Conclusion: Ethnohistory-in-the-field

This study of the 18th-century encounters between Samoans and Europeans which was the subject of Part One of this book, together with the comparative analysis of other Polynesian cases in Part Two, makes a further contribution to an historical anthropology or ethnohistory which has only recently begun to be written. The subject of this relatively new field is the cross-cultural encounters between the Polynesians and the Europeans (Papālagi-Popa’a-Pakeha-Haole) from the 16th century. It embraces the earliest encounters as they occurred throughout the Pacific, and their subsequent development.

By applying the same critical reading that has been attempted here for Samoa to the literature available for each Polynesian Islands group (including Fiji and the Outliers), including among our sources the very first texts, whether in Spanish or in Dutch, we would be in a position to reconstruct a history of the first contacts in Polynesia. No longer, then, would Ulafala Aiavao (whom I quoted at the beginning of this study) need to remind us that we tend to forget the dualism and the asymmetry of the scene, that there were two sides who came together in the ‘discoveries’ just as there are still two sides who meet in contemporary encounters.

Meanwhile, some comparative hypotheses have been advanced about (i) the Polynesian interpretation of the nature of the Papālagi; (ii) the irrelevance of any analysis in terms of the crude barter of goods; and (iii) the irrelevance, equally, of any analysis in terms of sexual hospitality, least of all in respect of those cultures completely misread by Westerners as valuing sexual freedom in adolescence.

On the first point, the comparison has been broad enough to enable us to put forward a conclusive generalisation. The Samoan examples from the western area of the Polynesian region and a number of instances from the central and eastern areas all point to the same configuration, namely that Europeans have in the past been considered as atua, aitu, tupua, kalou. However they were not deemed to be ‘gods’ but, rather, they were considered as ‘images’ of the super-human forces. They were envoys from elsewhere, perhaps from the gods. But these ideas were questions in the minds of the inhabitants, not definitive statements. Whatever their nature, these ‘atua’ sailed ‘on the boats of [the god] Tangaroa’, they came ‘from the [far away cosmic place called] Papalangi…’. They were nothing more nor less than that. The idea that the Polynesians conceived of the Europeans as a Christian-type of ‘divinity’ is a Western projection. However, the critique of Eurocentric analysis should not prevent us from considering why the Polynesians called the Europeans ‘atua’. Closing the case by saying simply that the inhabitants ‘could not have taken men for gods’ only serves to replace ethnohistory with ethnocentrism.
On the question of the ‘bartering’ of goods and the corresponding ‘thefts’, the whole range of Samoan-European early encounters and the early gifts of cloth in the Tahitian-European case also provide sufficient evidence, from both sides of the Polynesian region, to undertake a complete reconsideration of these views.

As to the question of sexual hospitality and assumptions about a cultural value supposedly ascribed to sexual freedom in Polynesia, the Samoan case leaves us in no doubt about the inaccuracies and misinterpretations of long-established European views. To validate this last conclusion definitively at a pan-Polynesian level would ideally require a sequential reconsideration of all cases of first contacts in Polynesia. Yet the ethnographic and historical evidence from Samoa is sufficient to call into question current models of explanation in terms of cultural beliefs and practices that promote free sex, and to urge us to reconsider the sexual nature of these first contacts. Furthermore, the analysis of the Western misconceptions about Polynesian ‘nudity’, through the analysis of the Tahitian gifts of cloth, adds weight to the Samoan case and has confirmed that we must depart from any explanation in terms of sexual freedom. If we take into account the fact that a similar study made of the journals of the French first contact in Tahiti (concerning the sexual presentations enacted by Tahitian girls [Tcherkezoff, in press-1]) affirms the need to revise earlier theories, we then have a strong case, based on studies from both sides of Polynesia, to assert a broadly based and unqualified conclusion, namely, that the story of sexual freedom practised on a wide scale during adolescence in Polynesia is indeed a Western myth and nothing more than that.

These conclusions have been reached by using an ethnohistorical method where the prefix ‘ethno-’ strongly implies that the results from field-based ethnography should be added to the historical study of archives and published texts. That is why a ‘deconstructive’ strategy—such as that followed by Obeyesekere (1992, 1998, 2003), and described in those terms by Sahlins (2003:1)—which is based solely on a reading of past European narratives cannot help us and, in fact, only obscures the matter by creating artificial controversies.

