KODAK Gray Scale

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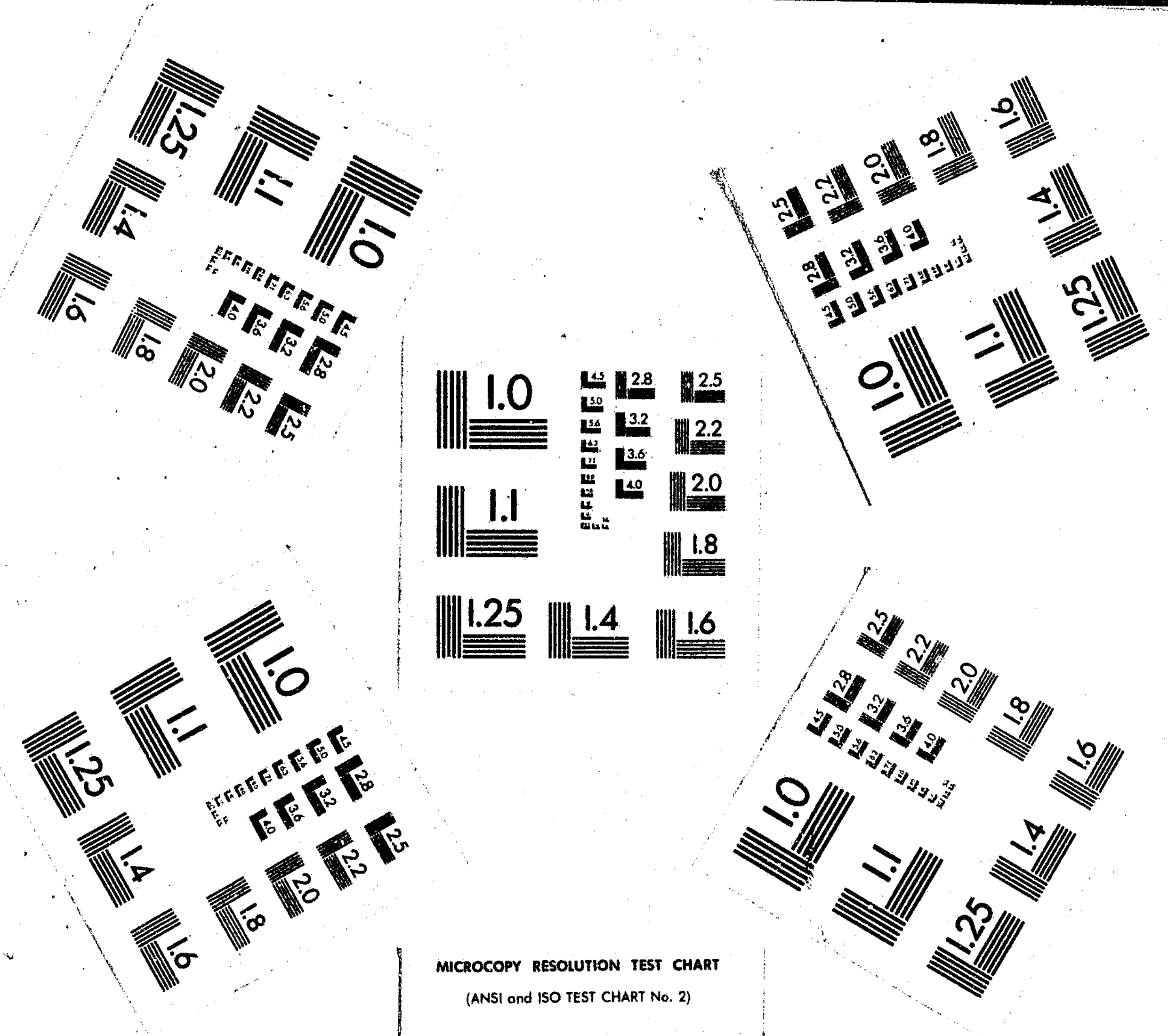

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Expedient technologies?
Obsidian artefacts in Island Melanesia

A thesis submitted for the degree of Doctor of Philosophy
of The Australian National University

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I certify that except where a reference to the work of another author is indicated, all the material presented here is my own.

Michael Hanslip
March, 2001

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Abbreviations

#	number
AN	Austronesian
bp	before present (calibrated one sigma)
c.	circa
CCD	charge coupled device
CD	compact disc
CLSM	confocal laser scanning microscope
CRT	cathode ray tube
D. of York	Duke of York Islands
dpi	dots per inch
DVD	digital versatile disc
EDS	energy dispersive spectroscopy
FAST	functional analysis of stone tools
g	gram
GIS	geographic information systems
kya	thousand years ago
l	litre
L	length
m	metre
N.Britain	New Britain
N.Ireland	New Ireland
NAA	neutron activation analysis
OHD	obsidian hydration dating
PDSM	post depositional surface modification
PFMD	per-fluoro-methyl-decalin
PIGME	proton induced gamma ray emission
PIXE	proton induced X-ray emission
r^2	correlation coefficient
R/SC	Reef - Santa Cruz
SEM	scanning electron microscope
T	thickness
v	volume
W	width
X^2	Chi squared
XRF	X-ray fluorescence

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Production

This thesis was created on Apple Macintosh computers. Text entry started with a Mac II Cx running Mac OS 7.1. Editing and revision moved onto a Performa 5260 running OS 7.5.5 and later OS 8.5.1. Final formatting was completed on an iBook running OS 9.1. All word processing was done in Microsoft Word (versions 5.0, 5.0a, 5.1a and finally 98).

The metrical data gathering, as explained in chapter eight, required the use of an IBM-clone computer for both the hardware and software. Once a file containing all the data was produced, the file was moved to the Mac and manipulated in Microsoft Excel (versions 3 and 5).

Illustrations came from several sources. Photographs were scanned into Adobe Photoshop 4. The map was downloaded from the esri internet website (www.esri.com). All the graphs were created in Excel 5. The site stratigraphy profiles in chapter five were initially completed using Deneba Canvas 3.5. Canvas proved incompatible with OS 8.5.1, so I redid them from scratch using Adobe Illustrator 7.

I had intended to directly paste the illustrations into the Word document. Actually doing this proved impossible (it caused havoc throughout the document). Word is inappropriate for the production of book-sized documents and in practice proved virtually incapable of properly dealing with 250+ pages of text without any pictures at all. Taking all of the illustrations into Illustrator was the solution. The 75 Illustrator image files were reduced to a single PageMaker 6.5 document containing the captions and page numbers.

Adobe products produce PostScript output—a language for printers. Virtually every laser printer is PostScript compatible, but colour PostScript printing is still rather expensive. Using Adobe Acrobat 4, both the text and the illustrations were 'distilled' into a single Acrobat file that yielded excellent quality output on any printer.

The document is set in 12 point Times font from Adobe. The paper is nopaColor 100 gsm A4 stock. The printer used is an Epson.

Abstract

The first section provides the necessary framework for the research undertaken. Questions which guided the study of the obsidian tools are presented. Background theory for Melanesian prehistory, exchange studies and lithic research are presented. This section ends with a description of all the archaeological sites used during the study. Nineteen primary sites located on five islands provide the bulk of the raw data. Six secondary sites and sixty tertiary sites provide the remaining obsidian data.

The central section of the document relates the methods used during examination of the obsidian flakes and the results produced by each approach. Consideration is made of four methods of source determination for obsidian. Advantages of each method are considered from the perspective of the lithic specialist. Variations in distribution for each source are calculated chronologically and spatially.

The amount of obsidian deposited within a site, termed *abundance*, is calculated for each primary site (when possible) in two different units and presented graphically. Combined with mean flake mass, the three measures provide a basis for differentiating the sites by how obsidian was handled. Flake size profiles, a measure of reduction strategy, are presented for each site and time period. Results from the profiling exercise are compared with statistical tests.

Development of a use-wear procedure and the testing of it via a blind-test are related. Application of the use-wear method to the archaeological tools reveals a non-specific approach to obsidian use in the past, and exposes the difficulty of working with flake tools in tropical environs. Surface weathering, which removes most of the traces of use over time, is explored in the literature and presented in photographs. Finally, a confirmation of the unguided nature of obsidian reduction in the region is presented.

The final section of the thesis begins with a discussion of the results of each procedure in turn. The questions which began the thesis are answered in light of the findings of this research, providing a summary of the key issues of the document. In conclusion, *expedient technology* was found to be the appropriate label for all aspects of obsidian exploitation from procurement through to disposal.

An appendix lists the data produced by sourcing several hundred obsidian flakes in the course of this study.

§1

Introduction

Obsidian is a volcanic glass. It is rare, beautiful, and functional. Rare because it is only found in certain locations spread around the world. Beautiful in the way it reflects and refracts light. Functional as a result of the way in which it fractures. Through it we have a window into the past.

One can study the chemistry of obsidian; the percentage of silicon or sodium that make up its structure; the effects of reagents on its surface; the manner in which it changes over long periods of time into perlite. One can study the physics of obsidian; the hardness on the Rockwell or Mohs scale; the way certain methods of percussion produce razor sharp edges; its density and colour. One can simply appreciate obsidian; the way it feels in the hand; the difficulty of grinding or flaking it into a desired shape and the results of doing so.

This thesis is about obsidian. The research that went into this thesis incorporates all manner of investigation into the nature of obsidian. Perhaps if I were a geologist, the obsidian in itself would be enough. As this is prehistory, the obsidian is merely a means to an end.

Obsidian is really a very nice material and it fascinates me. Apparently, from the enormous body of evidence, it has fascinated people for thousands of years—people who have exploited the various qualities of this material to better their life in some way (e.g., Torrence 1986, Berrin and Pasztory 1993). While a large part of this thesis deals with the physical study of the obsidian and as such concentrates on methodology, the ultimate goal is the prehistory of Melanesia, exploiting this information to learn what it says about past human behaviours.

I came to this particular research project through an initial interest in Pacific prehistory. Along the way I found a niche for myself studying flaked stone tools and examination of the flake tools of Melanesia neatly tied both interests together. This project also started from a specific need—for more than two decades archaeologists working in Melanesia had accumulated collections of obsidian flakes without taking full advantage of the data these pieces offered.

When a large group of obsidian collections was offered to me, I could not resist. The "big picture" it presented was fascinating. The study of Melanesian prehistory is at a point where the addition of a region-wide survey of the flaked tools might add significantly to the picture.

My first view of this project was to incorporate four different types of data: the functional analysis of the tools, based on use-wear traces and residues; the source data, based on physical and chemical properties of obsidian; the flaking processes used to take the raw lumps of stone and make utilitarian objects; and the dates showing how groups were related in time. Compared, these data sets would reveal much about the mode of appreciation these ancients had for obsidian. More than that, it would reveal something of how people on far-distant islands interacted with each other and how these interactions developed through time.

Unfortunately, it did not work out quite this simply. Problems in obtaining some of the data sets, and gaps in the required spread of archaeological sites ensured that at least as many questions were raised as were answered. The summation of island interactions in Melanesian prehistory, and the simpler topic of understanding regional obsidian use, will remain an unfulfilled goal. Despite the problems encountered, much was learned.

The following questions are specifically addressed by this text. They could just as well have been presented after the background section (chapters two, three and four) when the purpose for each question was more evident. I chose to present the questions first because for me it seemed the more logical starting point. I note below which section(s) the question is linked with.

(1)—What functions were obsidian tools used for?

Understanding the tasks employed with stone tools (see page 150 for a list of functions) is important not only because it reveals what sorts of activities the users were engaged in, but because obsidian is ideally suited only to a small suite of jobs. It reveals the value placed on the resource by a local group. Three further questions are subsumed within this functional one. Did functions vary from place to place? Did functions vary through time? Did functions vary with the source from which the obsidian came? The answers to these questions reflect cultural variation across the region, and through the years.

Use-wear analysis of stone tools is introduced with other analytic methods in chapter four, and considered more fully at the beginning of chapter eight. Results are discussed in the "Functional analysis" segment of chapter nine.

(2)—How were obsidian flake tools produced?

As with function, method of production reveals the value of obsidian to a group. It also reveals links or disparities across a region based on similarity and difference in the process. The same three sub-questions can be asked here as for use (question one above), with the same interpretations resulting from the answers. Did production vary from place to place? Did production methods vary through time? Did production processes depend on the source from which the obsidian derived?

As with functional issues (question one) analysis of reduction processes is one of the range of analyses introduced in chapter four. The majority of chapter seven is devoted to exploring obsidian reduction strategy. There are two segments of chapter nine ("Reduction strategy" and "Bipolar flaking") that consider the results of this line of inquiry.

(3)—How are form and function linked?

One can pound in a nail with a spanner, but a hammer works better. Even in a hypothetical situation where production means and function are identical, the form of any tool type has a range of permissible variation. Cultural differences would be expected to produce stylistic variation in at least one of these three (that is, production means, function and form) aspects of obsidian exploitation.

In a general sense, this is a key question underlying most lithic research and as such is discussed in chapter four. In the specific case of these Melanesian assemblages, chapter eight is devoted to this topic. As such a central issue, much of chapter nine deals directly with this question.

(4)—What is the nature of the exchange system?

Obsidian occurs naturally on only a few islands in Melanesia, but obsidian artefacts are found in most island groups. It had to get there by some human means. Potential variation in obsidian exchange ranges from direct acquisition of enough for personal needs as the most simple means through to the purchase from specialised merchants as the most complex option. Each possibility obviously offers different interpretations for the way islands were interconnected in the past. Are there differences in exchange modes across the region, or through the period of prehistory?

Chapter three provides the background for the study of exchange, but in this island environment the context provided by chapter two is also an essential part of understanding this subject. The topic of chapter six, source determination, is the basis by which archaeologists elucidate exchange networks. Exchange is only briefly dealt with in the results of chapter nine as this research contributed little to our understanding of exchange in Melanesia.

(5)—Are there alternative raw materials competing with obsidian for tool production?

As I noted in question one, obsidian is ideally suited to only a few tasks. If obsidian is used in any unsuitable activities, it could be due to a lack of alternative materials or because of specific decision by the tool user. First one must assess if there are tools made from other stone, and then determine if they are exploited in a similar or different manner.

The methods of chapter four combined with the data of chapter five provide the necessary means to discuss non-obsidian lithics briefly in "The chert" segment of chapter eight. Chert is discussed where appropriate in several other sections of chapters eight and nine.

While these first five questions were main themes from inception to completion of the research, new research questions arose as obstacles were encountered and data were analysed. These questions included:

(6)—What processes account for the modification of the surface of the majority of the stone tools examined?

Given that the flake tools are not in the same condition now as they were when originally discarded, how did these alterations occur? Retrodiction is much easier than prediction. Knowing the range of difficulties that can be encountered in attempting such research, and why each occurs, will make future attempts more successful.

While the existence of this phenomenon impacts on many of the methods and results in this text, this question is specifically addressed in chapter eight ("Glass hydration and dissolution") and chapter nine ("Glass erosion").

(7)—Which methods of obsidian study are successful, and why?

Not every procedure that was undertaken produced useable data. However, some variations on pre-existing routines and some entirely new methods resulted. Consideration of the procedures themselves should assist future research efforts.

Reading through the methods section of the thesis (chapters seven, eight and nine) should enlighten the reader as to which methods were successful, and which were not.

This question is addressed in chapter nine in discussing the results. It is only in chapter ten, however, that this question is specifically dealt with.

(8)—What next?

Reflecting on the extant results points the way for future research. Unlike the previous seven questions, this one is as much based on opinion as on experience. Only in chapter ten can this question be explicitly reviewed based on the whole of the thesis.

Chapter two begins with a review of regional prehistory. Following that is an examination of exchange in chapter three, covering both the history of archaeological exchange study and specific arguments important in Melanesia. Chapter four completes the background section of the thesis with a review of lithic studies relevant to this research project. Each site involved in the study is covered in chapter five. Obsidian sourcing is a very important aspect of Melanesian archaeological research, so the methodology section in chapter six examines means of sourcing and their applications to these particular obsidian flakes. Chapter seven addresses obsidian transport and flake production, and chapter eight details the study of function in obsidian flakes, the shape of the flakes, and the way in which factors prevented a really useful functional study. The results are discussed in chapter nine. Finally, in chapter ten the eight questions introduced above are discussed in light of the evidence. An appendix provides a listing of the raw data I produced for the density sourcing procedure as discussed in the body of the thesis.

