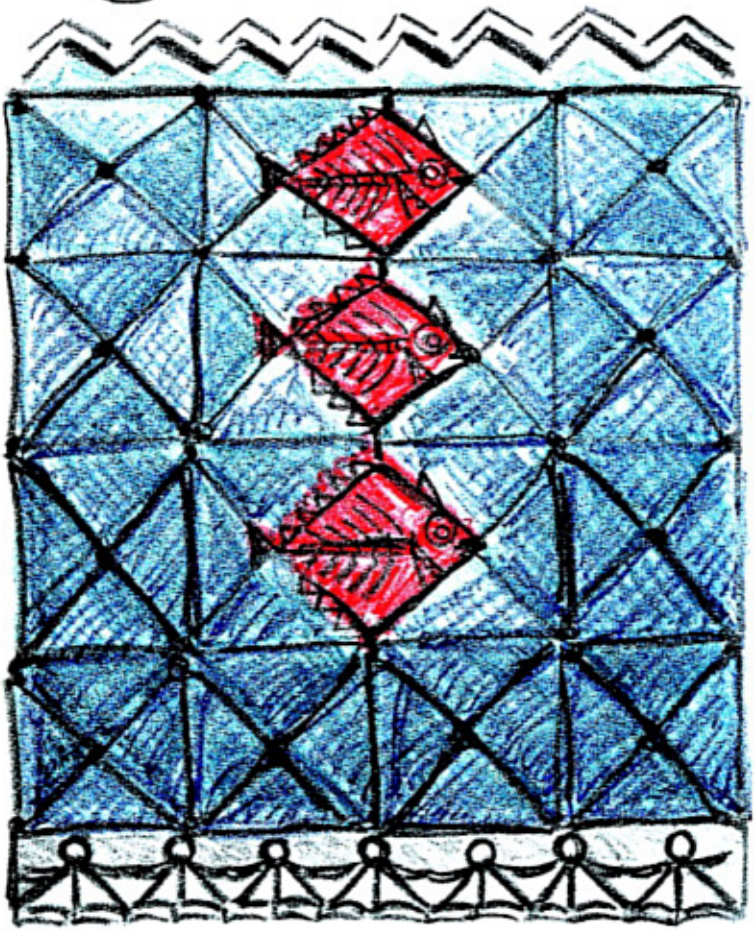
Hawaiki, Ancestral Polynesia

An Essay in Historical Anthropology



PATRICK VINTON KIRCH AND ROGER C. GREEN

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The power of an anthropological approach to long-term history lies in its unique ability to combine diverse evidence, from archaeological artifacts to ethnographic texts and comparative word lists. In this innovative book, Kirch and Green explicitly develop the theoretical underpinnings, as well as the particular methods, for such a historical anthropology. Drawing upon and integrating the approaches of archaeology, comparative ethnography, and historical linguistics, they advance a phylogenetic model for cultural diversification, and apply a triangulation method for historical reconstruction. They illustrate their approach through meticulous application to the history of the Polynesian cultures, and for the first time reconstruct in extensive detail the Ancestral Polynesian culture that flourished in the Polynesian homeland – Hawaiki – some 2,500 years ago. Of great significance for Oceanic studies, Kirch and Green's book will be essential reading for any anthropologist, prehistorian, linguist, or cultural historian concerned with the theory and method of long-term history.

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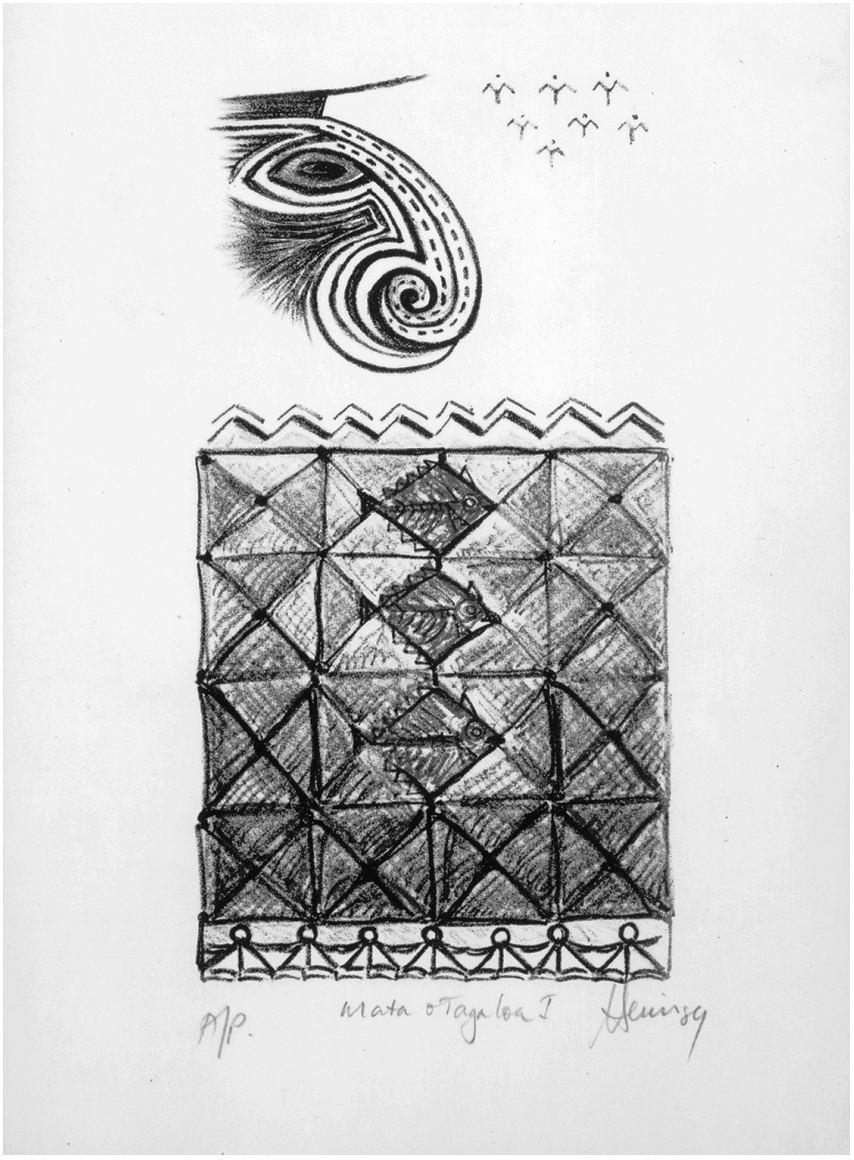

Frontispiece: *Mata o Tangaloa* (“Face of Tangaloa”), by Fatu Feu’u

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and

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To Thérèse and Valerie, for their love and support;
and
to the late Bruce Biggs, preeminent Polynesian linguist

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Preface

Enchanted by the seductively salubrious atmosphere of California's Napa Valley, we gazed over sun-drenched vineyards with the 1993 harvest ripening on the vine, sipping the last of a lush Cabernet while intently arguing the intricacies of some Proto Polynesian term. Perhaps – given the blissful feeling this setting inspired – we might have been excused our conceit that we would conspire to write "a little essay between covers." The notion, naive in retrospect, was to expand slightly on our 1987 article on "History, phylogeny, and evolution in Polynesia" (Kirch and Green 1987), so as to address certain critiques of the phylogenetic approach to historical anthropology, and to elaborate what we call a "triangulation method" for historical reconstruction. The proposition seemed straightforward enough. Yes, a "little essay," perhaps a hundred pages or so. Over plates of roast Petaluma duck and grilled swordfish, our wives had seconded the idea, insisting that we should keep the essay lean and trim.