I do not deny that there is some place for a deconstructive methodology. There is no doubt that it is useful in revealing the regime of power on whose behalf European narratives have been constructed: one underpinned by ‘discovery’, missionary and colonising goals, models and practices. It prevents us from accepting uncritically the interpretations and explanations that the authors of those narratives have made. This is exactly the kind of sceptical attitude that I have adopted here in my analysis of the conclusions reached by Lapérouse or Dumont d’Urville about ‘Samoan girls’ and which have been naïvely accepted by James Côté and other uncritical champions of Mead’s Coming of Age in Samoa.
But once we have strong grounds for supposing that such conclusions are biased, all the ethnohistorical work of reconstructing what happened is still in front of us. 'Reconstructing what happened?' Some would question even the possibility of such a reconstruction.

A first objection concerns the relationship between culture and socio-cultural change. To claim that we have reconstructed what occurred implies thinking that, during the event of first contact (the arrival of the Europeans)—let us say that it happened at the point in time $t$—the islanders attempted an interpretation of this event. We therefore reconstruct this interpretation; that was the aim of each chapter of this book. In doing so we suppose that, at the point in time $t-1$, the islanders had some kind of conceptual framework and, more generally, a ‘culture’—can I risk using that term? It was composed of various elements that we treat, when we reconstruct the various interpretations, as if they were all interrelated, parts of a single whole, at the risk of reifying everything of the historical period $t-1$ as a ‘culture’, as if a coherent cultural identity had been in place from immemorial times up until this period $t-1$. In fact, if it were possible for us to work on the period $t-1$, we would of course try to find that dynamic movement that always characterises social facts and we would go back to $t-2$ (where the problem would begin again). But of course we are unable to go back that far. The problem of ethnohistorical work on ‘first contacts’ is that, at a given point, it comes up against a complete absence of sources.

What is ‘that dynamic movement that always characterises social facts’? The interpretative work effected at the point in time $t$ by the islanders according to their $t-1$ conceptual framework obviously involved a modification of their $t-1$ categories. From the moment that the Europeans were taken for ancestors of a new kind, the ancestors of the Polynesians could never be quite the same again. There came into being a vision of the world that grew broader and broader, little by little breaking down the existing sociocentrism to make the notion of ‘man’ (ta(n)gata) something much greater than it had previously been. From the moment that ‘divine’ fecundation was seen as being able to combine the sacred vital principle and the mechanical act of impregnation in the body of a European man, then every European was suitable to be taken as a son-in-law. Now a new Polynesian social class had access to a formerly strict form of hypergamy where ‘divine’ son-in-laws had been limited to high chiefs. It is therefore clear that every interpretation becomes history and that no society has a ‘culture’ that is exactly the same before and after a given event, whether exogenous or endogenous.

If this observation about the dynamics of the historical process is quite obvious, it is still necessary to avoid falling into the trap of the facile response
'First Contacts' in Polynesia

which would be to say (as certain post-modernists do)¹ that, as a result, the notions of ‘culture’ and of sociocultural ‘identity’ are meaningless. Dispensing with these notions would leave us with no basis on which to analyse and understand the indigenous interpretive mechanisms by virtue of which indigenous people made their decisions and constructed their future, all of which we analyse later as their ‘history’, making our analysis of it a study in ‘ethnohistory’. At every point in time $t$, change is occurring. But to understand how this change is occurring, it is necessary to assume a minimal coherence of identity at the point in time $t-1$ which has allowed people to interpret an event, whatever it may be.