§2

An overview of Melanesian prehistory

The next section of the thesis, consisting of chapters two through four, discusses the background issues that brought about the research. Put another way, where did the questions in chapter one come from?

Some research projects take a narrow question and expend every effort to investigate that question alone. Were I attempting a project in this tradition, the questions asked in chapter one would be much too far ranging and broad to be covered within a single dissertation. Instead, I gathered a collection of artefacts and sought to learn as much about the collection as possible by asking many questions. As a result no portion of the research can really be considered separately from the rest. Virtually all of the methods employed were interconnected in some manner and the results of one investigation altered the course of concurrent ones.

The background to any investigation generally consists of theory and history. This chapter examines the theory and history of Melanesian archaeology in general. Chapter three discusses these two points with regards to exchange. Finally chapter four looks into the background of lithic studies in archaeology.

What is theory? In the preface to her book, Torrence (1989a) discusses how energies put into stone tool research through the late 70s and early 80s elucidated methods to detect and characterise variations in stone tool assemblages. She wanted to go beyond that to explore the reasons for the observed differences, both in the production methods and in the final products. I believe this relates directly to a comment I received on an earlier version of this thesis. That while I professed to be "doing prehistory" I was in fact "doing archaeology". For the source of this comment, archaeology consists of the methods and practices exploited by archaeologists—the methodology. Prehistory is the result of the application of this methodology—applied archaeology.

Torrence's critique of this obsession with methods is understandable but not entirely fair. During the development of a new methodology, it requires great effort to produce a viable procedure. Very little space or time can be devoted to real applications beyond simple test cases. It follows that this thesis should be seen to be more archaeological than prehistorical because more space is devoted to methodology than to underlying cause. In this instance the archaeological versus prehistorical mix was determined more by the data themselves than by the intent of the researcher.

Odell (1996), defines theory in general as "a set of systematically connected and verifiable propositions concerning the underlying principles that govern relations within a set of phenomena." When dealing specifically with stone tool assemblages, theory was more specifically defined as either: the physical principles relating stone tools to their source, their production or their use; or the behavioural principles governing the function of stone tools in a society. Torrence's call for more attention to theory refers to that theory which is contained in Odell's second definition. Odell comments that he did not wish to restrict participants at the lithic theory conference by assuming either definition was correct. In fact, it would be impossible to develop or apply a research procedure without at least implicitly taking account of the underlying physical principles and therefore having a theoretical basis for one's work.

The underlying principles and propositions can be found in examining prior research as well as in specifically theoretical works for any given topic. With theory in mind, I turn specifically to Melanesian archaeology.

Melanesia consists of the island of New Guinea and the island archipelagoes lying to the east through to and including Fiji. This includes the Admiralties, the Bismarcks, the Solomons, New Caledonia, Vanuatu and Fiji, and virtually all the islands in this geographical space not specifically included in the mentioned groups. To the west of Melanesia lies Island South East Asia; to the north lies Micronesia; to the east is Polynesia; to the south are Australia and New Zealand. Within the boundaries of Melanesia, this research confines itself to a region of Island Melanesia where suitable archaeological sites have been found and where the resulting collections were available for study. Island Melanesia specifically excludes the island of New Guinea, which together with Australia, Tasmania, and the Aru Islands were part of the continent of Sahul for a long portion of history (Ballard 1993).

Much of the following discussion on Melanesian prehistory necessarily concerns itself with the Lapita era. This is in part because I came to this research through an interest in Lapita, but more importantly because I believe it represents the key to understanding much of Melanesian prehistory. Also, roughly ten times as many archaeological sites are known from Lapita times as from pre-Lapita times despite the differences in their duration (at least thirty-five millennia for pre-Lapita and only one millennium or less for Lapita).

As I will discuss below, the study of Lapita sites during the past thirty years has led to controversy regarding the ultimate origins of Lapita (Terrell and Welsch 1997). The definitive solution to this problem reflects directly on much of the pre-Lapita history in this region. Lapita may prove a wholly indigenous phenomenon, a migratory group from the west, or some combination of the two. The reality necessarily alters our interpretation of behaviour during the pre-Lapita period, behaviour that stretches back more than ten thousand years in some sites.

Melanesian occupation begins at least forty thousand years ago with the first arrival of people (Spriggs 1997: 23). These earliest migrants came from the west at a time when a lower sea level required fewer open water crossings of shorter distance than the same journey would take today, but the journey still required maritime skills and technology. What we know of these people is derived from very few sites because most coastal sites from prior to eight thousand years ago are currently under water. That any truly coastal sites are found on dry land today is due to local uplift of the former beach. The main alternative location to find sites is within inland caves.

The earliest remains on any island are rare, but there is evidence for the introduction of both plant (nuts, fruit and tubers) and animal (pig, dog, chicken and a few marsupials) species to some of the islands settled (Bellwood 1993: 158, Spriggs 1997: chapter three). An increase in exploitation of marine resources (both fish and shellfish) is detectable through time (Kirch 1997: chapter two). Burial remains are very rare indeed, so we know little about the biological anthropology of these people.

Around twenty thousand years ago, we start to find obsidian pieces from two sources on New Britain (Talasea and Mopir) transported to nearby islands such as New Ireland (Allen, Gosden and White 1989). This transport of small amounts of obsidian across the water is evidence that nearby islands were in contact with each other. It offers the possibility that other, less durable, articles were making the journey along with the obsidian. The archaeological evidence remains consistent for thousands of years, based on the few sites known from this time (Spriggs 1997: 23). In an already sparse record, there appears to be a larger gap in the period from around eight to four thousand years ago (Kirch 1997: chapter two), exemplified by the lack of deposits in Balof 2 cave over this period (White et al. 1991).

In the Bismarcks, a major change is then heralded by a layer of ash; the result of a gigantic eruption about three and a half thousand years ago by the Witori volcano. A thick layer of ash from the eruption covers most of New Britain, and in the central part of the island (near Witori) the layer is up to several metres thick. This eruption buried the Mopir source; it disappears from the archaeological record for nearly a thousand years. The area around Talasea would have been uninhabitable for months, if not years (Spriggs 1997: 76).

When the archaeological record picks up again in the Talasea area subtle changes in comparison to the period immediately before the Witori eruption are obvious. Most obvious in the artefact record, the large tanged obsidian tools are absent, replaced with roughly similar but much smaller versions and Lapita style pottery appears (Torrence 1992). About the time of the Witori eruption, a new culture group appeared in Melanesia that may be responsible for the changes noted, a group archaeologists call Lapita.

LAPITA AS THE FOCUS OF MELANESIAN PREHISTORY

But who are the Lapita Peoples and where did they come from? These still controversial questions were first asked by people finding the pottery early last century (Meyer 1909, Piroutet 1917, Avias 1950). The finding of Lapita decorated pottery in Melanesia has been linked by some scholars with the appearance of the Austronesian (AN) language family, and with a virtual explosion of settlement on previously unoccupied islands (Shutler and Marck 1975, Bellwood 1978, Kirch 1997: chapter one, Spriggs 1997: 96–8).

There are two opposing schools of thought regarding Lapita origins. Essentially at opposite ends of a spectrum, both can be reduced to obvious oversimplifications of the facts. Presented in this manner they may seem almost silly, yet still convey the basic tenets of each stance and allow for a brief presentation. The *indigenous development view* places the origins of Lapita culture within the Bismarck Archipelago. The argument here is that because some aspects of Lapita culture are demonstrably present in the area prior to Lapita times, Lapita culture is a localised development from this pre-existing culture. The ANU Lapita Homeland Project of the mid-1980s was an investigation into the veracity of this line of thinking. Those most closely associated with this include Allen (1984, Allen and White 1989), Gosden (1991a, 1992) and White (1996, White, Allen and Specht 1988: 416, White and Harris 1997).

At the other extreme lies the alternative view that Lapita culture is wholly foreign to the area.[1] The main proponent of this end of the continuum is Bellwood (1978, 1985, 1992, 1993). In its simplest form the argument goes that because Lapita origins coincide with a new language, the origins of pottery, increases in ocean voyaging and other new characteristics that it must be a new people responsible. Of note is that Bellwood has something of a unique viewpoint on this topic as a researcher who has conducted field work in all the relevant geographic areas—Island South East Asia, Melanesia, and Polynesia (Kirch 1997: 282, footnote 6). Recent (but controversial) evidence of Bismarcks' obsidian in Borneo from a seven thousand year old site suggests that the links between Melanesia and Island South East Asia may be old indeed (Tykot and Chia 1997).

When examined at length, neither theory is nearly as simple as I have made out and both are clearly based on some direct evidence. The problem then becomes how to incorporate seemingly contradictory proposals into a unified theory that encompasses all the evidence. Roger Green (1991a, 1994) proposed a compromise theory, sometimes called the *Triple-I theory* (for imported, integrated and innovated—which could otherwise be labelled as Asian, Melanesian, and new). He allowed for a combination of traits arising outside of the Bismarcks (most notably pottery and Austronesian language use) with existing local traits (obsidian transport and use) and unique new traits (including the pottery decorations and a new adze form). This goes some way towards resolving the differences between the two camps. Terrell and Welsch (1997: 554) criticise Green's theory as not being explanatory enough and simply moving what we know about Lapita into three 'piles', but Spriggs (1996a) recently found it useful in assessing Lapita.

In fact, Terrell and Welsch find many negative things to say about the current state of Lapita archaeology in their recent offering (1997, see also Terrell 1989, 1996). After pointing out problems with all three of the above mentioned theoretical views, they examine their own evidence from research conducted on the north coast of mainland New Guinea. More than fourteen hundred pieces of obsidian (of which roughly 25% are from Talasea and 75% from Lou) were recovered but only one sherd of Lapita pottery. They offer their own alternative theory that they have dubbed (following Irwin 1992) the *voyaging corridor* model (Terrell and Welsch 1997: 554–557). It proposes that the Bismarcks are the logical focal point for overlapping spheres of influence. That is, in the Bismarcks the eastern (i.e., Melanesian) and western (i.e., SE Asian)

[1] I think this exemplifies the age-old rule taught as a warning to all undergraduate archaeologists: "when in doubt, invoke a migration."

influences came together because east-west travel through Melanesia would logically be pushed this way by the proximity of the mainland.

For them, the Lapita period is not the beginning of a great period of expansion into the Pacific Ocean, but the height of this era. It was a time where increasing ability and success at sea-faring is counterpoised by increasing ability to create sedentary lifestyles due to expanding food technologies. Lapita expansion is a culmination of an exploratory process begun some seventeen hundred generations earlier, and developed continuously. The geography of Melanesia is perfect for the development of voyaging, with ever increasing distances between islands as one venture eastwards. A concurrent increase in food production ability, via both land-focused horticultural practices and sea-focused intensification of fishing allowed for population increases that bestowed self-sufficiency on individual islands.

Terrell and Welsch (1997: 567–568) consider that for much of the duration of archaeology on Lapita sites (essentially the last thirty years only) it has been called "the Lapita cultural complex" and remind us that a cultural complex is not the whole of a culture. They go on to say that they believe that Lapita is not a monolithic culture at all, but rather a mosaic in which the decorated pottery style is merely a highly visible trait. They believe that Lapita is an unstable taxon with no secure definition.

Actually I do not find that this thinking is at odds with the proposals of Kirch (1997) and Spriggs (1997) in their respective books that cover this topic quite extensively, nor in contradiction with the recent compromise position of some former indigenous development proponents (Allen and Gosden 1996). Spriggs asserts the same philosophy by placing the Lapita homeland in the Bismarcks as by placing the Polynesian homeland in Tonga and Samoa. In a continuum of evolutionary changes, these groups (Lapita or Polynesian) develop recognisably only within the given island chain. For this to occur requires external factors, internal factors, and time.

Links between Island Melanesia and Island South East Asia are numerous and cross multiple disciplines. Linguistic reconstruction connects Lapita populations to Island South East Asia via the AN language family and words for objects unknown previously in Melanesia. The Austronesian language distribution in the Pacific is coincident with Lapita archaeological distribution and the AN language family seems far too pervasive in the Pacific to result from mere borrowing (Shutler and Marck 1975, Kirch 1997: chapter four). Genetics reveals that Polynesians are primarily of South East Asian origin with some notable Melanesian input (Serjeantson and Hill 1989). Lapita pottery is not unlike some South East Asian pottery in method of production and means of applying decoration, but the decorations themselves are unique. Some motifs seen on more than three thousand year old Lapita pottery are quite similar to tapa cloth decorations seen in Polynesia within the last three hundred years (Kirch 1997: chapter five). Thus there is far too much evidence linking Lapita with Asia to conclude that Lapita is a wholly Melanesian phenomenon.

Kirch (1997: chapter one) argues that Lapita peoples were not a single people, that Lapita culture was not a single culture; but that they did all belong to a single cultural group and generally they shared a single biological and linguistic origin. Lapita is 'difficult' because its influences cross the traditional boundaries between Micronesia, Polynesia and Melanesia.

The Lapita culture area has been classified into four regions on the basis of pottery decoration sub-styles. Originally seen as an Eastern and Western division, an earlier grouping found further west explains the name of the "Far Western" region (Anson 1983, 1986). Finally there is a Southern variant as well (Kirch 1997: chapter one). While the labels are geographical in nature, the differences they encompass are also chronological, as the process of change accompanied the migration eastwards to new uninhabited islands. The earliest Lapita sites are in the Far Western region (the Bismarcks) and contain artefacts of Far Western style (Spriggs 1997: 70, prefers the term "Early Western" because of the chronological implications). Far Western style has the most elaborate decoration forms, it began thirty-five hundred years ago and gave way to Western style in the Bismarcks at thirty-two hundred years ago. By then Lapita was also found in the wider western region encompassing North Vanuatu and the Reef/Santa Cruz group (Kirch 1997: chapter one).

According to Kirch (1997: chapter one) Lapita in the Bismarcks persisted for roughly one thousand years, until twenty-seven hundred years ago. In the Western region, it also persisted for roughly one thousand years, until twenty-two hundred years ago. The Southern region Lapita sites were first found from approximately the same date as the Western region sites, around thirty-two hundred years ago. At this time, the Western style (less elaborate, more stylised decoration than the Far Western style) covered the whole of the Lapita range. The Eastern region (Fiji and Western Polynesia) was settled only slightly later, around thirty-one hundred years ago for Fiji and three thousand years ago for Western Polynesia. Eastern style was the last of the Lapita sequence and the most simple. Eastern Lapita ended about twenty-seven hundred years ago when the pottery had developed into Polynesian Plain Ware (Kirch 1997: 69). Lapita-like remains of very late Eastern style have been found in eastern Micronesia. These sites on Truk, Pohnpei and Kosrae date from about two thousand years ago—clearly post-Lapita (Kirch 1997: chapter three).

Lapita, then, as defined by the stylistic variation in the pottery decoration begins at the extreme western end of the region. It develops over some three centuries before instantly (in archaeological terms) moving into the eastern and southern areas of the region. At this time there is extreme uniformity across all contemporary Lapita sites, but by the time the eastern end of the region was settled (only one to two hundred years later) some localised unique developments began to appear in Lapita decorations (also in the generalised material culture associated with the pottery, although uniformity is never high within the non-pottery component). Although there were several centuries of clear communication across the whole region, the trend towards distinctive development ensured the cultures of each island are a unique end product despite their common beginning.

The end of Lapita, therefore, is regionally variable and somewhat hard to define. In each locale there is continuity from Lapita to post-Lapita. Divergent cultural evolution and increasing isolation of Lapita communities towards the end of the Lapita period produced resulting post-Lapita culture groups of great diversity from place to place. Some Lapita cultural traits are found in extant communities today. Pottery of any variety, however, disappeared in most locations (Kirch 1997: chapter three).

Spriggs (1997: 152–61) proposes several hypotheses regarding the end of Lapita culture, pointing out that more evidence is needed before a definitive proposal can be put forth. One of these theories, also proposed by Terrell and Welsch (1997), suggests that increasing sedentism from better food technologies and larger population bases enabled islands to lose contact. As the expense of inter-island voyaging outweighed the benefits of marginal year stability that the voyaging conveyed, the pressure against such travel would increase. Perhaps the same processes of change, applied in different settings and with different stimuli to terminal Lapita culture, led to the radically different endpoints of the big man societies in the west and the chiefdoms in the east (Kirch 1997: 254).