Nearly a decade later, our "essay" has taken shape as a book, a more ponderous volume than we at first envisioned. Its writing has occupied far longer than anticipated, requiring several international trips and much long-distance collaboration. Yet we do not regret the transformation that our project has undergone, because out of it we have gained a deeper respect for the possibilities of a truly integrative historical anthropology.

We were trained (at Penn and Yale, New Mexico and Harvard, respectively) in the classic *holistic* perspective of Americanist anthropology, and although we are both primarily archaeologists of the Pacific, each of us in our respective careers has endeavored to bring a full spectrum of anthropological evidence and approaches to bear in our research programs. Green early on incorporated historical linguistics into his models of Polynesian settlement (e.g., Green 1966), while Kirch integrated field ethnography into his work on prehistoric ecology and economy (e.g., Kirch 1994a). This book reflects the maturing of those long-standing interests, a statement of our conviction that *anthropology at its best is always holistic and integrating.* At a time when at least one prominent biologist is crying out for "consilience" between the social and biological sciences (Wilson 1998), we would point out that anthropology has always heeded that call.

While engaged in drafting several chapters during June of 1997, in Berkeley, we became overtly conscious of how our respective ethnographic and linguistic experiences in a diversity of Polynesian venues critically aided the construction of the arguments we were striving to advance. Comparative ethnography can, in theory, be carried out by the proverbial ''armchair'' scholar, but there can be no doubting the value of personal ethnographic experience over a range of Polynesian cultures and societies. The most astute comparativists in the Oceanic field themselves had the advantage of original fieldwork in at least two or more locales: Handy, Hiroa, Burrows, Emory, Oliver, and Sahlins, among them.

As with our predecessors, we likewise have spent much time residing and working in many Polynesian societies, including: Anuta, Tikopia, Taumako, Tonga, Futuna, Samoa, ʻUvea, Mangaia, Moʻorea, Mangareva, Aotearoa, Rapa Nui, and Hawaiʻi. Between us we speak or have made significant efforts assembling vocabularies of the following Polynesian languages: Anutan, Tikopian, Taumako, Futunan, Tongan, Samoan, Tahitian, Mangarevan, and Hawaiian. This ethnographic and linguistic background, acquired through a combined total of seven decades of continuous effort in the Polynesian field, has proved invaluable for the task we set ourselves. All this, need we say, has been in addition to our primary efforts as archaeologists in the same islands, where we have endeavored to generate materially documented historical sequences of cultural change. We underscore this point here not to assert our authority, but rather to stress the necessity in historical anthropology of erudition based on broad comparative knowledge. Quite possibly, the kind of work we would wish to see undertaken and extended is, in fact, only possible through collaboration, for it is doubtful that any one individual can command either the necessary depth of methodological and theoretical expertise, or the range of specific knowledge acquired through field or library research.

Writing this book has been a true collaboration. But one of us writes books, having honed the necessary skills, while the other does not; the order of authors recognizes that reality. Of course, each of us read, emended, edited, and critiqued the drafts of the other, so the final book truly reflects a joint effort.

Acknowledgments

Green thanks the Miller Institute for Basic Research in Science, University of California at Berkeley, for a Visiting Miller Professorship which brought him to Berkeley in the fall of 1994, and allowed us to begin our collaboration. Kirch gratefully acknowledges the support of the Center for Advanced Study in the Behavioral Sciences, Stanford, California, which provided him

with ideal working conditions during the final stages of writing and editing. Kirch also thanks the National Science Foundation, which partially funded his 1997–98 CASBS Fellowship (Grant No. SBR-9601236).

We owe a great debt to our colleagues in Pacific historical linguistics, without whose decades of careful work in lexical reconstruction we would not have been able to undertake this book. In particular, the late Emeritus Professor Bruce Biggs of the University of Auckland provided a major underpinning for our research through his POLLEX database of Proto Polynesian reconstructions which he has tirelessly compiled since 1965. Professor Biggs gave us free access to his computerized database, for which we are immensely grateful. It was with great sadness, as this book was in final proof, that we learned of his passing. Other linguists, especially Andrew Pawley, Malcolm Ross, Ross Clark, and Bob Blust, have provided us with information, insights, and helpful critiques over the years.

We are especially grateful to the following colleagues who took the time to read and critique draft versions of various chapters: Peter Bellwood, Bob Blust, Janet Davidson, Ward Goodenough, Steve Hooper, John Moore, Frank Lichtenberk, Andrew Pawley, and Marshall Sahlins. David Tuggle kindly provided simulated southern hemisphere sky charts for the mid-first millennium BC, including that reproduced as Figure 9.4. Hans Schmidt kindly provided us with his transcriptions, in English and Rotuman, of selected excerpts from the manuscript notes of A. M. Hocart, housed in the Alexander Turnbull Library. Serge Tcherkézoff shared with us a copy of his manuscript paper on Samoan *matai*. In the final stages of manuscript preparation, Sara Diamond (Berkeley) and Dorothy Brown (Auckland) provided invaluable assistance with word processing and bibliography. Joan Lawrence prepared the illustrations from our rough copy.

It gives us great pleasure to dedicate this book to our wives, Thérèse Babineau and Valerie Green. They shared our early enthusiasm, encouraged us through the rough spots, and reminded us of the larger significance of our project.

Patrick Vinton Kirch
Roger C. Green

Abbreviations

Language abbreviations

Proto-language abbreviations

PAN	Proto Austronesian
PCE	Proto Central Eastern Polynesian
PCEMP	Proto Central-Eastern Malayo-Polynesian
PCP	Proto Central Pacific
PEC	Proto Ellicean
PEP	Proto Eastern Polynesian
PMP	Proto Malayo-Polynesian
PMQ	Proto Marquesic
PNP	Proto Nuclear Polynesian
POC	Proto Oceanic
PPN	Proto Polynesian
PTA	Proto Tahitic
PTO	Proto Tongic

Modern language abbreviations, and geographic affinity

AIT	Aitutaki (Cook Is.), Central Eastern Polynesia
ANU	Anuta (Cherry Is.), Outlier
AUS	Austral Is. (French Polynesia), Central Eastern Polynesia
EAS	Easter Is., Marginal Eastern Polynesia
ECE	Tuvalu (Ellice Is.), Western Polynesia
EFU	East Futuna (Horne Is.), Western Polynesia
EUV	East Uvea (Wallis Is.), Western Polynesia
FIJ	Fiji
HAW	Hawai‘i, Marginal Eastern Polynesia
KAP	Kapingamarangi, Outlier
MAE	Emae (Vanuatu), Outlier
MAO	New Zealand Maori, Marginal Eastern Polynesia
MFA	Mele-Fila (Vanuatu), Outlier
MIA	Mangaia (Cook Is.), Central Eastern Polynesia