The list of such critical events resulting from ‘contacts’ is a long one. It extends from the disruption imposed from outside (the most catastrophic being an invasion that brings death to the majority or even the whole of the population: atolls depopulated by Peruvian slavers, the genocide of the Tasmanians, and so on), to the cases where an event that one believed to be of little consequence, to have been integrated and assimilated, produces—sometimes much later on—a secondary effect. This secondary effect then affects the core values of a people and triggers an upheaval, but the process leading up to it has gone on unnoticed, sometimes remaining so until it is too late.²

A second objection to ‘reconstructing what happened’ is raised by some historians and anthropologists. Let us again take the Hawaiian case. Although such researchers criticise the ‘polemical and political’ background of Obeyesekere’s question as to ‘whether Hawaiians were foolish enough to take Cook for Lono’, they still interest themselves in a ‘more radical epistemological query’ raised by Obeyesekere, namely ‘how someone who makes interpretations while situated in the twentieth century could know what were the thoughts of the Hawaiians of the eighteenth century’ (Merle and Naepels 2003: 23). Any ‘anthropological information [on first contacts] constituted during the 19th and 20th centuries from ethnographical enquiries and from the record of oral literature’ requires, they say, that we understand how ‘an event such as the encounter with Cook or Wallis has become a narrative within the indigenous society that later came to be recorded by a European ethnographer’. The authors urge that what is needed is ‘the deconstruction of the fabrication of this anthropological knowledge’ (ibid.). They add that the same analysis should of course also be applied to the fabrication of the European voyagers’ narratives within the European society (ibid.: 22).

¹ See the discussion in Tcherkézoff (2003b:Postface).
² See Tcherkézoff (1997a) concerning certain forms of land tenure and of private ownership of houses or domestic goods. That is why, for the collection in which this article appears (Tcherkézoff and Marsaudon 1997), we chose the subtitle ‘Identities and cultural transformations’ (Identités et transformations culturelles) to qualify the main title The South Pacific Today (Le Pacifique-Sud aujourd’hui). The concept of (ethnohistorical) ‘transformations’ has no value without that of sociocultural ‘identity’. 
I entirely agree with this call for a close scrutiny of how (i) the published European narratives and (ii) the indigenous narratives recorded by ethnographers were constructed. My analysis of Dumont d’Urville’s narrative of his stay in Samoa affords a clear example. Such a critical examination reveals the role played by the local beachcomber Frazier in the fabrication of the narrative. And certainly, although I have not considered it here, the famous Maori narrative about Cook’s arrival and their conception of the European voyagers as ‘the spirits which are not like our spirits’ requires exactly the same examination. Yet I would still maintain that it is possible to understand some aspects of the way of seeing of the 18th-century Polynesians when they met the first Europeans, through comparing the accounts of early encounters in different parts of Polynesia and cautiously drawing on later or even recent field-based ethnography. Thus our data are not limited to ‘narratives’ (whether these be European or indigenous narratives).

Obeyesekere and his followers would go further yet. Not merely the conclusions proffered, but every phrase of the European narratives must be read as an expression of the regime of power under whose auspices the author conducted his voyage and produced his narrative. This position provides a cheap solution de facilité, an easy way out: it dispenses with the difficulty of scrutinising any European voyage narrative for information, since it allows us to conclude from the start that there cannot be any fact, any trace of ethnographic truth, contained in those narratives.