If this were the end of developments and migrations in Melanesia, then the contact societies would have been much different from those known ethnographically. If the phenotype of the typical Lapita person was such that the Polynesian body traits are the result, then all of eastern Melanesia should share in these (or very similar) characteristics. Further, Austronesian languages should be the only ones found on the previously uninhabited islands the Lapita settled and they are not. The Melanesian genes predominate in Melanesia, with only scattered evidence that the Lapita procession through the area ever occurred (Serjeantson and Hill 1989). Linguistically, the Lapita peoples had a much bigger impact on Melanesia than they did genetically. A mixture of Papuan (or non-AN) and Austronesian languages is found across the region, with borrowing and mixing having occurred in both directions. Culturally it is quite impossible to ascribe the proportion of contribution either the pre-Lapita or the Lapita populations had into any given group today because that would ignore the processes of continuous development every society undergoes through time.

The final external influence found in Melanesia (prior to Western contact) stems from Polynesian back-migrations in the past millennium. On Fiji where contact with the Polynesian archipelagoes of Tonga and Samoa was probably continuous from first settlement, the resulting culture is an interesting mix of Melanesian and Polynesian influences in culture, language and genetics. On the Polynesian Outlier islands, those few islands lying outside geographic Polynesia but displaying strongly Polynesian traits (cultural, linguistic and genetic), the arrival of sailing vessels in the recent past resulted in dramatic upheavals of the indigenous culture and language. Melanesia is an area of diversity of all forms: environmental, cultural, linguistic, genetic, and historical.

In many ways the Lapita period is the key to all of Melanesian prehistory. Not only is it necessary to understand how history progressed from the earliest settlers through to the modern situation through the Lapita period, but the Lapita period is the best known piece in the time line. Understanding of Lapita origins and culture provides a starting point to learning about the non-Lapita period Melanesians. A wholly endemic Lapita origin requires a vastly different interpretation of pre-Lapita culture than does the migratory alternative.

To summarise, we have two opposing theories regarding Melanesian development centred on the events during the relatively recent Lapita period. A better understanding of the Lapita phenomenon and its development will yield a better understanding of pre- and post-Lapita culture groups and therefore regional prehistory. The study of the lithic assemblages in this thesis was aimed at this goal of increased knowledge of Lapita in particular, but also at the non-Lapita groups.

§3

An overview of exchange

With forty thousand years of settlement and twenty thousand years of obsidian transport in mind, as discussed in chapter two, I turn to examine the importance of exchange in Melanesian prehistory and its study. Throughout this thesis, I use the term *exchange* as distinct from *trade*. Exchange was defined by Ambrose (1976a: 351) as the transference of goods and valuables by prestation or ceremonial exchange within the exclusive boundary of a group and by the Oxford dictionary (Little, Fowler and Coulson 1973: 696) as the act of reciprocal giving and receiving.

Trade, in contrast, was defined as transferring goods and valuables to and from social groups that were disposed to confine themselves or others within recognised bounded universes (Ambrose 1976a: 351) and as the practice of any occupation carried on as a means of livelihood (Little, Fowler and Coulson 1973: 2339). While the archaeological visibility of both is the same (and overlaps with straight transport of goods also)—objects found somewhere other than where they are known to have originated—the distinction is culturally significant (Allen 1982: 193). A third means of introducing exotic goods is that of settlement migration. That is, the original settlers of an island can be expected to bring a certain amount of supplies with them.

The purpose in defining a difference between these terms is so that the activities of specialist traders are made distinct from the more generalised actions of a community to obtain non-local goods. While trade and exchange exist both between and among regions, the intra-regional movements of material culture are archaeologically invisible. It is the foreign nature of imported goods that make them detectable in an archaeological site. Specialist traders, such as the many ethnographically well-studied Melanesian traders, are groups who could not survive without their trade (Ambrose 1976a, Macintyre and Allen 1990). The visible difference among all three modes of goods transport is the resulting amount of material that is found in a site.

Exchange and trade are both ongoing or recurring events. The volume of material moved through either trade or exchange systems depends both on the intensity of the system and the duration of it. The second definition of trade offered above is the distinguishing characteristic of trade for archaeological purposes. As trade is a livelihood activity, the amount of goods moved must be relatively large to support the traders. Exchange interactions being social interactions among community members incorporate relatively less material per unit time than for trade relations. Migration transport would provide the smallest amounts of goods in the archaeological record because the event only occurs once.

The earliest evidence of exchange in Melanesia is from obsidian movement in the Bismarck Archipelago. Obsidian is an excellent indicator of exchange because of the way it is confined to only certain eruptions from certain volcanoes and also because of the fact that each volcano provides a chemical fingerprint to the obsidian it produces. In other words, obsidian comes from fairly rare and easily identified source locations, and it can be traced back to the exact location it was originally extracted from.

There are only four obsidian source areas in Melanesia (fig 3.1): the Admiralty Islands, West New Britain, Fergusson Island and the Banks Islands. Each of these contains more than one volcanic source of obsidian (see chapter five).

The earliest transport of obsidian in Melanesia begins more than nineteen thousand years ago from West New Britain to nearby islands in the Bismarcks. At this early date it is unclear whether transport represents exchange, direct acquisition, or a combination of the two by the tool users. The New Britain obsidian is known today in archaeological sites from Sabah to Fiji (some eight thousand kilometres distance) (Torrence et al. 1992). New Britain obsidian is a visible import in many New Ireland sites from the earliest occupation levels (Summerhayes and Allen 1993, Kirch 1997: chapter two).

While obsidian in its own right is an important part of the record of any prehistoric community, it has attracted so much attention in archaeological study because it possesses the properties of long term durability, a unique chemistry for each source, and rarity of source locations. Obsidian therefore is often the only indicator of exchange links the archaeologist has to work with, at least in preceramic contexts (Harris 1994). For example, obsidian studies have revealed exchange throughout the Mayan period (Sidrys 1977), for seven thousand years in New Mexico (Findlow and Bolognese 1982) and for five thousand years in California (Singer and Ericson 1977). Use of chemical sourcing reveals that over the course of some three thousand years (six to three kya) obsidian from New Britain went from being found across a range of a few hundred kilometres to across a range of more than three thousand kilometres (Ambrose, Duerden and Bird 1981).

Despite the effort obviously invested into transporting obsidian to the eastern islands away from the sources, there is no evidence of the stone occurring in commercial quantities anywhere. As of 1987 there were only two flakes of obsidian known from Fijian excavations (Best 1987). Ambrose (1973) proposed that it was possible, given the small quantities of obsidian we are dealing with, that obsidian was taken along with the founding populations and used until it ran out. If this was the case, then there are few or no resupply episodes to be accounted for. It was suggested that obsidian functioned as a prestige good which would require only a small amount on any given island (Specht et al. 1988), that exotic items do not require a system of exchange to introduce them and that even though obsidian did make it into the Papua New Guinea highlands periodically in small amounts it never functioned as a prestige good there (Watson 1986). As I will point out below, these comments are relevant because for the most part we are dealing with tiny quantities of obsidian.

DISCRIMINATION AT THE SOURCES

At both the Admiralties sources and at the New Britain sources research has revealed that some complex cultural processes affected obsidian access in each region. At Pamwak (a site on Manus Island in the Admiralty chain), obsidian use begins at thirteen kya and Pam (the collective term for the two small and neighbouring Pam Islands) is the Admiralties' obsidian source responsible for the vast majority of obsidian found in early levels. This dominance of the Pam obsidian lasts until Lou sources suddenly become more important about nine thousand years ago (Fredericksen 1997a).

Export of obsidian from the Admiralties to other islands only begins in the Lapita period. The mere presence of Admiralties obsidian functions as a marker for the beginning of Lapita culture. During Lapita times the source for obsidian used within the Admiralties is split almost evenly between the Pam and Lou sources, but the exported obsidian is almost entirely from Lou. Obsidian from sources on Manus Island is used in lesser amounts for much of Admiralties prehistory, but none has been found exported (Fredericksen 1994: 80–98).

Kennedy's work in the Admiralties revealed low levels of source discrimination for flaked tool production in the distant past; that is, obsidian was exploited within the archipelago with little regard to its origin (Kennedy 1997). This lack of discrimination by source continued through much of prehistory for obsidian used within the Admiralties itself, but some different mechanism was involved in the Lapita acquisition of obsidian. Despite the obvious connections between Lapita peoples and the Admiralties obsidian sources, few Lapita sites have been located in the Admiralties (McEldowney and Ballard 1991). Further complicating our understanding of social processes in the Admiralties is the huge discrepancy in obsidian abundance between neighbouring contemporary sites. Many sites within twenty kilometres of a source produced the expected high quantity during excavation, while others produced inexplicably meagre amounts of obsidian (Fredericksen 1997a: 384).

Similar behaviour patterns occur in the Talasea area. Torrence and Victor (1995) have shown that despite easy access to several source volcanoes, each of comparably good quality, there is quite a bit of internal discrimination towards the Kutau/Bao source and nearly total domination of this source for exports. Even on Garua island with its own quality obsidian source, a sizeable portion of the obsidian found is not of local origin but from the Kutau/Bao volcano some four kilometres across the bay (Torrence et al. 1996, Torrence and Summerhayes 1997). This despite changing cultural groups through time being responsible for the obsidian procurement, and despite changing access issues with altered sea levels and the episodic eruptions of nearby volcanoes.

While not an issue for this thesis to focus on, both of the main source areas obviously require further research into the use of the obsidian sources. The exploitation of both data sources cooperatively, given the apparent similarity of behaviour involved, is more likely to elucidate the mechanisms involved.

EXCHANGE

There are numerous methods that have been used by archaeologists to study exchange: spatial regression analysis (Hodder and Orton 1976); graph theory (Irwin 1974, Hunt 1988, Hage and Harary 1991); functionalist arguments (Hunt and Graves 1990); graphical analysis (Newman 1994, Findlow and Bolognese 1982); ethnographic archaeology (Irwin 1978, Lilley 1986, 1988, 1991a, Rhoads and Mackenzie 1991); studies beginning at the source and moving outwards to the sites (Ericson 1977, Bettinger 1982, Ericson and Purdy 1984, Baugh and Nelson 1987) and from the sites back to the source (Alden 1982, Findlow and Bolognese 1982); using production process profiles (in the case of obsidian reduction strategy is the production process) to link sites together (Ammerman and Andrefsky Jr. 1982, Ericson 1982, Jeske 1989, Healy et al. 1992); predictive modelling (Irwin-Williams 1977, Plog 1977, Weigand, Harbottle and Sayre 1977, Ammerman 1979); and explanatory modelling (Renfrew 1975, 1977). Numerous regions and cultures where obsidian was an exchange good have also been studied including: among the Maya (Sidrys 1977), the Toltec (Healan, Kerley and Bey III 1983), the Chichén Itzá (Andrews et al. 1989), and in the Valley of Mexico (Boksenbaum et al. 1987), and various regions of the United States (Ericson 1977, Findlow and Bolognese 1982, Jeske 1989)—all in the New World; the island of Melos (Torrence 1984), the Calabrian coast of Italy (Ammerman 1979), and the eastern Mediterranean region (Cann and Renfrew 1964, Renfrew, Dixon and Cann 1966, 1968)—all in the Mediterranean; and in the Bismarcks (Hunt 1988), the Vitiaz Strait (Lilley 1986, 1988, 1991a), Eastern Papua (Irwin 1978, Egloff 1979), and inland New Guinea (Hughes 1977)—all in Melanesia.

Whole books have been compiled on the subject of trade (e.g., Earle and Ericson 1977, Ericson and Earle 1982, Hårdh et al. 1988). It is not my intent to duplicate these efforts, or to produce a treatise on exchange theory or practice. What I have gleaned from reviewing all these papers is that the successful attempts to characterise an exchange network have relied on extensive amounts of quality archaeological data and that the particular approach one uses depends on both the form of those data and the questions being asked. In Melanesia the focus for exchange studies is on the transport of obsidian.

Short distance obsidian transport begins early in both the West New Britain (Kirch 1997: chapter two, Spriggs 1997: chapter three) and Admiralty Islands (Fredericksen 1997a, 1997b) regional sequences. This early dispersion of obsidian may simply be a result of routine voyaging by the inhabitants of the Bismarck Archipelago. No long open-water voyages are necessary to maintain regular connections between New Britain and its neighbouring islands in the south, or among the islands of the Admiralties in the north. Long distance transportation is coincident with, or just slightly predating, the initial Lapita evidence from about thirty-five hundred years ago. From this point onwards, Admiralties obsidian is found outside that chain, and transportation of all Bismarck sources far to the east occurs.

Various functional and cultural reasons have been put forward as to why the Lapita might have gone to the trouble to maintain ties (as evidenced by the repeated transportation of obsidian) over such long distances with the inherent expense of travel. On the functional side lie arguments surrounding the new, increasingly impoverished island environments being occupied as settlement moves east; the demographic instability of founder sized populations; and the need for a lifeline in times of stress such as a large storm or a drought-filled season (Green 1974, Kirch 1988a, Hunt and Graves 1990, Spriggs 1997: chapter six). On the cultural side are arguments concerned with the continuation of traditional ways (that is, when islands were close together they continued return voyages and did not wish to cease despite the longer distances involved); the maintenance of links to 'home'; and the function of prestige goods (potentially such as obsidian) in Lapita society (Green 1982, 1987, Kirch 1988a, Green and Bird 1989). None of these proposals is contradictory with another—any and all of them could be valid and are generally untestable as hypotheses. While the reason for inter-island contacts to exist may never be known, the nature, frequency and importance of such contact are increasingly well understood.

In Melanesia, the temptation to exploit ethnographic analogy is strong. Numerous trade systems have been documented by anthropologists providing first-hand accounts of not only the network, but the motivations expressed by participants and the archaeologically invisible perishable contents of the trading system (Torrence 1993). Torrence (1993) cautions in general against using ethnography as an aid to interpreting archaeological exchange evidence, and Ambrose (1978) specifically denounces the use of ethnography in understanding Lapita exchange. While these views are strictly correct, the issue is more complicated. The exchange system of the Lapita is not directly responsible for those modern trading systems, but is an antecedent to many (Macintyre and Allen 1990, Green and Kirch 1997). This is one reason Lapita culture is so important to the understanding of Melanesian prehistory in general.

Nearly a decade ago Hunt and Graves (1990) described three steps common to most studies of exchange: source the materials, model their distribution, and characterise the exchange organisation. Kirch (1990) assessed the status on these three points with regards to Lapita culture. Source attribution is well established for obsidian and increasingly so for ceramics. At the time he wrote, distribution modelling at the eastern end of the Lapita range was more complete than for the western end, with neither completely understood. Finally he suggested that the characterisation of exchange organisation was at the stage of preliminary models only. Progress during the 1990s has added to our understanding but has not substantially altered things from the 1990 situation. One of my goals in this thesis was to incorporate any new information derived from the study of obsidian flakes into the existing knowledge base and review how this might impact on the view of exchange, not only during the Lapita period but also before and after.

Obsidian is an excellent material to study in an exchange system, particularly in Melanesia with only four well known sources. The chemistry of the material allows specific source allocation determination and it is found in some quantity virtually throughout Melanesia. The study of exchange has played an important role in many archaeological studies in elucidating facts about regions and culture groups. The analysis of exchange systems in Melanesia may hold the key to understanding cultural development in this region.