MKI	Manihiki (Cook Is.), Central Eastern Polynesia
MOR	Mooriori (Chatham Is.), Marginal Eastern Polynesia
MQA	Marquesas (French Polynesia), Marginal Eastern Polynesia
MQN	Northern Marquesan dialect (French Polynesia), Marginal Eastern Polynesia
MQS	Southern Marquesan dialect (French Polynesia), Marginal Eastern Polynesia
MRA	Manihiki/Rakahanga (Cook Is.), Central Eastern Polynesia
MVA	Mangareva (French Polynesia), Central Eastern Polynesia
NIU	Niue Is., Western Polynesia
NKO	Nukuoro, Outlier
NKR	Nukuria (Solomons), Outlier
OJA	Luangiua (Ontong-Java, Solomons), Outlier
PEN	Penrhyn (Cook Is.), Central Eastern Polynesia
PIL	Pileni (Solomons), Outlier
PUK	Pukapuka (Northern Cook Is.), Central Eastern Polynesia
RAR	Rarotonga (Cook Is.), Central Eastern Polynesia
REN	Rennell and Bellona Is. (Solomons), Outlier
ROT	Rotuma (Fiji)
RUR	Rurutu (French Polynesia), Central Eastern Polynesia
SAM	Samoa, Western Polynesia
SIK	Sikaiana (Solomons), Outlier
TAH	Tahitian (French Polynesia), Central Eastern Polynesia
TAK	Takuu (Solomons), Outlier
TIK	Tikopia (Solomons), Outlier
TOK	Tokelau Is., Western Polynesia
TON	Tonga, Western Polynesia
TUA	Tuamotu (French Polynesia), Central Eastern Polynesia
WFU	West Futuna (Vanuatu), Outlier
WUV or WEV	West Uvea (Ouvea, New Caledonia), Outlier
WYA	Waya, Western Fiji

Prologue: on historical anthropology

> Our problem may be metaphorically defined as the translation of a two-dimensional photographic picture of reality into the three-dimensional picture which lies back of it . . . The gaining of an historical perspective will mean the arrangement in as orderly temporal sequence as possible, within as definitely circumscribed absolute time limits as circumstances will allow, of the processes studied by our science, the carriers of these processes being generally defined more inclusively than in documentary history.
>
> SAPIR 1916:2

Polynesians called it *Hawaiki* (or sometimes, Kahiki, or Pulotu), the distantly remembered homeland, source of their ancestors, mythical site of the creation of culture, and spirit realm to which their own souls would voyage after death.[1] They honored this ancestral homeland in chant and song, and named newly found islands after it: Savai'i in Samoa, and the large island of Hawai'i, among them. But was there ever in reality such a "Hawaiki," or does it exist only in the shadowy realms of cosmogonic myth? Archaeologists, after a half-century of intensive pursuit of the question of Polynesian origins, would answer affirmatively. More precisely, they would fix the coordinates of this ancestral homeland in time and space: the archipelagos of Tonga and Samoa (with their immediate smaller neighbors), in the first millennium BC. Through an unbroken sequence of cultural change that begins with the arrival of small groups of Early Eastern Lapita peoples around 1100–1000 BC, a distinctive Ancestral Polynesian culture had developed four to five centuries later.

While archaeologists confidently point to various settlements and sites of this period and to their characteristic material assemblages of Polynesian Plainware pottery and plano-convex adzes, securely fixed in time by numerous radiocarbon dates – what do we really know about this Ancestral Polynesian world, this Hawaiki? Is it possible to move beyond the strictly material evidence of potsherds, adzes, and shell fishhooks, postmolds and earth ovens? Simply stated, this is the problem that has energized us to write this book, for we would maintain that twentieth-century anthropology has

indeed developed powerful tools and methods for recovering and writing the deep history of "peoples without history." Yet we are perturbed that as the twenty-first century dawns, the academic and scholarly rush toward specialization and even sub-specialization (not to mention the current postmodern conceit that "culture" or "history" are anything other than academic constructions) threatens to erode the essential strength of a *holistic* vision of anthropology as an integrated set of perspectives and methods trained upon a diversity of evidence.

The founders of the unique Americanist tradition in anthropology – Boas, Kroeber, Sapir, and others – reacted in part to the theoretical excesses of a generalizing "evolutionary" approach, and advocated a more rigorous "historical particularism." They saw the advantage to be gained from multiple lines of investigation and evidence, and thus bundled ethnography, archaeology, linguistics, and physical anthropology together in a way that the European academic world never fully embraced. Eighty years ago Edward Sapir advanced a charter for historical anthropology in his short monograph on *Time Perspective in Aboriginal American Culture: A Study in Method* (Sapir 1916). This paper – once famous but now seldom cited – laid out the potential contributions to historical reconstruction to be made by combining the *direct* evidence of documentary writings, native testimony, and archaeological finds, with the *inferential* evidence provided by physical anthropology, ethnology, and linguistics.[2] Sapir envisioned a historical anthropology that – as a joint intellectual enterprise – required contributions from all of these fields, each with its own unique evidential sources. The historical goals that motivated Sapir have waxed and waned in anthropology over the intervening decades, and the paradigms and methods of the "subfields" (archaeology, ethnology, biological anthropology, and linguistics) have also changed dramatically.[3]

Despite some interesting proposals in the interim (e.g., Romney 1957; Vogt 1964, 1994a), few integrated data-rich explorations along the lines conceived by Sapir have evolved. Nonetheless, in the first decade of the twenty-first century a renewed interest in matters historical may be discerned in the several subfields into which anthropology has been partitioned. These trends lend cautious optimism that our present endeavor – fundamentally similar to Sapir's, but here applied to Polynesia – may be of more than strictly regional interest.[4] Like Sapir, we aim to advance a historical anthropology, but one that brings to bear the myriad advances in data, methods, and theory developed throughout the twentieth century.

Sapir devoted most of his attention to linguistics and ethnology; he only briefly mentioned documentary sources, oral history, and physical anthropology, and relegated archaeology to a single page of his monograph. Sapir's ethnolinguistic bias is understandable, given the embryonic state of New

World prehistory in 1916. Even for the Old World, where archaeology had an earlier start, existing knowledge was then encompassed within the boldest of schemes: Palaeolithic, Neolithic, Bronze Age, and Iron Age. But a growing subdiscipline of anthropological archaeology, especially in North America, increasingly became the main player in historical anthropology, where during the first half of the twentieth century it struggled to develop methods for establishing relative or absolute chronology (Taylor 1948; Trigger 1989a). At the same time that archaeology concentrated on cultural *homologies* (similarities due to common ancestry) and *synologies* (similarities produced by diffusion or borrowing), within what became known in North America as "culture history,"[5] ethnology increasingly rejected historical reconstruction. Following Radcliffe-Brown's pejorative characterization of ethnology's earlier efforts in this direction as "conjectural" or "pseudo-history" (1941:1, 1950:1–2), developments in social and cultural anthropology moved steadily toward synchronic orientations.[6] In the Pacific, the ethnographies of Raymond Firth, Gregory Bateson, and Margaret Mead provide examples. Interest in historical sources and problems was largely relegated to the temporally restricted topic of "ethnohistory" (Dening 1966). Attempts to weld the shorter-term perspective of ethnohistory to the longer-term trajectories revealed by archaeology, proposed by some North American scholars, came to be known as the "direct historical approach" (Wedel 1938; Steward 1942; Strong 1953). Although the direct historical approach fell out of favor in the post-World War II era, it now shows signs of renewed application (Lightfoot 1995).