As Sahlins warns us, we should not indulge in this ‘post-modernist’ strategy of ‘creating doubts about apparent “truths” by arguing that their status as truths is derived [only] from the regime of power on whose behalf they have been constructed’ (Sahlins 2003: 1). Sahlins further cautions us that for any pre-contact or early contact practice (as for instance in the case of ‘cannibalism’ evoked by Sahlins in this recent article) this deconstructive attitude only obscures the historical practices, without delivering any alternative conclusion: the allegation that good descriptions of Fijian cannibalism are really bad prejudices of European imperialists has submerged its historical practice in a thick layer of epistemic murk. The deconstructive strategy [followed by Obeyesekere] is not to deny the existence of cannibalism altogether … rather to establish doubt about it. Not that there was no cannibalism, then, only that the European reports of it are fabrications (Obeyesekere 1998). Even so, not all such reports need be questioned. It is enough to create sufficient uncertainty about a few of them so as to cast suspicion on all the rest, and thus dismiss the whole historical record by implication (ibid.: 64-65). Literary criticism of one or two European texts, reducing them to some fictional genre such as sailors’ yarns, serves the purpose of obscuring the factuality of scores of cannibal events, which then remain unmentioned and unexamined (Sahlins 2003: 1).
We know that for some topics or some periods we have only European reports as a source. Should one, therefore, claim that a doubt must be cast on the entire field and upon the whole period? The very notion of the ‘factuality’ of some ‘events’ would be an illusion. Everything then becomes subject to radical doubt, anything a possible ‘fabrication’. If, adopting the postmodernist perspective, we were to return to our main topic—the role of sexuality in early encounters—each of the two following statements would be quite plausible, and neither of them could be verified or finally disproved: (i) Samoans offered sexual hospitality to the French of Lapérouse; or (ii) they did not offer such hospitality but, rather, they organised a sacred marriage ceremony in which the French were unaware of the role assigned to them. Perhaps Lapérouse’s conclusions were an instance of wild European myth-making, but then again perhaps not. In such a slippery epistemological regime, one could argue that, a hundred and fifty years later, Mead’s conclusions about Samoan sexual freedom in adolescence in the late 1920s could have been as ‘right’ as they were ‘wrong’. In this radically sceptical paradigm we could never know anything as there is no such thing as reliable truth to be extracted from ethnography… But this dismissal of any search for historical truth in fact opens the door to all kinds of disturbing and condescending Eurocentric fantasies about the ‘natives’. Whatever the imagination of European travellers of the past or of the present might produce could, in this mind-set, always be wrong or… right!

As the analyses that I have presented in this book make clear, we can deal only with facts, not fantasies. There are the facts to be revealed from an internal textual analysis. This kind of close analysis was able to reveal, for example, that Lapérouse’s assertions about the sexual hospitality offered by the Samoans were in fact his own interpretations and conclusions, whereas the scene in which we are given a description of the marriage display came from what he, or one of his lieutenants, had actually observed. There are also facts of a strictly ethnographic type: this same scene is described by early 19th-century observers in Samoa such as Williams and Pritchard who explicitly state that they have been told by the Samoans that they are witnessing a ‘marriage’.

Even recent ethnography can assist in ethnohistorical study. It should be clear by now that the reconsideration proposed here in Part One of all the early European visits to Samoa, and even the reconsideration in Part Two of some of the 18th-century events that happened outside Samoa, in Central and Eastern Polynesia, would not have been possible had I not been guided by hypotheses that emerged from the field enquiries that I conducted during the 1980s and 1990s in Samoa. I strongly advocate the potential—and the application of the method that I have employed in order to do so—of extrapolating backwards from more recent ethnographic accounts, as well as from those from the 19th century (at least in those cases where the 19th-century ethnographer had worked in the
local language). This approach constitutes a kind of *ethnohistory-in-the-field*. It is quite different from a purely textual ethnohistory and is, of course, at the opposite end of the methodological spectrum from those deconstructive strategies which reduce all ethnography to the status of a mere tool of imperialism.

Narratives of 19th-century Samoan marriages and field observations from the 1980s about Samoan houses have given us important clues in reinterpreting Lapérouse’s account of the apparently sexual welcome offered by Samoans in 1787. Other recent observations in Samoa about the social nature of the ‘chiefs’ have helped us to raise questions about the meaning of the ‘atua’ nature of the Europeans in the eyes of the 18th-century Hawaiians, Tahitians, Cook Islanders, and other Polynesians. This insight into indigenous perceptions has suggested comparative hypotheses about Eastern Polynesian first contacts which, in turn, helped us to revisit the Samoan case and to reinterpret Lapérouse’s account of the so-called ‘thefts’ carried out by the Samoans on the French boats of de Langle’s party. Finally, ethnohistory-in-the-field has enabled us to re-analyse the whole account of the so-called ‘massacre’ that ensued from these ‘thefts’ on that fateful day of 11 December 1787.

Ethnohistorical research advances dialectically from the field to the archives and back again. At the same time, it proceeds from a hypothetical generalisation built on one case to a verification of that hypothesis by reference to other cases that can then confirm, contradict, or enrich the initial hypothesis. This, in essence, defines the ethnohistorical method that I have employed in this book, whose aim has been to propose to present-day generations of Polynesians certain hypotheses about how their forefathers discovered the *Papālagi*. 