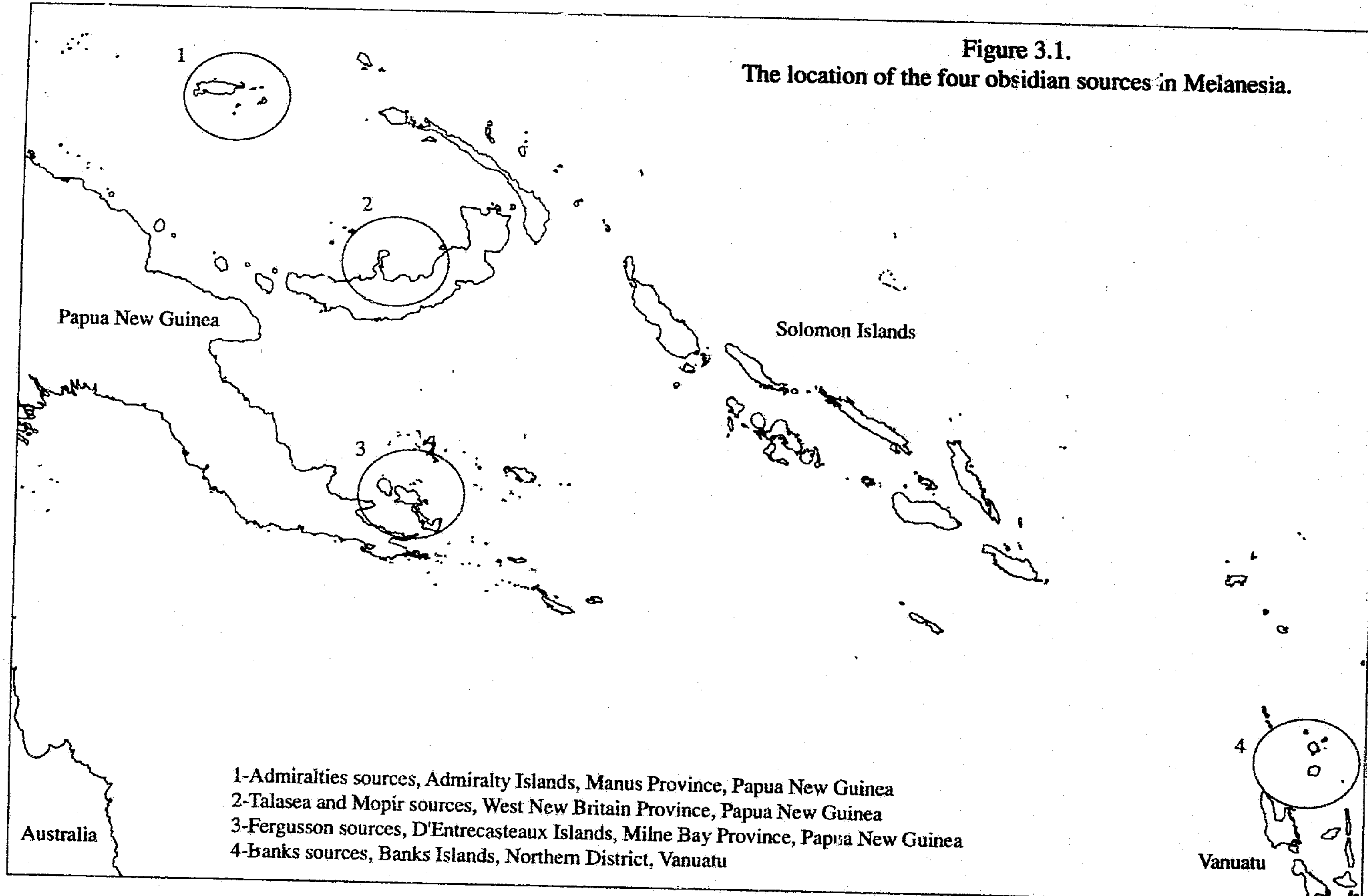


Figure 3.1.
The location of the four obsidian sources in Melanesia.

§4

An overview of lithic research

This chapter concludes the background section of the thesis with a generalised examination of lithic analyses. Specific issues are dealt with in subsequent sections where particular topics occur, but in this section the value and efficacy of studying stone tools is explored.

While stone tools provide the most complete and enduring record of human activity on Earth, from Palaeolithic times (Willoughby 1987) through to ethnographic observations (Meehan and Jones 1988, especially section two), it was only within the last half-century that archaeologists have paid much systematic attention to lithic evidence in sites (Andrefsky Jr 1998: 3–8, Sussman 1988: 1–5). Over the past thirty-five years or so, many methods of analysis have been developed.

Organising flaked tool assemblages by shape works very well with European assemblages where recognisable tool types abound. This typological analysis is closely associated with Bordes (1961, 1968) and other French archaeologists (Tixier 1963, Brézillon 1968, Dauvois 1976, Tixier, Inizam and Roche 1980, Piel-Desruisseaux 1986) and their work on Upper Palaeolithic tool types of western Europe. It fails in the Melanesian context where the vast majority of recovered pieces are plain flakes without any retouch and therefore typologically all fall into the single category of 'debitage' (see chapter seven).

An example of how this organisation process yields cultural information is useful. Imagine removing a random sample of tools from buildings in a modern city. Superficial examination of the tools would allow division between industrial and residential structures. If one knew more about this culture, further examination would sub-divide the industrial collections into such groups as electricians and plumbers, while the different tools from the residences could be sub-divided by levels of wealth, ethnicity and so on.

This same process works on prehistoric assemblages. Binford's (Binford and Binford 1966, Binford 1973) toolkits identify the purpose of a site based on the profile of tool types found within it. If most of the pieces are projectile points, it could be the remains of a hunting camp. If, however, there are a few projectile points and some butchering tools, then it is more likely a kill site where a hunting party found success. A long-term settlement site would contain few or no projectile points and a much different selection of tools from the butchering variety found at the kill site. These same profiles work well in a different fashion in use-wear studies, as I will discuss below and in chapter eight.

A fascination with typology controlled lithic analysis for decades, even in locations outside of Europe where the utility of the approach was questionable. Hiscock (1983) criticised stone tool studies in Australian archaeology during the 60s and 70s. Typology was the central focus, with the assumption that everything important that could be gleaned from lithic artefacts could be determined from 'typing' the pieces (see the discussion on risk management below). This changed during the 1970s. Fullagar (1994) points to Richard Wright's (1977) volume for establishing the kernel of modern lithic analyses amongst Australian archaeologists, while Hiscock (1998) describes Brian Hayden's ethno-archaeological work (1979) in Australia as establishing new ideas about the importance of stone tools in understanding a cultural group. This work during the 1970s led directly to new methods including: sourcing and quarry studies; direct dating of the stone; use-wear and residue; heat treatment and flaking properties; refitting/conjoining; ethno-archaeology and experimental flaking (Fullagar 1994).

The dissociation of function from form created an opening for novel ideas such as Sackett's (1982) proposal that there is room to examine stylistic variation in lithics. Sackett used what he terms an isochrestic approach. Literally isochrestic means "equivalent in use" and in this methodology *style* is a complement to *function* (and therefore inseparable from it). Style is found wherever any choice exists in how to fashion an object, in this case a stone tool. From the choice of which raw material to use in the first place to what the tool gets used for in the end, culturally influenced elements of style are present in all aspects of lithic assemblages.

The isochrestic approach is explicitly in contrast to the iconological approach in which style is only extant in formal variation intentionally included in the final tool by the knapper. The iconological approach resulted from attempting to copy the methods of ceramic analysis into the lithic realm. Such attempts have been unsuccessful due to the restriction in the lithic medium for artistic input independent of function and the ability to objectively identify the variation which does exist. Sackett developed his ideas on Palaeolithic European assemblages, but the potential for other collections is similar, including in Melanesia.

A simple example of a potential stylistic variant from this thesis is the choice of stone in the Reef/Santa Cruz group (see chapter five "Reef/Santa Cruz islands" and chapter eight "The chert"). The choice between obsidian and chert appears independent of reduction process (Sheppard 1992) so this could be a stylistic variation that distinguishes one group from the others, or perhaps even a single person in a particular village given the small numbers of chert flakes.

Once the form and the function of stone tools were separated theoretically, new procedures were required to deal with each one independently. Replication studies lead to both the experimental use of tools and to the reduction processes that produced them. Many people, following the pioneering work of Crabtree (1972), have replicated known tool types in order to determine what they could be used for and how they could be produced. Only by examining the whole process of producing flake tools can the archaeologist become acquainted with the full range of waste flakes and end products that stone tool production processes create. I will exploit the products of replication experiments in chapters seven and eight while examining the obsidian assemblages.

Schick's experiments (1991) are an example of replication where the reduction process was repeated multiple times in order to determine size variation in an average primary production process. Working with million-plus year-old sites in Africa, she could assume that a primary reduction process was the only form of knapping to occur. These data allowed Schick to identify sites where the original reduction process remained intact by comparison of waste flake size profiles. In other words, when she uncovered a site with a similar waste flake size distribution as the experimental one, she could conclude that post-depositional processes had not affected the site. The approach of Schick is duplicated with modifications in chapter seven.

An example of what could be termed "applied replication study" reveals the intrinsic importance of the data derived from these types of experiments. Torrence's work on Melos (1984, 1986) used the waste flake counts to reveal that specialist obsidian working did not exist at Phylakopi throughout the several hundred years that the Melosian obsidian resources were exploited. That is, there was not enough of the waste material to have resulted from a specialist industry. Instead she had to conclude that casual visitors to the island exploited the obsidian resource while there on other business and that the local population did not exert a profitable control over the natural resource. Without a good idea of the amount of waste material produced during tool production, it would have been impossible to make conclusions about Phylakopi.

Replication often goes hand-in-hand with use-wear experiments since you have to have the tools made before you can try to use them. The tools also have to be morphologically appropriate to the proposed uses, which requires experience in the replication process. My Master's thesis (Hanslip 1991) is an example of this, where I made several similar scraping tools in order to test the time dependency of use-wear development. One piece of flint was reduced into several end scrapers of similar size and morphology. Three of these were chosen for their overall similarity and put to the tasks of scraping a soft, a medium, and a hard material respectively. At set times the tools were cleaned and examined under high magnification, complete with numerous photographs to document the development of use-wear traces. This continued until the use-wear traces did not change through three consecutive sessions. A development curve was produced for use-wear on each tool. Only by producing similar working edges on the three scraping tools were the results of the experiments valid.

Source allocation studies on stone tools using geological techniques is another analytic method employed on lithic assemblages. Some regions are stone-rich, and knowing where each particular piece of stone came from may not reveal much about cultural activities. In regions with isolated or rare stone sources, identifying the source of each piece of stone demonstrates a network of contacts between areas (Ambrose, Duerden and Bird 1981, Torrence et al. 1992) and has implications for trade as discussed in chapter three. Source determination is the topic of chapter six.

The island setting of Melanesia is perfect for exploitation of source allocation studies. Previously they have been put to excellent use in studies of regions of a continent. A lot of work in the American Southwest (to name one region) was done taking advantage of the fact that artificial and natural boundaries on study areas can work equally well (see Earle and Ericson 1977, Ericson and Earle 1982). A specific example is provided by Ericson's doctoral thesis (1977) where the second section is of interest.

By linking together sites containing common sources of obsidian, Ericson defined ten unique regional exchange networks in the state of California. He was able to derive three variables that control for most of the variation in these networks: the location of the obsidian sources, the trails available to connect settlements, and the linguistic regional boundaries. Population numbers were found to be a key factor in the development and growth of exchange systems in California. In the two test cases that crossed the Sierra mountain range (the Bodie Hills and Casa Diablo systems) he found similar patterning in the way regional population growth was mirrored in the increasing extension and intensity of the obsidian exchange network.

Use-wear studies are another avenue of inquiry for stone pieces. Beginning with relatively simple microscopic examinations (Semenov 1964, Keeley 1980), use-wear practice has developed and diverged to satisfy numerous specific questions regarding material or area of interest (Richards 1988, Sussman 1988, Grace 1989, Hurcombe 1992). Use-wear is dealt with in detail in chapter eight.

As part of a multi-disciplinary study of a single site in Britain (Healey et al. 1992), Grace exploited the idea of toolkits presented above but instead of using typology to derive the function of each tool, he used use-wear examination. The typological types rely on an *etic* interpretation of the function of each tool in an assemblage, an interpretation that may not reflect the actual use of the piece of stone (Grace 1989, see also chapter eight, "Gravers"). The use-wear study, however, identified unused, single-use and multiple-use flakes and tools by the investigation of every piece in the assemblage. The resulting picture is different because typology classified simple flakes as waste products where use-wear discovered that many of these flakes were used in some identifiable manner. The tool-use profile for the site was compared to several theoretical ones (base-camp, kill site, hunting camp etc. as introduced above) and found to differ least from the profile developed for a moderate-term settlement. This conclusion was not contradicted by any of the other archaeological evidence at the site.

The *chaînes opératoires* approach to lithic analysis relies on a solid understanding of the physical properties of the assemblage before looking to determine the function of a tool in that society (Pelegrin, Karlin and Bodu 1988, Grace n.d.). As the name implies, this method relies on linking actions performed on, or with, the stone from original extraction from the environment, through production, use, curation and even discard in a continuous chain or cycle. Grace contrasts this specifically with the methods of typology, where only the final product is considered and the process is not considered. Unlike the methods outlined above which really can be incorporated into numerous research designs, this is not so much a method as a new paradigm for dealing with lithic materials. *Chaînes opératoires* does not dictate a particular methodology, but an holistic approach to piecing together the results.

An example of this process comes from the work of Grace (n.d.) on two small sites in Norway. The choices that are made at each step of the cycle reflect the culture of the one making the choice. Similar tools in different locations are therefore a clue to a shared culture in both places. Were these two coastal Norwegian sites, separated by thousands of years in time, as alike as they seemed superficially? As a first step, Grace established that the tool types did not correlate with environmental or social factors, leaving personal choice responsible for them. He then looked at the choice of raw material. There were several to pick from, and they clearly differed in their efficacy for certain tool forms, so the question of whether choice of material was free or dictated by use was dependent on the later analytic steps. Raw material procurement strategy was found to differ between the two sites and evidence from contemporary sites suggested that this was a result of someone's choice.

Reduction processes were virtually identical between the two sites, lending strength to the notion that raw material use was a free choice. The results of the use-wear study indicated that the differences in the two sites were not a result of choice, but of site function. Finally the discard patterns agreed with the conclusions of the use-wear examination, that differences in the two sites were dependent on site function. Grace (n.d.) concluded that the differences in the two sites reflected a change in lifestyle that accompanied an increase in population between the era of the earlier site and that of the latter site. No single analysis employed could secure this conclusion—only the combination of all stages into a "lithic life-cycle" could do this.

We know from studies in other parts of the world that stone "tool kits", if examined as part of a risk management strategy, can have some of their variability accounted for (including even apparently anomalous periods of prehistory) through the timing and severity of risks important to the existence of that group (Torrence 1989b). Risk is specifically defined as the chance of not being able to procure needed resources such as food. Thus, risk management is a specific way of examining resource acquisition habits in a culture group.

Looked at in another way, risk reduction strategies may account for not only the composition of assemblages recovered but for the initial appearance of specific tool types or tool functions in a region, in response to new economic exigencies. Stone tool production would only be one part of a response to perceived risk that also includes formation of new cultural alliances, changes in group mobility, and alterations in other technological areas such as food production and storage (Hiscock 1994). Like *chaînes opératoires*, using a risk assessment approach is not a specific method but a framework to fit the results within. As such it can be used to reinterpret existing results as successfully as to format research design.

One example concerns the appearance of new tool types in Australia during the mid-Holocene period. Prior to approximately 4500 years ago, Australian lithic archaeology is characterised by simple flake production. Then, at roughly the same time across the continent, the retouched point, the backed blade and the tula (the three comprising the Australian Small Tool Tradition) appear in the deposits. Associated with the arrival of the dingo and new plant processing technology, the traditional explanation was that these new items were the result of cultural contact with Southeast Asian peoples (Hiscock 1994).

The risk management view is different. Environmental changes during this period forced people into new, more mobile settlement patterns. High mobility is a risk management adaptation, but this in turn requires a more robust toolkit of greater flexibility and reliability than does a low mobility lifestyle. Careful dating of the appearance of these various new tools and technologies places their introduction across a span of thousands of years, and not all at once (and therefore the result of local innovation rather than wholesale borrowing). Finally, risk management theory suggests that risks and risk reduction behaviour should covary. The disappearance of portions of the Small Tool Tradition toolkit across Australia coincide with greater sedentism amongst Aboriginal populations in the more recent past (Hiscock 1994). Risk management provides a comprehensively more compelling explanation to developments in Holocene Australia than does the alternative view (which originally developed from the typology obsession as mentioned above).

This section on history and theory, particularly chapters three and four, may have read more like a string of methodological examples than a typical theoretical discussion. Returning to Odell's (1996) practical definitions of theory regarding the study of stone tools introduced in chapter two, these examples should be viewed as a combination of archaeological history and their contained theoretical bases. I did not start from a position of a specific theoretical stance (such as a Marxist, Feminist or Structuralist might), nor did I have a specific repertoire of methods committed to the research. Rather I knew that there was a realm of investigative procedures that might be applicable to the assemblages available, and that beginning with general questions about these lithics I might learn new information about the people that made and used them.

