional and symbolic systems and which cannot have anything to do with any historical events. Archaeology cannot pretend to have the capacity of verifying any extraneous details of any myth. It came into this situation because there was an uncommon burial site and the people knew who was inside.

One of the more important results of pre-war social anthropology was the discovery of the importance to each lineage of their particular share of the oral tradition: that is, of the variants of this tradition that they claim as their own, the next lineage living according to other variants of the same global tradition, or adding new traits to it. Each of these variants has the function of supporting and justifying, a posteriori, the competitive social status of those who claim its ownership.

The concept of the existence of “variants” was developed by Claude Lévi-Strauss. It bears on the apparent incoherence of much of the oral tradition and means that in each case, oral tradition is coherent with the status, ambitions and strategies of the local owners of each specific variant, not the particular personal ideas of a would-be researcher.
What Luders has been told must be interpreted as being the translation of specific strategies, the details of which he ignores entirely. It would take a great deal of space to show this in full, and that will have to wait for another opportunity. There is a lot of unpublished material available regarding prestige competition on Tongoa, and about Tongoa versus the rest of the Shepherd’s Islands and Efate.

What must be said, nevertheless, is that the existence of chieftainships (a European concept introduced early into the situation) was never the important issue, if they ever did exist as such (the vernacular concept is scientifically more fruitful than the word in the white man’s language). A chieftainship does not exist as such, to be defined individually. The constant Pacific Islands institution is not the so-called chief *per se*, but the prestige competition between chiefs, on a dual or multi-polar basis. This means that a society is in constant flux, owing to the differences of the actions and their consequences of all of the actors in that game over time. Coherence or incoherence of such oral traditions that deal with this state of affairs can only be judged by reference to the vernacular logic of the tradition built upon the vernacular social structures, and not through the distorting lens of our Western logic.

The Roymata burial is one of the more interesting aspects of that very complex and convoluted game. The idea that Roymata should be considered as Polynesian, because some outside versions of the myth want him to have come in a canoe, does not hold water. All lineages in the area state they have come in canoes. Nevertheless they nearly all speak Austronesian, non-Polynesian languages, *nakanamanga* or *namakura*, the truly Polynesian related groups having kept their own specific vernacular, equally spoken by nearly everybody through intermarriage. Evidence of Western Polynesian links with Eastern Melanesia is everywhere. But Polynesians have remained as such only where they had come, over the years, in sufficient numbers (e.g., Mwele, Erakor, Fila Island, Makata on Emelae). Elsewhere they have been absorbed by the Melanesian majority (e.g., Tongoa, Tanna, Lifou, Mare, New Caledonia). Methodologically speaking, there is no justification to force any oral tradition to support what it does not acknowledge on its own. Pacific written literature, mostly of European origin, has aired for ages the unwholesome idea that anything of value inside Melanesia must be of Polynesian origin. This is unacceptable and wrong.

There never had been a general state of war before Roymata. This issue is going far beyond what some utterances expressed in Pidgin-English may have meant. The data describes a kind of anarchy, feeding feuds and local wars, and the desire to build a system allowing for greater security of the individuals, who could go in peace where they knew they had (a) local correspondent(s) sharing the same indirect marriage symbol (*naflak*). This desire for peace, and the multiple systems built so as to engineer it, are constant factors in Oceania, the aim being equally to stabilise the land tenure process by taking it out of the realm of the petty quarrels leading to violence.

The concept of a tribute owed by a number of people to a so-called Pacific Islands chief is none other than a white man’s idea. There is no such thing on Efate. All systems of gift exchange, of pig or agricultural produce, function on a reciprocal basis, by which a given individual will give more and receive less in an instance, but give less and receive more in another one.
The interpretation of Roymata coming into conflict with the chiefly system is highly romantic. There is nothing in the data to prove or disprove it. There was no such thing on Efate as a chiefly system, but a system of ranked titles, which is not at all the same thing. On and around Efate these titles were inherited matrilineally. The Presbyterian mission obliged Christians to go over to patrilinearity for the principal titles. They reverted to matrilinearity the day the last missionary left on an outbound plane. This may have created some tension and an opening for manipulation by those whose interests were hurt by this reversal to tradition, taking into account that the system of titles governs the greater part of land tenure (i.e., land for coconut plantations). The other factor involved is inheritance of land from mother to daughter.

A less strange story is that some people from Tongoa have been claiming for themselves the former Ohlen plantation at Devil’s Rock (la Pointe du Diable) on Efate. Mistakenly brought in by Ohlen’s son so as to conveniently block any claim coming from Mele village, they took over the plantation and threw the Ohlens out. Later, they were sent packing back to Tongoa by the Mele people, acting under the authority of the then president of the republic of Vanuatu, himself from Mele. Devil’s Rock is of great symbolical value, being where the dead of all the Efate area come and plunge into the sea, so as to join the subterranean land of the dead. This story might be a bit too hot to handle for the time being.

The discussion about naruk (netik, narëk, ruku) is a mix of two traditions: of the execution of a man on the order of a high-ranking person, which exists all round the western and eastern Pacific, and of the introduction of a certain form of witchcraft at the time of Christianisation, a type of witchcraft coming from outside the area (the same story of its foreign origin is found both in New Guinea and in Vanuatu/New Caledonia), apparently brought in by some of the lascar sailors of the ships of the British West India Company. This may be counted as a modernised script of one or the other versions of the Roymata tradition. To treat it as semi-historical information in the quest for the true Roymata story is mixing romance with what is at best truly conjectural history.

NOTES
1. This retort has been read and approved by José Garanger, whose health did not allow him to write one himself.
2. As Garanger (1972:17) succinctly explains:

   En ce qui concerne les périodes les plus récentes, l’enquête archéologique devait tenter de vérifier la véracité de certaines informations orales. Dans ce monde sans écriture, et plus que partout ailleurs, la collaboration de l’archéologue et l’ethnologue, celui-ci se trouve aux prises avec des traditions encore vivantes, mouvantes, complexes, souvent contradictoires d’un groupe à l’autre parce qu’intentionnellement gauchies par chacun d’eux à son profit, traditions, de surcroît, désorientés par l’impact de la pensée occidentale. Si ce premier travail de l’ethnologue est un précieux guide pour le préhistorien qui aborde un terrain vierge de toute recherche archéologique systématique, celui-ci peut, en retour, essayer de retrouver
les traces de certains faits que les traditions recueillies avaient laissés dans la doute ou l’imprécision et tenter de les localiser dans le temps et dans l’espace. L’importance des travaux ethno-historiques que J. Guiart avait déjà effectués dans cette région de l’archipel, justifiait l’entreprise d’une telle expérience.”

See also Guiart (1998) for a recent discussion.

3. The idea that David Luders, who does not claim to have any archaeological expertise, who was not there at the site and at the time of the excavation, who has not seen anything of the site with his own eyes, would know better than José Garanger, who was there, saw and photographed every detail, or the people themselves, who not only saw, but also excavated and offered comments upon each result, is ludicrous. Luders did not even try to have access to the hundreds of photographs which describe each detail of the collective grave being dug. To decide that there were two different burials rather than one is truly an unreasonable claim without reopening the excavation (or at the very least carefully studying the photographs). Garanger was one of the more careful archaeologists of his generation. I have often raised doubts about the legitimacy of some interpretations of the results by some archaeologists. But to reinterpret what an archaeologists said on the basis of the assistance he received from the people owning the site who were digging with him and who told him, and to do so without a single look at what the grave contained, is to speculate without good grounds. Let the site be reopened, if the people who own the land agree, preferably by a Melanesian archaeologist.

REFERENCES