Archaeology too, at least in North America, went through its own phase in which the particular contingencies of history were devalued in favor of a more "scientific" orientation that sought universal "laws" of cultural process. The New Archaeology of the 1960s and 1970s replaced the earlier emphasis on homologous change with a concern for *analogous* change, driven in part by a paradigm of archaeology as an experimental and even predictive social science (e.g., Watson *et al.* 1971). Anthropological linguistics, in contrast, has always retained to varying degrees its historical component (Hock 1986:v–vi), even while it underwent a range of transformations in its more mainstream descriptive, theoretical, and sociological varieties (Hymes 1964). These continuing historical linguistic enterprises – largely independent of archaeology – have culminated in a series of language-family histories based on genetic subgroupings, for many of the world's languages (Blench 1997: table 2). Finally, like linguistics, biological anthropology has long maintained its evolutionary interests in the genetic history of human populations.[7]

In spite of these varied efforts in anthropological history over the course of the twentieth century – or perhaps just because they remained largely

uncoordinated as the subdisciplines burgeoned and specialized – a genuinely systematic, methodologically rigorous, and theoretically sophisticated historical anthropology of the kind that Sapir envisioned eighty years ago failed to materialize. However, that situation has begun to change, and especially in the Pacific.

The varied strands of a new historical orientation are contained within what Trigger (1989a, 1989b, 1991) calls "holistic archaeology," an approach he sees as forming "a new synthesis for archaeological explanation." Echoing Sapir, Trigger proposes to combine archaeological data with the findings of historical linguistics, oral traditions, historical ethnography, and historical records so as to produce a more rounded view of prehistory, as well as of ethnohistory and historical archaeology. Trigger (1991:562) argues that such interdisciplinary approaches first developed as early as the 1950s, citing examples from Africa (e.g., Murdock 1959; McCall 1964; Trigger 1968). Early efforts were, however, largely rejected by the emerging and rapidly dominant "processual" archaeologists. Renewed efforts at tackling sequences of homologous change are noted by Trigger as recurring in the late 1970s and early 1980s in North America, the Mayan region, and Polynesia, as well as in Africa.[8] They are one basis for Trigger's claim that "the direct historical approach is perhaps the most challenging and potentially important task confronting archaeology today," requiring archaeologists to become "still more open to using non-archaeological forms of data to study the past" (Trigger 1991:563). Other recent examples include the collaborative works of Kent Flannery and Joyce Marcus (1983; Marcus and Flannery 1996) on the long-term historical evolution of the Zapotec and Mixtec peoples of Mesoamerica, and Kirch and Sahlins' collaborative work on the Hawaiian Kingdom (1992).[9]

Calls for a renewed historical orientation within anthropology are not limited to archaeology. Throughout the 1980s some sociocultural anthropologists became increasingly historicized (Ohnuki-Tierney 1990:1–6), taking their lead in part from the well-developed *Annales* tradition of encompassing social history as practiced by Marc Bloch, Fernand Braudel, Georges Duby, and others. Marshall Sahlins incorporated and modified aspects of Braudel's (1980) famous "wavelength" scheme of history in his brilliant work on Captain Cook and the confluence of Hawaiian and British cultures in 1778–79 (Sahlins 1981, 1985, 1995). At the same time, Greg Dening – a historian with anthropological training – was moving in his studies of Marquesan ethnohistory and early European contacts in the Pacific toward what he calls "history's anthropology" (1980, 1988, 1992). The pioneering efforts of Sahlins and Dening have been extended by others (e.g., Linnekin 1990; Thomas 1991, 1997). Such historicization of social anthropology was, moreover, by no means confined to the Pacific arena (see Cohn 1980, 1981;

Ohnuki-Tierney 1990). Biersack, in her introduction to *Clio in Oceania*, a book with the notable subtitle "Toward a Historical Anthropology," writes:

> In varying degrees, the issues of history and theory rehearsed herein bear on other branches of anthropology [in addition to archaeology] and serve as core issues around which the subfields of anthropology may coalesce and enter into collaboration . . . Positioned among historical and cultural studies and at a powerful confluence of subdisciplines within anthropology, historical anthropology provides a forum within which to perpetuate the debates of the last two decades but on new and less parochial terrain. To historical anthropology is thus transferred the theoretical commissions of the discipline: past, present, and future. (1991:25)

A concrete expression of these merging historical interests within social anthropology and archaeology is the collaborative work of Kirch and Sahlins, *Anahulu: The Anthropology of History in the Kingdom of Hawaii* (1992). This project – combining the data and perspectives of a historical ethnologist and an archaeologist, focused on a particular geographic and historic space, the Anahulu Valley – is a book-length example of research that purposively merges subdisciplinary approaches. That more collaboration between archaeologists and historical ethnographers has not been undertaken may reflect a long-standing – and in most cases implicit rather than explicit – bias toward those last few hundred years of global European expansion, and an implicit privileging of textual records (Wolf 1982).[10] Thus Sahlins, while discovering that the "peoples of the Pacific I had studied indeed had a history," could still remark that "these exotic histories . . . as recorded do not go very far back" (1985:xviii). And Dening can claim that "the history of Polynesian cultures *could only be written* out of sources that were European" (1991:372, emphasis added). These comments for the Pacific are echoed in Ohnuki-Tierney's more general remark that "the *longue durée* is not easily accessible for histories of nonliterate peoples" (1990:3, fn. 2).

Thus turning their backs to archaeological colleagues often housed in the very same academic departments of anthropology, historical ethnographers have often haughtily disdained anything except the documentary form of the literate world's historical texts, usually European-authored. In such agendas, the archaeological record is assumed to be either irrelevant to history, or relevant only to a short segment of it.[11] But the historical "texts" of the *longue durée* are encoded not just in the ciphers of Western scribes; they exist equally as material traces dispersed over landscapes and sedimented in their depths, no less as patterns of cognate words in the linguists' comparative lexicons, or as indigenous traditions transmitted orally over long generations. Only when archaeologists, as valued interpreters of their unique historical "texts," are accorded seats in the same seminar room will historical

anthropology truly be able to encompass the *longue durée* of nonliterate societies.