As this chapter reveals, there are a multitude of methods that can be applied to stone tools. Whatever the methodological or theoretical basis of any study of stone tools, it is only logical that one should approach the research with an open mind and incorporate as much as possible of what is known about the background of the lithics. Ultimately lithic artefacts should be linked to subsistence, exchange, settlement patterns and ideology by this research (Fullagar 1994), rather than studying the stone as an end in itself. Obviously one cannot try every procedure on every piece in every assemblage. This cultural linkage requires careful thought and planning in research design. Innovation and creativity are required to maximise the informational return from stone tools. The traditional recipe book approaches generally ignore extant knowledge and fail to exploit the unique nature of every research project (Hiscock and Clarkson 2000).

§5

Obsidian-bearing sites of Melanesia

This chapter begins the presentation of data integral to the research project. After a brief review of the obsidian bearing sites in Melanesia, the specifics of each of the three categories of sites used in this research are reviewed. The primary sites, those for which obsidian was available and fully investigated (tables 5.1 & 5.2) are discussed in some detail with regards to dating and stratigraphy. Next are the secondary sites, those for which obsidian was available but complete investigations were not completed and which are only briefly discussed. Finally a listing of tertiary sites is provided. These sites are ones for which only published data were utilised.

Radiocarbon dates were essential to determining the dating of portions of the excavation for most of the primary sites. Those dates presented in table 5.3 are calibrated at one standard deviation. In reference to these specific dates the use of the abbreviation 'bp' implies calibration. By association, so too are the endpoints of the five periods used to divide up the chronology of Melanesia based on one sigma calibration. Elsewhere in the text (for example in the preceding discussion on the history of Melanesian archaeology), date references which are general approximations lack the 'bp' reference.

Also worth noting at this point is that some radiocarbon dates are more secure than others. Individual excavators have relied on specific ones in preference to other, anomalous, ones. I have listed all of the dates available both in table 5.3 and in the appropriate illustration. On the basis of information from the site reports and the excavating archaeologist, certain dates have been interpreted as to which period they fall in. This reasoning is explained in the text, but the outcome is implicit in the illustrations. Dates and layers are colour coded with similar colours belonging together: period one = tan; period two = purple; period three = blue; period four = red; period five = green. Date #1 for example spans the boundary of periods three and four. The assignment of Zone C1 to period three is apparent from the colour scheme in the illustration, and necessary because of non-obsidian materials in that layer linking deposits with the earlier period. Another example from the same site, date #5, associates the bottom of Zone C2 with period two. This date is of unclear association with any cultural material and therefore has no influence on the assignments at site SAC.

Archaeological sites that contain obsidian occur in virtually all archipelagoes of Melanesia. Their distribution, however, is quite discrete. This is mostly attributable to the history of archaeology in the area. The islands that have received the most attention by archaeologists are generally those with the most discovered sites, and those at the western end of Melanesia (closer to the two main obsidian source areas of the Admiralty Islands and West New Britain) tend to be much more obsidian-rich than those at the eastern end of Melanesia. So while the general trend in settlement history is well known, the fine details are subject to change with further investigations (Spriggs 1990, 1997).

Despite almost all sites containing obsidian from more than one source (particularly removed from the immediate region of each particular source), there is a trend towards finding obsidian from Admiralties and West New Britain sources in particular locales. Throughout the Admiralties (Gosden et al. 1989), on the large island of Manus (Kennedy et al. 1991) and many of the small islands nearby such as Ahus (Kennedy 1981), Andra (McEldowney 1994) and Mouk (McEldowney and Ballard 1991), the local obsidian is the dominant one. To the west, Admiralties obsidian is found along the New Guinea coast and is the dominant source in certain locations (Terrell and Welsch 1997). Obsidian also travelled east across the ocean gap to the Saint Matthias group (Kirch 1987, 1988b, Kirch et al. 1991), and on to many of the small islands off the east coast of New Ireland, including the Tabar, Lihir and Tanga groups (Ambrose 1978), Ambitle (Ambrose 1973) and Nissan (Spriggs 1991). Finally Admiralties obsidian is almost completely dominant around Buka at the northern end of the Solomons (Wickler 1995).

West New Britain obsidian (primarily Talasea sources, but Mopir was included when it was available; Fullagar et al. 1991) is the dominant source immediately to the south of those locations dominated by Admiralties obsidian, beginning far to the west of New Britain and extending south-east to the Reef Islands. Talasea obsidian also travelled west along the north coast of New Guinea (Ambrose 1976a, Swadling 1990: 81) and at least as far as Borneo (Bellwood and Koon 1989). From the Arawe Islands off the south-west coast (Gosden 1991b), across the island of New Britain (Specht and Koettig 1981, Ambrose and Gosden 1991) to Watom Island off the north-east coast (Green 1989) and into the Duke of York Islands (Lilley 1991b, White 1997), the more local source logically predominates in this region. At the far south-eastern end of the Solomons, in the Reef Islands and on Santa Cruz, several sites have been discovered with primarily Talasea obsidian in them (McCoy and Cleghorn 1988, "Reef/Santa Cruz islands" section, this chapter).

The Southern Solomons are dominated by Talasea obsidian, while the north end of the archipelago is dominated by Admiralties obsidian. The bulk of the Solomon Islands generally lack obsidian bearing sites (Swadling 1996) with the exceptions of some obsidian on Choiseul (Miller 1979) and New Georgia (Reeve 1989). Due to both the scarcity of obsidian and the lack of source attributions for what has been found, the Solomons do not come under the distribution range of either source. New Ireland has not been included in the above either. Crossing both the northern and southern obsidian distribution regions, there is no one dominant source in New Ireland (Allen, Gosden and White 1989, Gorecki, Bassett and Head 1991, Gosden and Robertson 1991, Marshall and Allen 1991). Obviously this description only applies to the period since the arrival of Lapita peoples and the concomitant introduction of Admiralties obsidian outside the Admiralty Islands.

While this constitutes the major range of obsidian distribution in Melanesia for the two main source areas, the distribution of obsidian from source to site is not clear cut (varying through time and by site; this is specifically addressed in the chapter on sourcing of obsidian) and not limited to the islands mentioned above. Obsidian is now known across the full length of Melanesia, having been found on Fiji, Vanuatu and New Caledonia (Best 1987). At this time obsidian from the Bismarcks is not known to occur anywhere in Polynesia, although they did exploit localised sources of volcanic glasses (Kirch 1981). A single obsidian point from a Bismarck source was found on Pohnpei in Micronesia (Schurig 1930: 7, Ayres and Mauricio 1987).

Of lesser importance in terms of quantity of obsidian distributed into the region of interest are the two source areas of Fergusson Island in the D'Entrecasteaux group and the Banks Islands in Northern Vanuatu. Fergusson obsidian was an important commodity in the region closer to that source (Egloff 1979: 99). Similarly, the Banks Islands are an important source area but even in the case of the Reef/Santa Cruz group (much closer to the Banks than any other source) only a small portion of flaked material recovered there is from Banks Islands sources. Chapter six considers this issue in more depth.

In Melanesia, with at least a forty millennia history of human occupation, one of the major cultural changes occurred with the appearance of Lapita culture around thirty-five hundred years ago. The archaeological work in this area goes back about thirty years. The sites described in this chapter (fig 5.1, table 5.1) represent a large portion of the excavated sites that contain obsidian flakes.

In order to try to put the artefact remains from these sites into a framework allowing direct comparison, I have divided the chronological continuum into a series of discrete periods. These units were based not only on regional culture history, but on the spread of existing data as well.

I decided on five time segments (fig 5.2). The oldest, period one, is the 'pre-Lapita' period. This is an all-encompassing period that covers everything before the arrival of Lapita cultural markers in each site, and therefore does not represent a specific historical date at its beginning. It starts with the earliest pre-Lapita levels in a particular site (nothing earlier than c. 15000 bp in this study), and ends with the appearance of Lapita levels (which can be as early as c. 3500 years ago, but are not necessarily this early).

Logically, the next time division begins with the appearance of Lapita cultural markers; the presence of pig remains, the distinctive pottery decoration, and outside the Admiralties by Admiralty Islands derived obsidian pieces. Period two, the 'Lapita' period, as mentioned above, begins around 3500 years ago, but sampling error, island differences, etc. can vary the actual beginning of this phase for each site. The end of this unit is set at 2700 bp because this time marks the end of the 'classic' Lapita period in most areas. After this date, the Lapita Cultural Complex as a pan-regional entity is no longer recognisable, with only some isolated groups possibly still producing Lapita style pottery (Kirch 1997).

The next time unit, period three, separates the post-Lapita segment from later-still cultural groups. Much of the influence of Lapita still existed post-2700, either in the form of late-Lapita culture groups or in their immediate successors. By 2000 bp, however, this was no longer true. So this third period is 2700–2000 bp. The final division does not rely on cultural events as clearly as the earlier ones. Different scholars have divided up this period of the recent past in ways appropriate to their specific interest. Summerhayes et al. (1998) separate the most recent fifteen hundred years from the period before on the basis of ceramic production. Spriggs (1997) argues for a division around seven hundred years ago when he considers Melanesian prehistory in the widest view. In my case, I did not have pre-conceived notions of which direction this division should go so I chose "arbitrary spits" as a separator. On the basis of the material at hand the remaining two millennia can be simply divided into halves with a 2000–1000 bp segment (period four) and a 1000–0 bp segment (period five). I have specifically refrained from using more descriptive terms for each time period because of the potential bias many of these terms might introduce to the analyses.

I have employed a shorthand nomenclature in referring to the time periods within the sites throughout this thesis. A terminal suffix when appended to the site code reference refers to only that period of the particular site. For example, the Dori site on New Ireland has site code ELS. When I refer to the obsidian flakes from period three at this site, it is written as ELS-3. This should not be confused with numbers that are part of the site code designation, such as EAB1 (the Balof 1 cave site on New Ireland) or RF2 (the Nenumbo site on Ngangaua in the Reef/Santa Cruz group), both of which still take a period designation suffix (EAB1-1, or RF2-2).

WATOM ISLAND

Three sites on Watom Island provide a large portion of the obsidian available for this research by virtue of their large size and abundance of obsidian. Watom island is located twelve kilometres north of Rabaul, New Britain and is a small island (eight square kilometres area) typical of 1960s era archaeological finds of Lapita materials.

Jim Specht excavated in 1965–66 at two sites (Specht 1967a, 1967b). Both sites are located in the area of the Reber Mission (Rakival village) where Lapita decorated pottery was first discovered. Site SAD (also known as site six, site FAD, and the Maravot site) is located at the south end of the village area where streams, waves and a World War II trench have disturbed the soil (Specht 1968). Nine trenches totalling an area of seventy square metres were dug in a line cutting inland from the beach. Deposits varied in depth between trenches, with the full sequence not represented in any single trench.

The four trenches numbered three, four, five and nine were the most highly disturbed. In these trenches, the stratigraphic assignments were made on tentative grounds by Specht, so the usefulness of the artefacts must be considered carefully. With all the potential for problems at this site, I have decided to err on the side of caution (easier to do with site SAD when there are so many flakes available) and not use the obsidian from these uncertain pits.

Trenches six, seven and eight displayed moderate disturbance, with some obvious reworking of the ash layers. It was not extensive, and the obsidian from these units can be provenienced well enough to assign a date to it. Finally, trenches number one and two were virtually undisturbed.

In such a highly volcanic area and with the reworking of the sediments, there are many distinct ash layers in the site. Specht designated each layer as belonging to one of four cultural zones. The majority of the ash at the site is from the largest eruption of the Rabaul volcano of the past 3500 years (c. 1400 bp, also dated by radiocarbon date #7, see table 5.3). This ash layer seals in all of site SAD. The earliest deposits of zone four are full of Lapita decorated potsherds, putting the whole of site SAD between 3500 bp and 1400 bp, but not necessarily covering that entire span.

On the basis of decoration style, zone four was assigned to a terminal Lapita era occupation, which for Watom falls in period three (Specht 1993). Zone three deposits which Specht believes were laid down after a time gap from zone four are in a redeposited tephra from the slope behind the site and of probable post-Lapita context. As it turns out, the removal of the four trenches from the assemblage also removed all the zone three finds, so the exact dating of this layer is immaterial, except for how it relates to zone two. Zones one and two are clearly post-zone three, but before the Rabaul eruption, and therefore in time period four. This stratigraphy is presented in figure 5.3.

Trench one is all zone one. Trench two contained a mixing of zones one and two, with zone three at the lowest level (there was no obsidian in zone three in this trench). Trench three had all its artefacts in the probable zone four layer. Trench four had a few pieces of obsidian from each of zones one, two and three, and much obsidian in the probably-zone-four layer. Trench five lacked any zone one deposit, and had a large quantity of obsidian in the probably-zone-four layer. Trench six and trench seven had an intermixed zone one and two layer, with some obsidian, and much more was found in zone four. Trenches eight and nine only had obsidian in zone four (what was "probably-zone-four" in the case of trench nine).

With all the problems at this site, Specht turned his attention to other, less disturbed ones, and subsequently only fifty-seven flakes were sourced. These comprised eight Mopir, thirty-three Admiralties, and fifteen Talasea flakes.

On the same village grounds Specht also excavated site SAC (originally site eight, also site FAC and Kainapirina site) in the same season as site SAD. Located away from the activity of the stream that affected the other site, the two trenches dug here (total area sixteen square metres) contained an intact stratigraphy. Several burials were found at the bottom of trench number one (which was not excavated to the base at that time).

The potential value of those burials prompted Green and Anson to continue excavations at this site in 1985 (Green and Anson 1987, Green 1989, Green, Anson and Specht

1989, Green and Anson 1991). They completed trench one and extended it on two sides (a total of twenty-eight square metres more area) to expose the burials crossing the initial trench.

Specht designated the three main layers at site SAC as zones A, B, and C. All of the obsidian was found (along with the Lapita decorated pottery) in the approximately fifty centimetres of zone C, underlying nearly a metre of zones A and B. The bottom layer was subdivided into C1 and C2 on the basis of a colour change in the soil. The four dates (#1–#4) suggest that both layers belong to the same time division; period three. Site SAC stratigraphy is illustrated in figure 5.4.

About 1075 of the 2328 pieces of obsidian from SAC have been sourced (yielding a ratio of about 50:50 for the Talasea and Admiralties sources). As at site SAD, the Mopir source shows up in site SAC, primarily in the earliest levels (with fewer than twenty examples). Site SAC contained only three worked pieces of stone, in addition to a large volume of rock used to build structures (this rock was not tabulated in the report).

Finally, Green and Anson dug four square metres at a location on the north edge of the Rakival village site during the 1985 field season at SAC. Designated as site SDI (also known as the Vunuvuang site), the one and a half metres of cultural deposits (underneath two metres of ash) had four clear layers, the lower three of which were dated. This stratigraphy is illustrated in figure 5.5.

Roughly a thousand years of prehistory is represented by site SDI deposits. Layer four contains Lapita markers and one date (#11); therefore period two. Layer three falls into period three (date #10) with layer two in period four (date #9). Based on the pottery found in layer one, Green and Anson place it in period four as well. There are only forty-six pieces of obsidian from this site, with forty-one from period four. Forty-four of these are from Talasea, with two from the Admiralties. Site SDI only contained four pieces of non-obsidian rock.

BALOF CAVE, NEW IRELAND

There are two rock shelter sites as part of a complex known as Balof, located near the north-east coast of New Ireland; situated about twenty metres apart, at an altitude of eighty metres and 2.7 kilometres from the coast. The first site excavated is known as Balof 1 (site designation EAB/1) and the second site is known as Balof 2 (site designation EAB/2). While Lapita period sites are most often discovered on the ancient beach of a small island, the Balof site is in a cave; an expected location for a Lapita period site on a larger island and the setting for most of the known Pleistocene deposits is Melanesia. Situated in a cave it is away from locations where the potential exists for subsequent cultural (primarily gardening) and geological (primarily subsidence) activities to have obscured any remains.

Balof 1 has a floor area of nine metres by four metres. In 1969 Peter White excavated six square metres here, seventeen per cent of total floor area, in arbitrary ten centimetre spits to a depth of eighty centimetres (Downie and White 1978). Another three square metres were excavated in 1988 (again by White), this time to one hundred and twenty centimetre depth (the earlier excavation hit the rock floor of the cave) (White et al. 1991, J.P. White pers. comm.). A total of four radiocarbon dates (see table 5.3 for all the radiocarbon dates used in this chapter) were produced for the site, only two of which have been published (Downie and White 1978, J.P. White pers. comm.). Figure 5.6 displays the stratigraphy and the two published dates from this site.

Obsidian is found in nearly every spit, with a total number of eighty-five pieces. To assign each flake to a time segment requires some interpolation. Pottery appears at spit five, and pig remains appear at spit four. Spits six and below (date #13) are therefore period one (being deposited prior to the markers for Lapita). Spits two and three, on the basis of the ~1500 bp date (#12, which spans the juncture of these levels), are period four. Spit one is period five on the basis of the recent date (unpublished) produced at this level. We know that Lapita markers appear in spit five in the early third millennium bp. Given that spit four is between spit five (at about 2800 bp in period two on the basis of the other unpublished date) and spit three (at about 1500 bp in period four; date #12) I have assigned spit four to period three.

There are some interesting details to be gleaned from the 1969 excavation. The appearance of Admiralties obsidian in spit five coincides with the disappearance of New Britain chert (which was found only in levels six to eight). The sourcing of the obsidian shows that supply is split between Talasea and Admiralties sources, with the exception of spits six and seven, which are Talasea sources only (White, Downie and Ambrose 1978). A decrease in absolute amounts of stone occurs at the spit six to spit five transition as well (by an order of magnitude decrease—from hundreds of grams to tens of grams per excavated level). Clearly the arrival of Lapita culture on New Ireland coincides with a marked shift in the deposition at the site.

Overall, obsidian represents only 2.8% by mass of the total and is a minor component of the rock excavated at the site. New Ireland rocks (from sources ranging from the adjacent cave wall to locales half way down the island) account for ninety-one per cent of all the stone. Almost all of the flaked stone comes from the nine per cent imported rock. Those sites containing large amounts of local rock are similar to Balof in this distribution.

Balof 2 has a floor area of fourteen metres by seventeen metres. White excavated a total of 8.25 square metres (three per cent) in three visits (1985, 1988, & 1990) (White et al. 1991). Only the 1985 excavation has been published, but the obsidian from this season has gone missing and therefore was unavailable for examination.

There should be enough information about this site from the 1985 report to use the stone from the subsequent excavations for at least some of the procedures in this investigation. Five dates and the natural stratigraphy assisted here. Sixteen spits (ten centimetres where natural stratigraphy allowed) were later grouped into eight horizons on the basis of their internal consistency.

While there appears to be some minor disturbance in horizons five and six, with an apparently older date higher up (#16 & #17), all obsidian from horizons three through eight (dates #15–#18) is clearly period one in age. Horizon two, at ~3100 bp (#14), is period two. Horizon one flakes have been excluded because this twenty centimetre layer covers 2500 years and three time periods. While twenty-five flakes from Balof 2 were available for study, the confusion surrounding the spits which made up each horizon prevented accurate provenience for any of the obsidian and precluded its use in some parts of the study.

From the 1985 excavation data only, we know that less than eight per cent of the total mass of recovered rocks are exotic in origin, and only 0.2% of the total are obsidian. The remainder of this imported rock is a fine grained stone that disappears in horizon three, where the obsidian appears for the first time. It should not be surprising then that obsidian occurs in such small numbers at a site with a long history because much of it contains no obsidian pieces. Unlike at Balof 1, there is no change in total rock abundance across the horizon boundaries.

LASIGI VILLAGE, NEW IRELAND

There are two sites located in the area around Lasigi village. This village is located one hundred and fifty kilometres south of Kavieng, on the coast of New Ireland. The first of these sites, called the Dori site (site code ELS) is a mound ten metres by twenty metres in area. A test trench was placed through the mound (one metre by five metres dimension, three per cent) by Jack Golson in 1985 as part of the Lapita Homeland Project (Golson 1991).

Deposits range in depth from seventy centimetres to one hundred and twenty centimetres along the length of the trench, and deeper where the burials intrude on the sterile sand beneath. Excavation was made in ten centimetre arbitrary spits and natural levels. Golson divides the site into five phases dated with six radiocarbon samples. Figure 5.7 displays the stratigraphy, the dating, and the assignment of layers into time periods for this site.

Golson's phase one (date #24) is the earliest period at this site, falling in my period two, and dating well before phase two. Phases two, three and four would seem to be contemporary on the dating evidence (dates #19–#23), and fall in my period three. Finally phase five is full of historic artefacts and therefore of period five.

There is an order of magnitude increase in the mass of obsidian per cubic metre of excavated soil recovered in phase three and four compared to phase one and two. What is referred to by Golson as "other stone" (including a few fragments of ground stone tools) represents about ninety-nine per cent of the total stone recovered at the Dori site, but the changes in obsidian density are reflected in the change in total stone density for each phase. All the obsidian in my collection is from phase four (period three).

The other site at Lasigi village is called the Mission site due to its location in the yard of the village mission. Designated site ELT, it was excavated in 1985 by Andrée Rosenfeld for Jack Golson. Only a two square metre test pit was made to determine the archaeological potential in this area.

Deposits were no more than fifty centimetres deep, with no internal differentiation of stratigraphy. Five centimetre spits were dug to control for vertical distribution of the artefacts. On the basis of artefact density, four phases were distinguished for site ELT. Only one date (#25) is available for the site, coming from the top of phase three.

This phase, the only dated phase, falls at the end of period three. It is unlikely that phase two was significantly earlier than phase three on the basis of the internal artefacts and the lack of stratigraphic distinction, so phase two is also assigned to period three. J. Golson (pers. comm.) noted that it was quite possible that both phases one and four (especially four) could be the results of artefact movement from the neighbouring phases, perhaps as a result of local gardening activity. Both phases are a lower density sample of the adjacent phase. Thus I will classify all four phases at site ELT into the same time frame.

Peter White excavated at Lesu (or Lossu), a village nearby to Lasigi on New Ireland. Both villages contain several midden mounds and overall the excavation at Lesu is comparable to that at Lasigi. Available dating suggests the sites are contemporary. The pottery found is similar in decoration and fabric. Obsidian finds reportedly compare well also (the Lesu obsidian was not available to me), with small pieces being the norm and Admiralties sources representing all forty-three pieces from the Mission site, sixty of sixty-five from the Dori site, and "the majority" from Lesu. The Lesu site did produce two formal tools of a sort often seen on Lou Island and unlike anything seen at Lasigi (and rarely elsewhere outside the Admiralties for that matter) (White and Downie 1980, Golson 1991, 1992).

NISSAN ISLAND

To the east of New Ireland lies the atoll of Nissan. Fairly large for an atoll (at twenty-eight square kilometres), at least seventy-seven sites are known on it. In 1985, 1986, and 1987 Matthew Spriggs excavated six sites and collected from three reef-top artefact scatters (Spriggs 1991).

Site DHH consists solely of a large block of obsidian that was dropped on the reef long enough ago for the reef to have grown up around it. Based on the visible portion, this piece of obsidian (source from Lou Island, in the Admiralties) is about one and a half litres in volume. It is possible that this is indicative of the way obsidian was transported around Melanesia in the past, as large blocks.

Two other reef-top scatters of artefacts, known as sites DES (the Tarmon site) and DIZ (the Warom reef site), contained obsidian and pottery. Direct dating is impossible for these scatter sites, but the pottery associated with each one allows for relative dating in comparison to other sites. Site DES (area approximately one half hectare) produced only nine flakes interspersed with Lapita and post-Lapita styles of pottery. All obsidian was from Admiralties sources. Site DIZ (area approximately one hectare) is much more recent, on the basis of its potsherd decoration. The seventeen flakes (two from Talasea, the rest from the Admiralties) belong to the most recent millennium.

Three excavations were made in different rock shelters of a dissolution cave network known as the Yomining complex. Site DGD/1 produced no obsidian in the excavation. Site DGD/2 had a total area of six square metres dug from the thirty-five square metre floor area (seventeen per cent). Deposits above eighty centimetres in depth are clearly from a post-Lapita period. The level between sixty and eighty centimetres is of interest because it contains deposits of the immediate Lapita successor on Nissan, named for this cave complex (the Yomining phase). Lapita deposits occur from eighty centimetres to nearly two metres depth.

The eleven radiocarbon dates place the obsidian from each level in a secure time frame. The earliest dates (#47–#51) place the deposits containing Lapita artefacts in period two. The 90–100 cm depth date (#46) and the early part of the Yomining phase (dates #44 & #45) fit into period three. The final dates (#41–#43), from the end of the Yomining phase, fit into period four. The relationship of the layers and these dates is illustrated in figure 5.8.

Overall, about twenty-five per cent of the obsidian sources to the Talasea area (with the rest from the Admiralties), but the Talasea contribution to the obsidian deposit decreases with time. As at all the Nissan sites there was negligible non-obsidian stone recovered during the excavation. On an atoll all stone would be an imported good, so it is logical that the amount of other stone present would be low. A large amount of the stone found in many sites, such as the Balof cave shelters, is composed of large, low value rock used for oven stones and can be replaced in an atoll environment with pieces of coral.

Site DGD/3 had only one square metre of its twenty-six square metre area (four per cent) dug in a test pit. About two metres deep, all of site DGD/3 is period five based on the one date (#52) from one hundred and eighty centimetres depth and the pottery style. Two of the three flakes of obsidian are from the Admiralty Islands, with the other from Talasea area.

Site DGW (site Lebang Tatale) is another deeply stratified cave site. Three square metres were dug from the approximately eight square metre floor area. The 3.3 metres depth covers pre-Lapita to recent deposits as shown by the five dates displayed in figure 5.9. Below one hundred and ninety centimetres (dates #56–#62) are Halika phase deposits (aceramic and pre-Lapita; period one) with the two pieces of obsidian recovered coming from Talasea sources. Between one hundred and thirty and one hundred and ninety centimetres depth (dates #54 & #55) lies Yomining phase deposits, which at this site seem to be confined to period four. Everything above one hundred and thirty centimetres depth (date #53) is period five. All the Yomining phase obsidian (forty-one pieces) is from Admiralties sources. Two of the ten more recent flakes are from Talasea, the rest from the Admiralty Islands.

Site DFV (Lebang Takoroi) is the earliest known site on Nissan, and lent its name to the first occupation phase for the atoll (the Takoroi phase). Another cave site, seven square metres of the fifteen metre by twelve metre area (four per cent) have been excavated (see fig 5.10). Modern materials were found to a depth of approximately twenty centimetres; the rest is Takoroi phase deposits according to several dates (#36–#40), but portions of this site have been disturbed.

There are sixty-two flakes from site DFV, of which forty-two are from clearly pre-ceramic deposits (all Talasea derived), while there are twenty that, according to Spriggs, may have been disturbed upwards or may belong to the upper occupation level. On the basis of the source data (chapter five) the differences between the two groups of obsidian is significant enough that I placed the upper obsidian into period five. Mopir obsidian is found only in the lower levels, Admiralties obsidian only in the upper levels while the difference in mass between the Talasea obsidian of the lower and upper levels is more than 50%. This situation could be clarified via hydration dating of the upper level obsidians.

The final Nissan site I will discuss is site DFF (Lebang Halika). A two hundred square metre rock shelter that had a total of seven square metres excavated (less than four per cent), it lent its name to the aceramic Halika phase. Halika deposits were found below seventy centimetres (to the one hundred and forty centimetre base, dates #29–#35) and produced no obsidian. Lapita deposits were found between seventy centimetres and twenty centimetres (dates #26–#28). The top layer was of recent deposition and devoid of obsidian as well. There are ten carbon dates (#26–#35) for site DFF, which along with the stratigraphy are illustrated in figure 5.11. The pre-Lapita Halika phase ends by about 3400 bp. Lapita deposits are probably all pre-2700 bp. All of the obsidian is from Lapita period layers, and is derived about fifty per cent each from Admiralties and Talasea sources.

SOHANO ISLAND

As part of his PhD research, Steven Wickler recovered the obsidian and pottery from an extensive reef-top scatter (designated site DAF) along the coast of Sohano Island (Wickler and Spriggs 1988, Wickler 1990, 1995). The remainder of Wickler's other sites on Sohano, Pororan and Buka Islands, both excavations and surface collections, produced virtually no obsidian and are not included in this study. The DAF reef scatter, however, produced 341 flakes from the six hundred metre long distribution on top of the reef, the beach and the fore-dune. Four blades and one (possibly two) Manus-style triangular spear point (the best example described in Ambrose 1991 from the Emsin site) were found. Some of the pottery and obsidian appeared to be eroding out of the sand, however a test pit in the area revealed nothing.

The obsidian can be dated by the pottery decorations, and Wickler has assigned it a date of "very late Lapita culture", or mid-third millennium bp (in period three). With no excavation, there is no stratigraphy or excavation strategy, but Wickler did divide the area of collection into twelve parts, allowing him to identify the central area of deposition and the periphery of the site. The surf action removed the smallest component of the assemblage, but otherwise appears to differ little from an excavated assemblage. Virtually one hundred per cent of the pieces are from Admiralty Island sources.

REEF/SANTA CRUZ ISLANDS

Located at the southern end of the Solomon Islands chain is a group of islands known as the Reef Islands. In conjunction with the nearby large island of Santa Cruz (also known as Nendö), these islands are referred to as the Reef/Santa Cruz group.

The three Reef/Santa Cruz sites included here are unique in this study by having a major non-obsidian flaked tool component contemporary with the obsidian tools. None of the other sites have an alternative stone resource in competition with the obsidian. This may be due to the availability of a good chert source three hundred and fifty kilometres away coupled with the over seventeen hundred kilometre journey necessary to reach the obsidian sources (Green 1976). The Banks Islands obsidian is available with much less travel than the Bismarcks obsidian but this source was apparently never heavily exploited.

Peter Sheppard did a comparative study of the two raw materials; comprising 972 pieces of obsidian and 395 pieces of chert, which determined that the chert increasingly replaces the obsidian through time (though never replacing it completely), and is essentially a direct replacement for the obsidian in terms of flake technology (Sheppard 1992, 1993).

Site SE-RF-2 (also SE-RL-2, the Nenumbo site, in some references, on Ngangaua island) is the largest excavation in this study (Green 1986, Sheppard and Green 1991). Occupying over one hectare, the central artefact-rich area was the focus of excavations (one hundred and fifty-four square metres total; fourteen per cent of the site). Extensive spatial analysis of the artefacts across the site reveal a probable structure which had the densest concentration of lithic materials (and pottery, but not shell or bone) inside its walls.

The main impact of that study on this thesis is the knowledge of how the obsidian recovered represents a sample of the whole site. Furthermore, comparisons between the three Reef/Santa Cruz sites allow extrapolation to the other two less extensively excavated and analysed sites. Thus the potential for sampling errors to induce false conclusions, that is so high for many other sites (which were only test-pit excavated), is reduced. Chronological trends between these three sites can be securely considered as cultural rather than as a consequence of sampling; for example that the chert becomes more important through time (Green 1985). In other words, the data from the Reef/Santa Cruz group are safe to exploit despite there being only three sites.

SE-RF-2 is, like the other two Reef/Santa Cruz sites, a short term occupation site (only fifty centimetres deep). The original site report had only a few dates (#63–#66), of uncertain security indicating an approximately three thousand year old site. On the basis of seriation of several artefact classes Green's final assessment (Green 1991b), that the site is just about three thousand years old, did not change.

Site SE-RF-6 (Ngamanie site, also site SE-RL-6, on Ngambelipa island) is ten times the overall size (approximately ten hectares) of site SE-RF-2, but less than one per cent of the area has been excavated. The sampling strategy allows extrapolation from the twenty square metres excavated to represent ten times that much. Site RF-6 had a deposit depth of forty centimetres, and produced two carbon dates (#67 & #68). These dates place the site right at a division in my time scale. On the basis of the dates being generally more recent than 2700, I will place this site in period three.

Located on Santa Cruz Island, site SE-SZ-8 covers similar area to site RF-6 (about fourteen hectares), and had fifty-one square metres excavated (less than one per cent), which Green extrapolates to nearly ten times that area. Site depth was fifty centimetres, carbon dates number only two but they agree closely with the pottery seriation evidence and therefore are quite secure. These dates (#69 & #70) place this site as earliest of the three in the Reef/Santa Cruz group, early in period two.

There were 619 obsidian flakes from site RF-2, and 353 chert flakes. From site RF-6 there were sixteen obsidian flakes and eleven chert flakes. There were 294 obsidian flakes, and thirty-one chert flakes from site SZ-8. All of these flakes were sourced, revealing that more than ninety-seven per cent of them came from Talasea sources. The remaining two per cent or so were divided evenly between Admiralties sources and Banks Islands sources, with one single flake linked to Fergusson Island (Green 1985, 1987, Green and Bird 1989).