Also damaging to the effort to develop a historical ethnography has been the postmodernist critique in anthropology (e.g., Clifford and Marcus 1986), which among other things has eschewed or rejected regional and comparative perspectives.[12] For a Pacific example, in his book on *South Coast New Guinea Cultures*, Knauft struggles with the problem of describing and comparing *ethnographic regions* in the face of the postmodernist stance that such regions in and of themselves are no more than "the result of a Western academic discourse that projects its own cultural biases and assumes incorrectly that these characterizations reflect other people's reality" (1993:3; see also Knauft 1999). Significantly, Knauft finds a key to the reinvigoration of ethnographic comparison in the analysis of "historical context." While we do not dispute the potential validity of the critique that concepts such as "cultural regions" are anthropological constructions, we do find disturbing the postmodernist tendency to dismiss such constructions out of hand, rather than on the basis of a critical examination of empirical validity.

With respect to linguistics, we detect a renewed and more weighty interest in the intersection of its disciplinary contribution to the historical concerns among the various subdisciplines of anthropology. An example from the 1970s, notable for its methodological rigor, is Dyen and Aberle's (1974) reconstruction of Proto Athapaskan kinship systems. Marshall (1984) offered an exposition on the culture history of structural patterning in Oceanic sibling classifications, a line of inquiry more recently taken up by Hage and Harary (1996). A return to an interest in linguistics and archaeology is evident as one major theme selected for the 1994 World Archaeology Congress, stimulated in part by provocative ideas of Colin Renfrew (1987, 1989, 1992) on the spread of Indo-European (Blench and Spriggs 1997, 1998).

Two of the most robust regional endeavors linking archaeological and linguistic evidence focus on Africa, and on the Pacific. The first includes the work of Ehret and his collaborators (Ehret and Posnansky 1982; Ehret 1998) on Mashariki Bantu origins and their spread in sub-Saharan Africa, and on Nubian speakers in the Sudan. In the Pacific, collaborative linguistic, archaeological, and anthropological research has burgeoned since the 1970s. In his extensive writings leading toward the reconstruction of the Proto Austronesian lexicon, Blust (e.g., 1980, 1985, 1987, 1995a) advances many important hypotheses regarding early Austronesian social organization and culture, as well as the locations of homelands and particular protolanguages, stimulating new archaeological research. The Comparative Austronesian Project of the Australian National University (Fox, ed., 1993;

Pawley and Ross 1994; Fox and Sather 1996; Ross *et al.*, eds., 1998) has likewise adopted a research methodology explicitly incorporating a historical perspective, and drawing upon linguistic, comparative ethnographic, and archaeological approaches. Some of these trends in the study of the Austronesian language family and culture history are reviewed by Pawley and Ross (1993). Recently, McConvell and Evans (1997) attempt to bring archaeology and linguistics closer together, with a geographical emphasis on Australia.

For those who, like us, would advance anew the cause of historical anthropology, Pawley and Ross (1993) make several salient claims. Although they concur that the job of the culture historian is to make sense of resemblances as well as differences by aligning the evidence compiled by various disciplines, Pawley and Ross point out a number of methodological challenges. One is the sizable gaps in the data sets provided by each contributing field of study. A second issue – the problem of synthesis – is more serious and not so readily corrected. Whereas each discipline and subdiscipline has its own kinds of data and particular array of methods for their interpretation, historical anthropology (or ''culture history'' in their terms) as yet has no equally reliable procedures for marrying the evidence of different disciplines.[13] A third problem is ''that much writing on culture history is marred by a weak understanding of linguistic methods'' (Pawley and Ross 1993:428). Nonetheless their conclusion is worth quoting in full:

> The problem of culture history is that it is an interdisciplinary enterprise, but the methods and data used by each of its major constituent disciplines are not readily comparable. Nonetheless such comparisons are necessary in order to evaluate competing hypotheses within disciplines and to gain a more complete picture of the past than any single method can provide. *The AN[Austronesian]-speaking region offers exceptionally favorable conditions for such interdisciplinary research*. Until recently, most prominent hypotheses about the culture history of the AN-speaking regions originated in the data of comparative linguistics or comparative ethnography, with scholars from these two disciplines generally working independently. Archaeology has been a vigorous latecomer. Early attempts at integrating linguistic and archaeological evidence concentrated on centers and directions of AN dispersal, with archaeology providing a chronological framework for linguistically-based scenarios. Currently, the focus of culture historical syntheses is shifting toward comparisons of the lexicons of reconstructed languages with the content and environmental contexts of various archaeological assemblages. There has been no serious attempt to square the recent findings of historical human biology with those of other disciplines, but there are signs that this too is under way. (1993:452, emphasis added)

In sum, not since Sapir has there been such renewed interest in developing an interdisciplinary approach to historical anthropology. What Trigger, an

archaeologist, espouses under the umbrella of "holistic archaeology," the social anthropologist Biersack advocates under the rubric "historical anthropology," while linguists Pawley and Ross label the same endeavor a kind of "culture history." (Biological anthropologists might subsume it all under "co-evolution" and wonder about all the fuss.) This kind of "culture history," moreover, is quite different from (although a congruent development out of) "traditional archaeology" (Feinman 1997; Renfrew and Bahn 1991:407) or "Americanist culture history" (Willey and Sabloff 1980; Lyman *et al.* 1997) of the first half of the twentieth century. One would be tempted to call such a project a "New Culture History," were that label not already appropriated by others (e.g., Hunt, ed., 1989). Although the current emphasis on history has its "new" elements, its roots in anthropology run deep indeed, as a rereading of Sapir reminds us; the adjective "new" is hardly required. We thus find the rubric "historical anthropology" elegantly suited to our purposes.

These varied subdisciplinary efforts, not always coordinated but clearly tending toward a common direction of historical anthropology, might be seen on a larger canvas of late twentieth-century science as part of a movement toward increased sophistication of the "historical sciences." Thus Stephen Jay Gould has drawn a distinction between two modes of science (1989:277–91).[14] The first mode (including *traditional* physics and chemistry, for example) is the Newtonian form concerned with universal laws of invariant expression, able to make predictions about a deterministic universe. In these largely experimental sciences, time is motion, and history is irrelevant. The second mode, of which geology is a good exemplar, is thermodynamically based, concerned with open (rather than closed) systems in which time and history "matter" (Gould 1986). This is the terrain of the historical sciences including cosmology, historical geology, evolutionary biology and – notably – archaeology and historical linguistics, in which retrodiction rather than prediction must be to the fore. As Gould (1980), Ernst Mayr (1982, 1997), and others have eloquently argued, in such historical sciences the recognition of contingency and a historical narrative mode of explanation become not only philosophically valid, but essential. As Gould cogently writes, "If the primacy of history is evolution's lesson for other sciences, then we should explore the consequences of valuing history as a source of law and similarity, rather than dismissing it as narrative unworthy of the name science" (1986:68).

Our book integrates a study in method with a substantive, data-rich case: the reconstruction of the world of the Ancestral Polynesian homeland, of "Hawaiki." Polynesia offers exceptionally favorable conditions for historical anthropology, a model region in which to investigate the congruence of

history, phylogeny, and evolution (Kirch and Green 1987). We intend to explicate more fully the theoretical issues involved, as well as the methodological procedures required to forward a phylogenetic approach in historical anthropology.