SECONDARY SITES

The excavations from three islands constitute the secondary sites in this research. Secondary sites are those sites for which the obsidian was available, but for reasons discussed below were not used to the same extent as the primary sites. The location of eacn is displayed in figure 5.1.

In 1978, Ambrose and Kennedy excavated a one square metre test pit on Ahus Island, just off the north coast of Manus Island in the Admiralties (Kennedy 1981). The resulting lithic assemblage does not fit the profile for those sites mentioned above and therefore was not incorporated into this research. The main problem was the fact that all the flakes were stored together in a bag, rendering them quite useless for use-wear. Also the lithic material itself is composed almost entirely of knapping debris rather than the use-flakes themselves. It is, however, an obsidian-bearing Melanesian site and the assemblage proved invaluable as an independent test case during the examination of flake morphology.

Ambitle is located in the strait between New Ireland and Bougainville, north of Nissan atoll. Ambrose's excavation produced an assemblage of noticeably small flakes (Ambrose 1973, 1976a, 1978). I had intended for Ambitle to be a primary site in this research, but for several reasons unrelated to the assemblage itself it still had not been examined when the need for another collection arose in the morphology study. I exploited the obsidian for this purpose and never returned it to the use-wear examination.

The work of Kirch and Yen (1982) on Tikopia revealed many archaeological sites dating from the earliest occupation of this island. Located at the far south-east end of the Solomon Islands chain, Tikopia is the furthest island from the Bismarck sources in this study. While nearly one thousand pieces of volcanic glass were recovered in excavations on this island, only about ten of these were obsidian, the rest being a poorer quality material obtained from less distant sources in the Banks Islands. With all of the obsidian thin sectioned for hydration dating, it was impossible to add anything to the investigations presented in the Tikopia volume. It was possible to source the pieces again.

TERTIARY SITES

After I completed my analyses of these sites above, I incorporated the data from other sites in Island Melanesia. These tertiary sites help to place the sites in this thesis into the larger context of Island Melanesia. While all the sites are itemised later on when these data are used, at this point I will list the islands involved (figure 5.12) and tabulate the sites (table 5.4). Sites are on New Britain itself as well as in the nearby Arawe Islands and on Garua Island. To the east of New Britain are several sites in the Duke of York Islands. Many sites, especially cave sites, are known from the length of New Ireland. The Saint Matthias group off the north coast of New Ireland holds many excavated sites. The only site included from the Admiralties is located on Manus. Further to the east, there are sites from Sohano Island in the North Solomon Islands and from Santa Cruz in the South Solomon Islands.

Table 5.1.
Site coding for primary sites.

Register code	Abbreviation	Site name	Island
SAC	SAC	Kainapirina	Watom
SAD	SAD	Maravot	Watom
SDI	SDI	Vunuvuang	Watom
EAB/1	EAB1	Balof Cave locale 1	New Ireland
EAB/2	EAB2	Balof Cave locale 2	New Ireland
ELS	ELS	Dori (Lasigi)	New Ireland
ELT	ELT	Mission (Lasigi)	New Ireland
DES	DES	Tarmon Reef	Nissan
DFF	DFF	Lebang Halika	Nissan
DFV	DFV	Lebang Takoroi	Nissan
DGD/1	DGD1	Yomining Complex locale 1	Nissan
DGD/2	DGD2	Yomining Complex locale 2	Nissan
DGD/3	DGD3	Yomining Complex locale 3	Nissan
DGW	DGW	Lebang Tatale	Nissan
DHH	DHH	Tatarsikirion block	Nissan
DIZ	DIZ	Warom Reef	Nissan
DAF	DAF	Sohano Wharf	Sohano
SE-RF2	RF2	Nenumbo	Ngangaua
SE-RF6	RF6	Ngamanie	Ngambelipa
SE-SZ8	SZ8	Nanggu	Santa Cruz

Table 5.2.
A summary of the information presented about each site.

Site code	Excavation area (m^2)	Portion of total area (%)	Site 'type'	Sieve size
SAD	70	8	Village/redeposited	1/4"
SAC	44	29	Village/redeposited	1/4" & 5 mm
SDI	4	3	Village area	5 mm
EAB1	9	26	Cave shelter	1/4" & 5 mm
EAB2	8.25	3	Cave shelter	5 mm
ELS	5	3	Midden	5 mm
ELT	2	1	Midden	5 mm
DAF	n/a	–	Reef scatter	–
DES	n/a	–	Reef scatter	–
DIZ	n/a	–	Reef scatter	–
DGD2	6	17	Cave shelter	5 mm
DGD3	1	4	Cave shelter	5 mm
DGW	8	38	Cave shelter	5 mm
DFV	7	4	Cave shelter	5 mm
DFF	7	4	Cave shelter	5 mm
RF2	145	14	Village area	1/4"
RF6	20	1	Village area	1/4"
SZ8	51	1	Village area	1/4"

Table 5.3.
Radiocarbon dates used to determine artefact dating at each site.

#	Site	Provenience	Sample #	Radiocarbon age	Sample type	Calibrated age (years BP)*	Time period
1	SAC	zone c1	anu5330[1]	2390±80	marine	2107–1901	3
2	SAC	zone c2	anu37b[2]	2420±110	human	2715–2338	3
3	SAC	zone c2	beta16835[1]	2470±75	marine	2191–1998	3
4	SAC	zone c2	anu5336[1]	2530±90	marine	2306–2064	3
5	SAC	base	anu5339[1]	3490±80	marine	3446–3270	2
6	SAD	above ash	anu72[2]	720±57	charcoal	687–650	5
7	SAD	zone 4 top	anu73[2]	1595±60	charcoal	1537–1408	4
8	SDI	ash layer	anu5338[3]	1290±90	charcoal	1295–1080	4
9	SDI	layer 2	anu5329[1]	2190±80	marine	1862–1684	4
10	SDI	layer 3	anu6475[1]	2630±80	marine	2355–2197	3
11	SDI	layer 4	beta16836[1]	3020±90	marine	2859–2716	2
12	EAB1	spit 2	gak2437[4]	1540±270	charcoal	1712–1178	4
13	EAB1	spit 7	nsw95[4]	6800±410	charcoal	7958–7263	1
14	EAB2	horizon 2	anu4972[5]	3120±190	charcoal	3550–3003	2
15	EAB2	horizon 3	anu4849[5]	7680±510	charcoal	9191–7936	1
16	EAB2	horizon 5	sua2502[5]	10560±230	charcoal	12726–12186	1
17	EAB2	horizon 6	anu4973[5]	9970±390	charcoal	12252–10583	1
18	EAB2	horizon 7	anu4848[5]	14240±400	charcoal	17534–16590	1
19	ELS	phase 4	anu7483[6]	2250±70	charcoal	2341–2124	3
20	ELS	phase 4	anu7482[6]	2580±70	charcoal	2755–2404	3
21	ELS	phase 4	anu5851[7]	2730±80	marine	2534–2322	3
22	ELS	phase 3	anu5850[7]	2870±80	marine	2727–2486	3
23	ELS	phase 2	anu7484[6]	2470±70	charcoal	2718–2356	3
24	ELS	phase 1	anu7485[6]	3040±80	charcoal	3356–3061	2
25	ELT	phase 3 top	anu5852[7]	2730±80	marine	2534–2322	3
26	DFF	20–30cm	anu6804[6]	2430±50	marine	2115–1982	3
27	DFF	30–50cm	anu5221[8]	2920±80	marine	2752–2647	3
28	DFF	60–70cm	anu7518[6]	3460±70	marine	3392–3247	2
29	DFF	70–80cm	anu5224[8]	3140±120	charcoal	3454–3145	2
30	DFF	70–90cn	anu8340[6]	3450±180	charcoal	3959–3460	1
31	DFF	100–110cm	anu5225[8]	2910±150	charcoal	3318–2791	2
32	DFF	100–110cm	anu8339[6]	3320±170	charcoal	3720–3360	1
33	DFF	110–135cm	beta14447[8]	3740±80	marine	3737–3553	1
34	DFF	120–130cm	anu7519[6]	3400±70	charcoal	3716–3484	1
35	DFF	130cm	anu6805[6]	3270±190	charcoal	3689–3268	1

36	DFV	20–40cm	anu8304[6]	3130±240	charcoal	3636–2966	2
37	DFV	45–55cm	anu6815[6]	5320±280	charcoal	6403–5749	1
38	DFV	45–55cm	anu7862[6]	3820±200	charcoal	4508–3891	1
39	DFV	50–67cm	anu5220[8]	4300±210	charcoal	5279–4529	1
40	DFV	60–80cm	anu6813[6]	3860±100	marine	3926–3609	1
41	DGD2	50–60cm	anu6808[6]	1970±210	charcoal	2286–1629	4
42	DGD2	50–60cm	anu7863[6]	2080±150	charcoal	2302–1875	3
43	DGD2	55–65cm	anu5227[8]	1590±70	marine	1221–1053	4
44	DGD2	70–80cm	anu6136[8]	2440±110	charcoal	2719–2342	3
45	DGD2	60–70cm	anu6810[6]	2460±130	charcoal	2740–2342	3
46	DGD2	90–100cm	anu6137[8]	2480±120	charcoal	2742–2349	3
47	DGD2	100–110cm	anu8301[6]	2820±70	charcoal	3016–2784	2
48	DGD2	105–115cm	anu5228[8]	3350±80	marine	3312–3080	2
49	DGD2	110–120cm	anu6809[6]	2990±60	charcoal	3317–3003	2
50	DGD2	120–130cm	anu8302[6]	3310±170	charcoal	3718–3358	2
51	DGD2	145–165cm	anu5229[8]	3110±80	marine	2961–2769	2
52	DGD3	170–180cm	anu5230[8]	850±80	charcoal	884–672	5
53	DGW	120–130cm	anu5142[8]	860±110	charcoal	920–669	5
54	DGW	140–150cm	anu5226[8]	1310±100	charcoal	1305–1086	4
55	DGW	170–180cm	anu5223[8]	1860±150	charcoal	1948–1571	4
56	DGW	190–200cm	anu6799[6]	2400±210	charcoal	2743–2154	3
57	DGW	210–220cm	anu6138[8]	3380±130	charcoal	3812–3445	1
58	DGW	220–230cm	anu8337[6]	3660±190	charcoal	4346–3690	1
59	DGW	230–250cm	anu7515[6]	3800±170	charcoal	4419–3899	1
60	DGW	250–270cm	anu8338[6]	3210±240	charcoal	3687–3063	2
61	DGW	270–290cm	anu7516[6]	4440±80	marine	4717–4483	1
62	DGW	290–310cm	anu5222[8]	3460±80	marine	3404–3235	2
63	RF2	grey layer	anu6477[9]	2730±120	charcoal	2956–2743	2
64	RF2	grey layer	i5748[9]	2775±100	charcoal	2997–2760	2
65	RF2	grey layer	anu6476[9]	2850±130	charcoal	3161–2780	2
66	RF2	grey layer	i5747[9]	2955±95	charcoal	3317–2957	2
67	RF6	grey layer	i5750[9]	2460±95	charcoal	2720–2350	3
68	RF6	grey layer	i5749[9]	2530±95	charcoal	2747–2362	3
69	SZ8	grey/brown	sua112[9]	3140±70	marine	2989–2814	2
70	SZ8	grey/brown	sua111[9]	3250±70	marine	3159–2949	2

*Calibration at one sigma using CALIB 3.0.3a (Stuiver and Reimer 1993).
1-(Green and Anson 1991: 173–4), 2-(Specht 1968: 124), 3-(Green and Anson 1987: 123), 4-(White, Downie and Ambrose 1978: 877), 5-(White et al. 1991: 49), 6-(Spriggs 1996b: 37–8), 7-(Golson 1991: 249), 8-(Spriggs 1991: 227), 9-(Green 1991b: 206–7).

Table 5.4.
The tertiary sites used in this thesis. References are provided where the data are used.

Area	Location	Code
Arawe	Apugi	FFS
Arawe	Apugi	FFT
Arawe	Yimilo	FLB
Arawe	Awakuo	FLE
Arawe	Alanglongromo	FLF
Arawe	Alanglong	FLQ
Arawe	Paligmete	FNY
Arawe	Lolmo Cave	FOF
Arawe	Makekur	FOH
Arawe	Apalo	FOJ
Arawe	Amulut	FOL
Arawe	Winkapiplo	FON
Arawe	Murien	FST
D.of York	Duke of York	SDP
D.of York	Kabakon	SEE
Talasea	Garua	FAO
Talasea	Garua	FAP
Talasea	Garua	FAQ
Talasea	Garua	FRL
Talasea	Garua	FSZ
Manus	Pamwak	GOD
St Matthias	Eloaua	ECA
St Matthias	Eloaua	ECB
St Matthias	Emananus	EHB
St Matthias	Mussau	EKN
St Matthias	Eloaua	EKO
St Matthias	Mussau	EKP
St Matthias	Emussau	EKS
St Matthias	Mussau	EKU
N.Britain	Boduna	FEA
N.Britain	Yombon	FGT
N.Britain	Misisil Cave	FHC
N.Britain	Walindi	FRI
N.Britain	Buvusi	FRK
N.Britain	Bitokara	FRL
N.Britain	Lamogai	FSS
N.Ireland	Buang Merabak	EFP
N.Ireland	Panakiwuk	EAS
N.Ireland	Matenkupkum	EFR
N.Ireland	Matenbek	EFS
N.Ireland	Lamassa Island	EPE
N.Ireland	Lamassa Island	EPG
N.Ireland	Lambon Island?	EPK
N.Ireland	Karias	EPQ
N.Ireland	Karias	EPR
N.Ireland	Karias	EPS
N.Ireland	East Coast	EPZ
N.Ireland	East Coast	EQA
N.Ireland	East Coast	EQB
N.Ireland	East Coast	EQC
N.Ireland	East Coast	EQD
N.Ireland	East Coast	EQE
N.Ireland	East Coast	EQF
N.Ireland	East Coast	EQH
Santa Cruz	Tömotu Noi	SZ10
Sohano	Reef	DAA

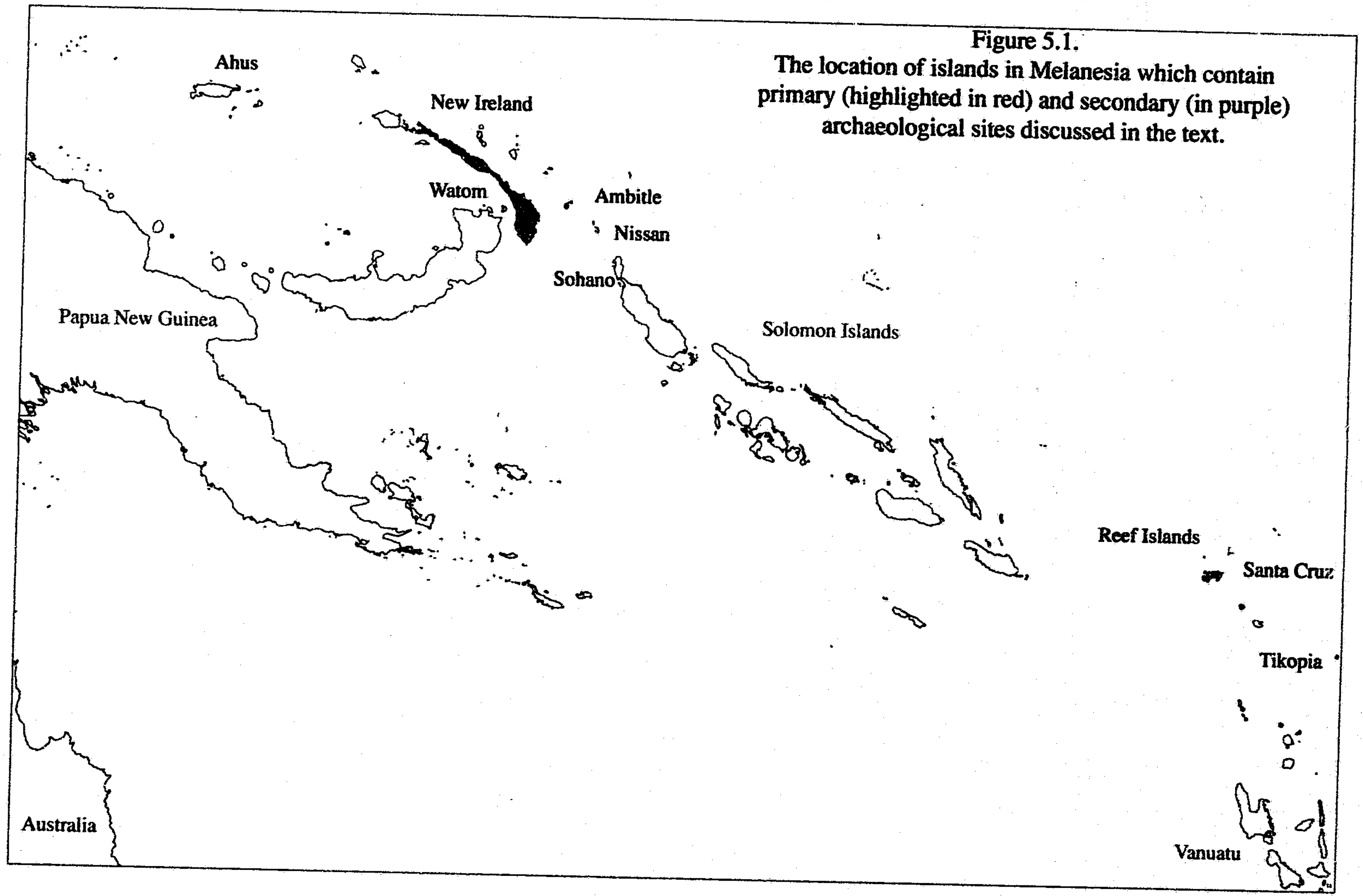


Figure 5.1.
The location of islands in Melanesia which contain primary (highlighted in red) and secondary (in purple) archaeological sites discussed in the text.