Biersack (1991:25), commenting on our 1987 contribution in *Current Anthropology*, wrote that "judging by the responses to their . . . article, the effort [of Kirch and Green] to produce a historical archaeology . . . will prove as theoretically and methodologically challenging and as fraught with contention as the parallel effort in cultural anthropology has proved." The contention is anticipated. Such is inevitably the case with scholarship that aims, not to sit conformably and comfortably within its own disciplinary cocoon, but rather to reach across disciplinary boundaries, to engage in dialogue across ingrained scholarly traditions. We have written a work that dares to draw upon not just the theoretical perspectives and methodological approaches of our own field of archaeology, but also those of historical linguistics and comparative ethnography. Our hope is that this effort will inspire a renewed appreciation of the power of a holistic, "historical anthropology." Most importantly, if this book manages to move us closer to the kind of integrative anthropology envisioned decades ago by Edward Sapir, we shall be pleased.

PART I

The phylogenetic model: theory and method

As a problem, recognized since Aristotle, natural similarities come in two basic, largely contradictory styles. We cannot simply measure and tabulate; we must factor and divide. Similarities may be homologies, shared by simple reason of descent and history, or analogies, actively developed . . . as evolutionary responses to common situations. GOULD 1986:66

Chapter 1

The phylogenetic model in historical anthropology

> Physical type and language, we would say, have no causal relationship; there is no functional reason why a given physical type should occur within a given language family. Therefore, when these two variables do show significant concordance in their distribution this may well represent an important historical fact, namely that the explanation for their concordance can be traced to a common point somewhere in the past. A demonstration that these two factors are also uniquely accompanied by a systemic culture pattern . . . strengthens the belief in a common origin. ROMNEY 1957:36

The "phylogenetic model" has a long pedigree within historical anthropology, traceable in its essentials to Sapir's 1916 monograph. In the 1950s, it was formally developed under the label of "the genetic model," a term that might be confused with a strictly biological perspective of somatic (genetic) inheritance, and which we (Kirch and Green 1987) therefore replaced with "phylogenetic model." This revised label emphasizes historical sequences of cultural differentiation or divergence within related groups, regardless of the mechanism of transmission. Indeed, in the complexities of human history, both somatic and extra-somatic modes of trans-generational inheritance are salient (Durham 1982, 1991; Boyd and Richerson 1985). Thus a phylogenetic model within historical anthropology must incorporate data and perspectives from the full range of anthropological subdisciplines, including biological anthropology, archaeology, historical linguistics, and comparative ethnology.

In this introductory chapter we will sketch the intellectual history of the phylogenetic model within anthropology, including its applications in Polynesia; discuss some current issues surrounding its applicability; compare its principal methods with phylogenetic (cladistic) approaches in biology and linguistics; and, finally, argue the fundamental significance of a phylogenetic understanding of homologous change within historical anthropology. Our aim, in short, is to lay out the theoretical background and framework necessary for developing specific methods for an integrated historical anthropology, especially as these apply to Remote Oceania and Polynesia.

A brief history of the phylogenetic model

Kim Romney (1957) first delineated the specific criteria for cultural phylogenetic units, building upon Fred Eggan's proposals (1954) regarding "controlled comparison" in anthropology (see also Goodenough 1957). Basing his argument upon the fundamental anthropological observation that there is no necessary relation or correspondence between language, biology, and culture, Romney outlined the essentials of what he termed the "genetic model":

> The genetic model takes as its segment of cultural history a group of tribes which are set off from all other groups by sharing a common physical type, possessing common systemic patterns, and speaking genetically related languages. It is assumed that correspondence among these three factors indicates a common historical tradition at some time in the past for these tribes. We shall designate this segment of cultural history as the "genetic unit" and it includes the ancestral group and all intermediate groups, as well as the tribes in the ethnographic present. The genetic unit represents a substantive segment of cultural history while the term "genetic model" refers to the conceptual framework which serves as a tool to order the data. (1957:36)

Romney's seminal proposals were expanded and refined by Evon Vogt (1964) in his introductory essay to a volume on Maya cultural development (see also Vogt 1994a). Vogt, like Romney, stressed that a "common historical tradition" in any area, such as the Maya, would need to be defined on *independent* criteria of (1) common physical type, (2) common systemic cultural patterns, and (3) genetically related languages. Vogt elaborated on the theoretical implications of the "genetic model," explicitly comparing it to models of adaptive radiation in biology:

> In brief, the genetic model assumes that genetically related tribes, as determined by related languages, physical types, and systemic patterns, are derived from a small proto-group with a proto-culture at some time in the past. The model resembles that of the zoologist who views a certain species of animal as evolving and making an adaptive adjustment to a given ecological niche and then radiating from this point as the population expands into neighboring ecological niches. As the population moves into different ecological settings, further adaptive variations occur in the species. But these variations are traceable to the ancestral animal, or, in other words, back to the proto-type.
>
> In the genetic model, as applied to human populations, we assume that a small proto-group succeeds in adapting itself efficiently to a certain ecological niche and in developing certain basic systemic patterns which constitute the basic aspects of the proto-culture. If the adaptation proves to be efficient, the population expands, and the group begins to radiate from this point of dispersal. As members split off from the proto-group and move into neighboring ecological niches, they make appropriate adaptations to these new situations and begin to differentiate – that is,

there are adaptive variations from the proto-type over time as the members of the genetic unit spread from the dispersal area. (1964:11–12)

Vogt moved the "genetic" model beyond a strictly theoretical concept, and proposed a series of methodological steps and procedures for its implementation in historical anthropology. These required "the combined use of a number of linguistic, archaeological, physical anthropological, ethnological, and historical methods bringing to bear the full range of anthropological data as these become available from field and archival research" (Vogt 1964:12). Vogt gave primacy to the evidence of language, suggesting that an anthropologist should commence with "the definition of genetic units in terms of genetically related languages."[1] As Sanders put it: "Methodologically speaking, the basis of defining such genetic units should be linguistic because of the relative exactness of linguistic methods as compared to those of ethnography and archaeology" (Sanders 1966). However, this was primacy only in using linguistic data to define which groups to include within a specific genetic unit; linguistic data were always to be cross-checked against those provided by other subfields, and thus were not in any ultimate sense privileged.[2]

Vogt (1964:10–13) advocated eight steps for the application of the "genetic" model to a specific "segment of cultural history":

1. plot the geographical distribution of related languages;
2. calculate time depth, using lexicostatistics and glotto-chronology;
3. locate the dispersal area and spread of the proto-group;
4. reconstruct the proto-language and proto-culture using the linguistic methods of lexical reconstruction;
5. use archaeological data to test specific hypotheses generated by steps 3 and 4;
6. check the sequences of divergence derived from linguistic and archaeological analyses with the independent evidence of physical or biological anthropology;
7. use ethnohistorical materials to "provide readings on the various branches of the genetic unit" between the time of first European contact and the present; and
8. add ethnographic data on contemporary communities to "map variations in systemic patterns that have survived from earlier time levels and to detect cultural 'drifts' or trends that are still occurring in these living systems."

These steps constitute an integrated methodology for delineating an evolutionarily meaningful unit, one whose branches have diverged from a

common ancestor, according to a historical sequence that can be temporally and geographically defined. Despite some necessary modifications and refinements to Vogt's research procedure (to be discussed below), Vogt's methodology remains eminently sound and reasonable. In effect, Vogt proposed to bring the full *holistic* power of twentieth-century anthropology to bear on the problems originally outlined by Sapir (1916) nearly a half-century earlier.

Despite its potential, Vogt's research strategy was not widely applied, in part because of the move by sociocultural anthropologists away from an interest in historical and evolutionary issues, as discussed in our Prologue. Archaeologists, too, increasingly downplayed history and homologous change. Flannery and Marcus (1983), however, explicitly used Romney and Vogt's "genetic model" in their insightful study of divergence among the Zapotec and Mixtec populations of Mesoamerica. It was an initially independent reading of their work that inspired the two of us to collaborate on a joint application of the phylogenetic model to Polynesia (Kirch and Green 1987).

Controlled comparison in Polynesia

Edwin G. Burrows (1938a, 1940) first championed the Polynesian cultures as an exemplary unit for controlled comparison. His classic monograph, *Western Polynesia: A Study in Cultural Differentiation*, drew explicitly on Sapir's methodology (1916) and established Polynesia as a cultural area (Figure 1.1). Burrows lacked the advantages of a developed archaeological record, and of careful historical linguistic analyses of relationships between Polynesian groups; these were to come only later. Thus, his evidence was confined to comparative ethnography, examining the distribution of a range of cultural "traits," including material culture, kinship systems, cosmogony, and religious beliefs. Nonetheless, Burrows deduced a series of "historical processes which had apparently brought about the differentiation of western from central-marginal Polynesia" (1938a:92), including diffusion, local development, and abandonment or rejection of specific cultural traits.[3]

In the context of a renewed emphasis on Pacific regional studies after World War II, Ward Goodenough (1957) authored a programmatic agenda for comparative research in Oceania. Marshall Sahlins (1958) produced the first new comparative study of Polynesia, now theoretically situated within a *cultural evolutionary* framework (Sahlins and Service 1960), and explicitly invoking a *phylogenetic analogy* by describing Polynesian cultures as "members of a single cultural genus that has filled in and adapted to a variety of local habitats" (1958:ix). Sahlins, however, was interested neither in phylogenetic analysis *per se*, nor in the reconstruction of historical trajectories of change

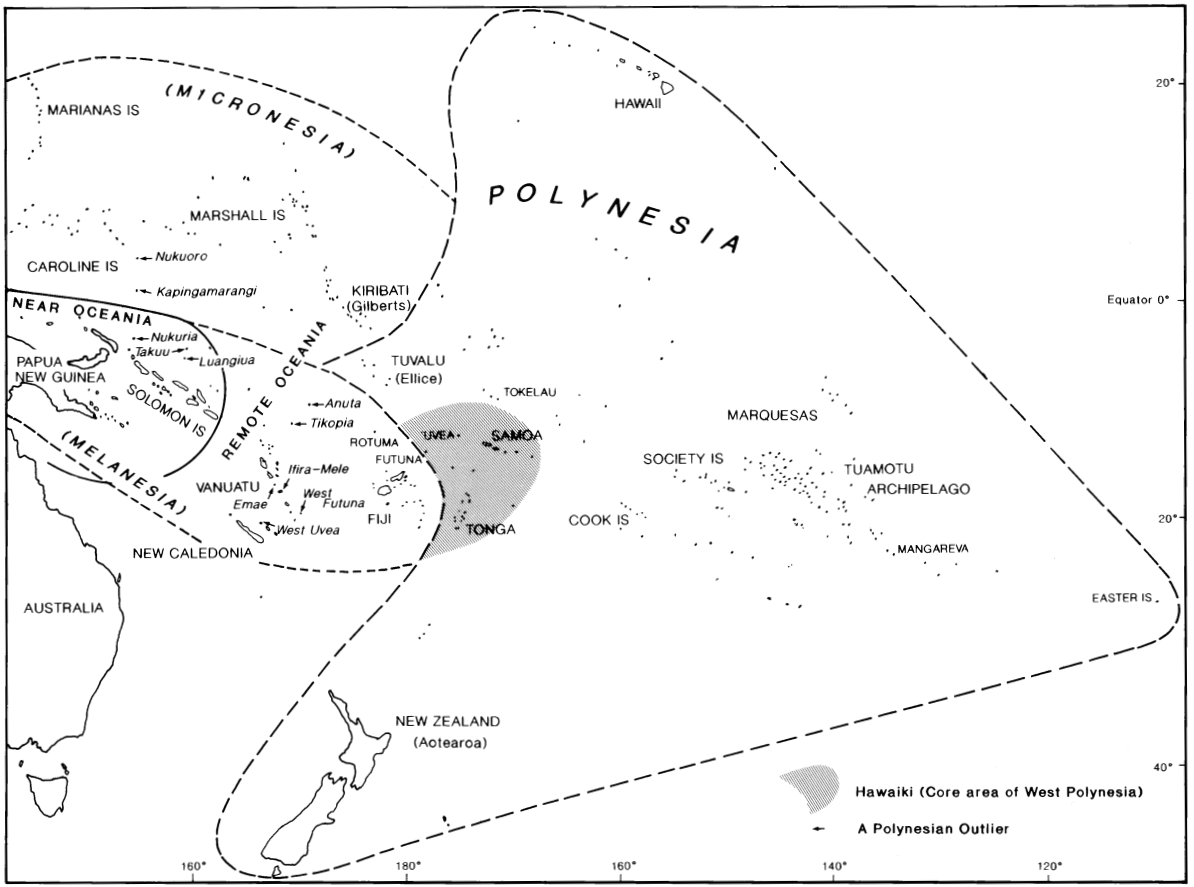

Fig. 1.1 Map of the Polynesian triangle and the Polynesian Outliers.

within Polynesia. Nor were such historical issues the main concern of Goldman (1955, 1970), who like Sahlins regarded Polynesia as a group of genetically related societies admirably suited for comparative analysis. Goldman (1970) nonetheless incorporated newly emerging archaeological data into his work, which, along with genealogically based oral traditions and related ethnohistorical records, provided a historical context for his comparative analysis of the Polynesian "status system" and of descent group organization.

Motivated by broad theoretical questions of cultural evolution, both Sahlins and Goldman were more interested in "process" (analogic change) than in particular sequences of homologous change. Moreover, their enterprises relied on *synchronic* data sets – the ethnographic record – only minimally integrating information from historical linguistic and archaeological sources. As a consequence, particular ethnographic endpoints in their evolutionary schemes inevitably stood as exemplars of putative earlier stages in the historical process. To use Goldman's model as an example, the "Traditional" societies of Tikopia, Pukapuka, or Ontong-Java represented an original, ancestral form of Polynesian society. Such a strategy – we now know – simply will not work for, to paraphrase the great evolutionist George Gaylord Simpson, "one cannot be one's own ancestor." Ethnographically attested societies are not the changeless descendants of their ancestors, even though they may be assessed as culturally conservative. Thus the pioneering strategies of Sahlins and Goldman – although they yielded valuable insights through systematic ethnographic comparison – are not suitable as models for a theoretically rigorous historical anthropology.

The phylogenetic model applied to Polynesia

In the early 1980s, Kirch (1980, 1984a) attempted a broad synthesis of historical change within Polynesia, using an explicitly comparative and evolutionary approach, and according archaeological evidence primacy over the ethnographic data emphasized by Burrows, Sahlins, or Goldman. Although unaware at the time of Romney's or Vogt's "genetic" models, Kirch (1984a:5–8) proposed an essentially identical type of "study of internal differentiation of Polynesian societies," one designed to draw on the power of holistic anthropology:

> Precisely because Polynesia as a region consists of a series of discrete, but historically related societies – all derived from a common ancestor – and because there was direct historical continuity between the "ethnographic present" and the prehistoric past, we are in an excellent position to draw upon ethnohistoric, ethnographic, and linguistic data, as well as upon strictly archaeological evidence in an attempt to understand the region's prehistory. The Polynesian ethnographic baseline does not

provide mere analogies for the interpretation of archaeological data; it illuminates directly the *endpoints* of indigenous developmental sequences. (Kirch 1984a:5)[4]

A graphic model, reproduced here as Figure 1.2, illustrated the process of differentiation within Polynesia. It was fundamentally a phylogenetic model, in which the ethnographically attested Polynesian cultures and societies were regarded as ultimately derived from a proto-group, termed "Ancestral Polynesian Society" (abbreviated APS in the diagram). Although Kirch emphasized a series of successive colonization events (migrations out of the original APS homeland, and out of later daughter communities), and the effects of subsequent isolation between descendent populations, he explicitly pointed out that cultural contact and borrowing had occurred between some island groups (as depicted by the double-arrow linking W_3 and X_3 in the diagram).

Kirch singled out the reconstruction of Ancestral Polynesian Society as a critical step in any evolutionary study of cultural differentiation within Polynesia. Knowledge of the APS "baseline" was necessary in order to assess or measure later historical changes in the descendent cultural traditions within Polynesia. Only by first having some reasonable idea of the social and technological bases of APS would it be feasible to determine which later features were retentions, adaptations, or elaborations of older patterns, and which were entirely new innovations, borrowing, or at times convergences. Recognizing that archaeology alone was insufficient to reconstruct some aspects of APS (such as social structure), Kirch drew upon evidence from linguistics to outline important aspects of APS, including technology, production systems, and social relations (Kirch 1984a:53–69). His methods for such reconstruction were in retrospect insufficiently developed, and some of his reconstructions were later challenged (e.g., Sutton 1990, 1996). We will redress these initial methodological shortcomings, and significantly extend and improve his interpretations of Ancestral Polynesia in Part II.

In 1987, Kirch and Green put forward an analysis of Polynesia, for the first time explicitly referring to Romney's "genetic" model. Others had already delineated Polynesia's advantages for historical anthropology; we sought to extend those advantages by adopting a formal set of procedures under the label of the "phylogenetic model." These derived initially from the analytical steps outlined by Vogt (1964), to whose work we had been introduced through our reading of Flannery and Marcus' monograph, *The Cloud People* (1983). We demonstrated how Vogt's procedural steps could be more rigorously applied in Polynesia, but also argued some "initial propositions" regarding "evolutionary process" within the Polynesian region. We emphasized the critical importance of "establishing homologies, thus clearing the path for the analysis of evolutionary process" (1987:432). We

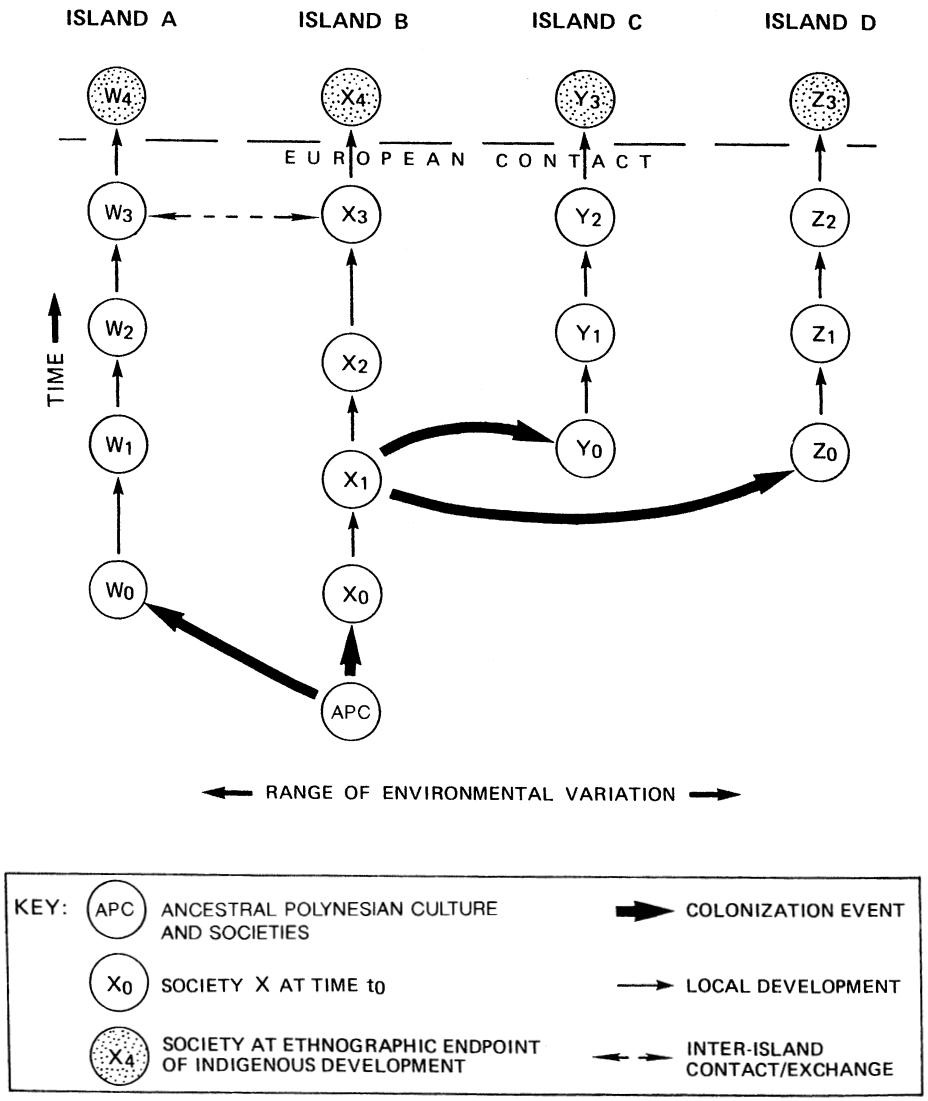

Fig. 1.2 Kirch's 1984 model of phylogenetic differentiation in Polynesia (from Kirch 1984a: fig. 1).