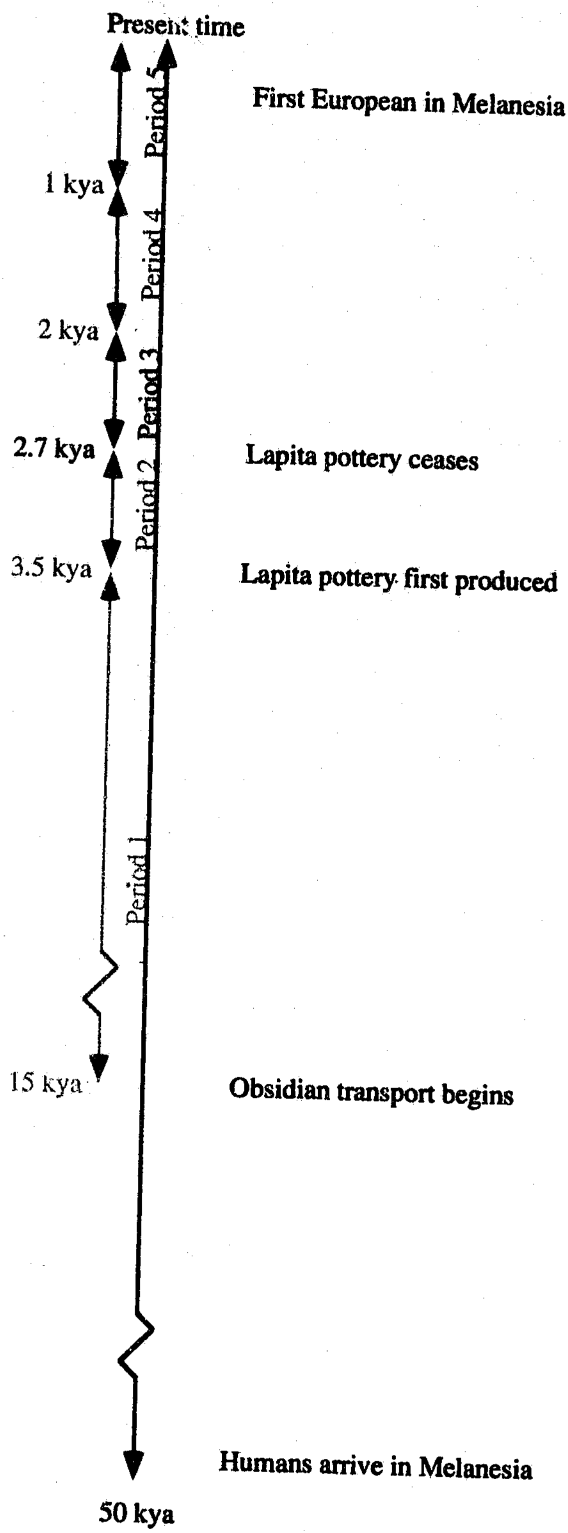


Figure 5.2.
A time-line displaying the five time periods used in this thesis set into the context of Melanesian history.

Figure 5.3.
An idealised view of the stratigraphy at site SAD showing the cultural zones, the two radiocarbon dates (the number in brackets refers to table 5.3), and by colour, the time period for each layer.

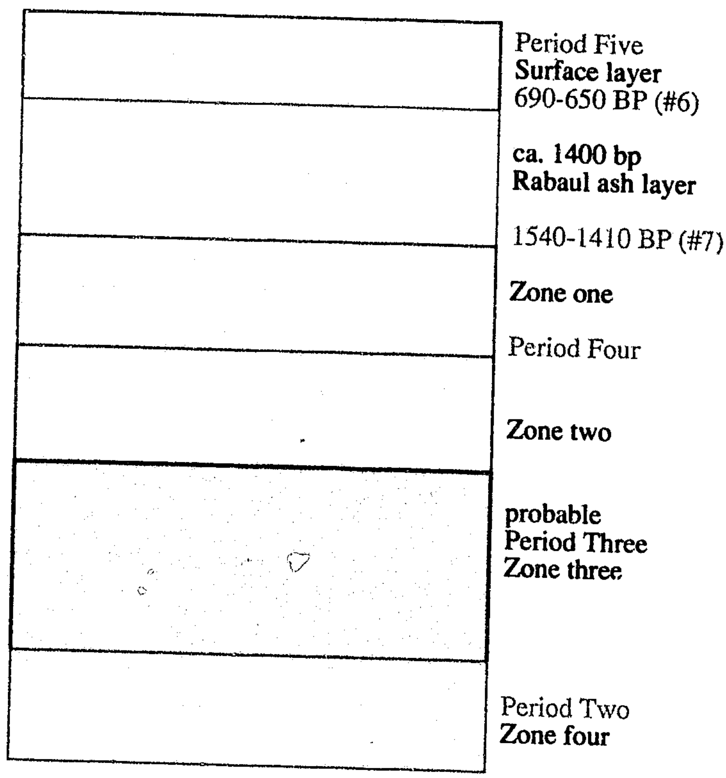


Figure 5.4.
An idealised view of the stratigraphy at site SAC showing the cultural zones, and the five radiocarbon dates (the number in brackets refers to table 5.3).

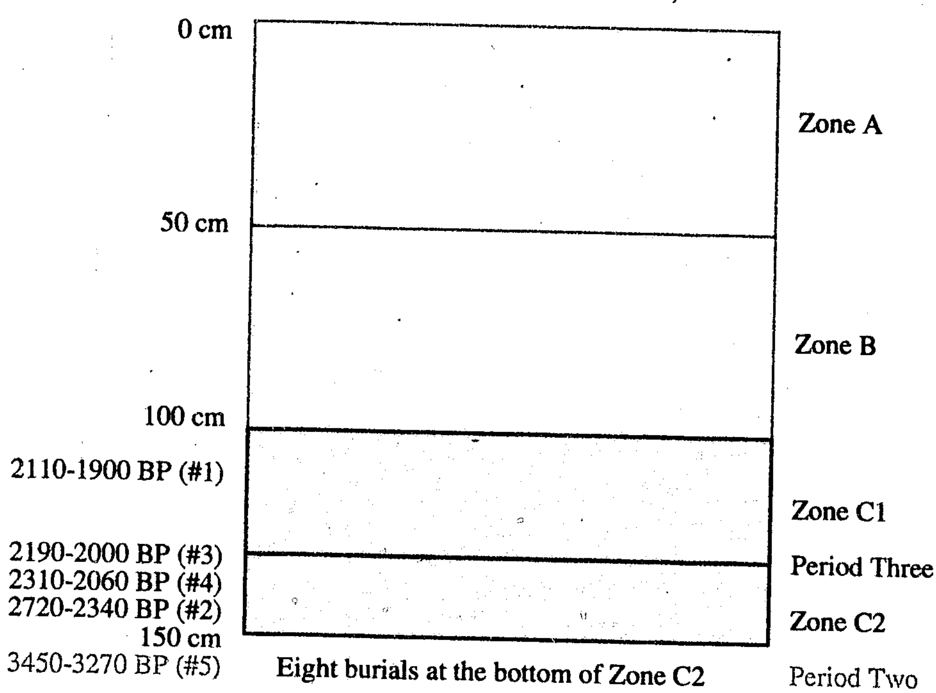


Figure 5.5.
An idealised view of the stratigraphy at site SDI showing the extensive ash layer above the cultural deposits, the four identified cultural layers, the four radiocarbon dates (the number in brackets refers to table 5.3), and by colour, the time period for each layer.

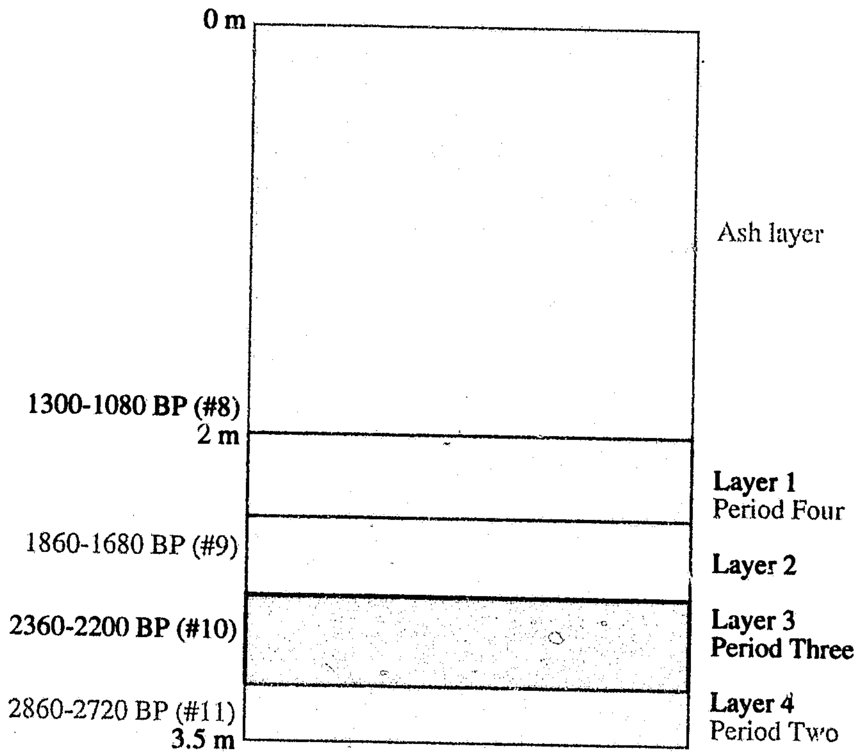


Figure 5.6.
An idealised view of the stratigraphy at site EAB-1 showing the excavation spits, the four radiocarbon dates (the number in brackets refers to table 5.3), and by colour, the time period for each spit.

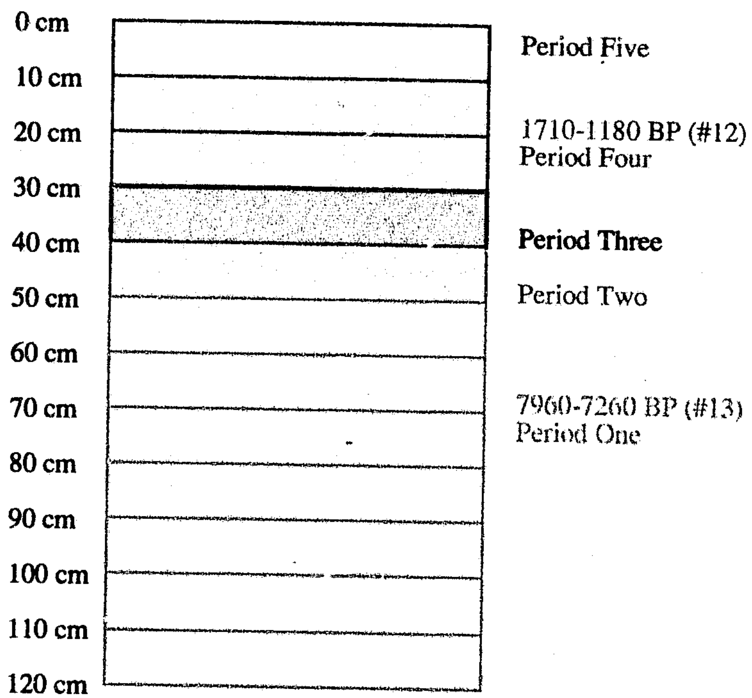


Figure 5.7.
An idealised view of the five meter long trench wall at site ELS showing the relationship of the five cultural phases, the radiocarbon dates used to date each phase (the number in brackets refers to table 5.3), and by colour, the time period for each phase.

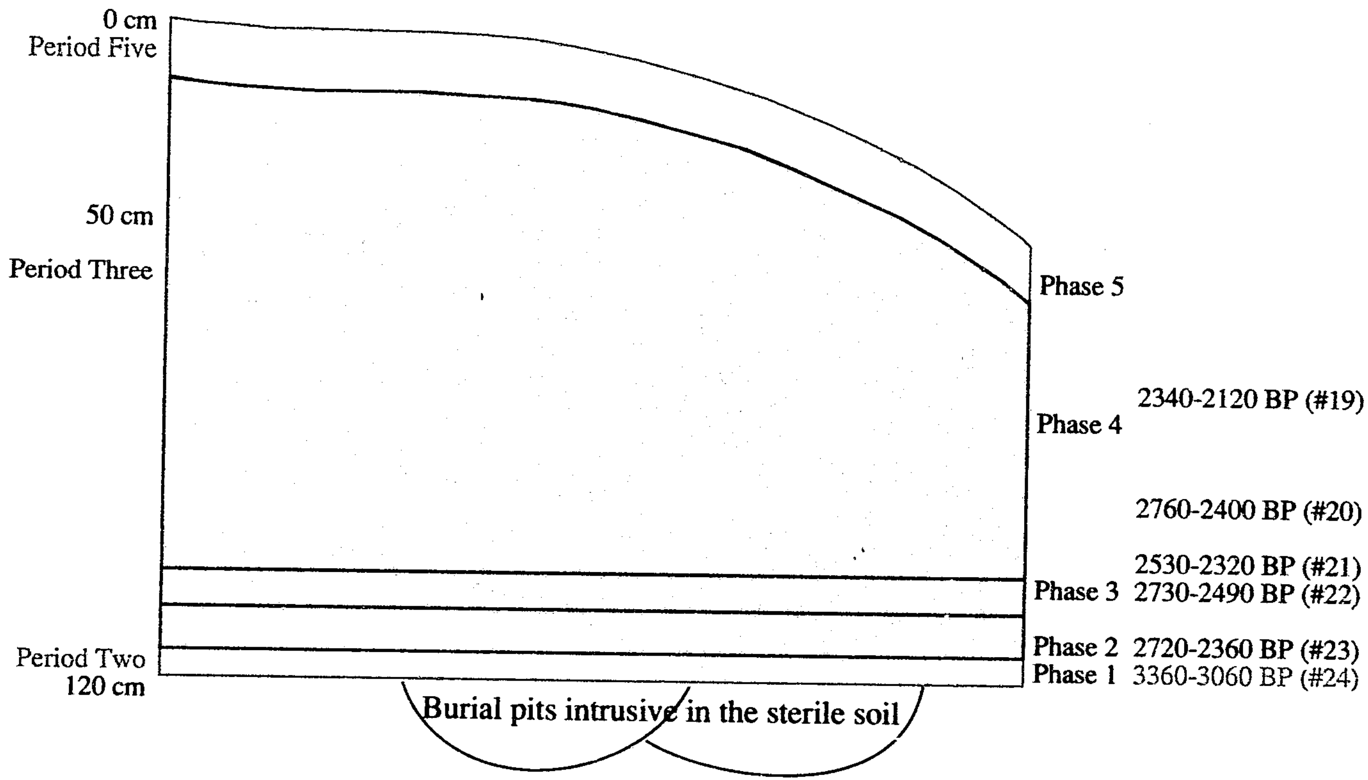


Figure 5.8.
An idealised view of the stratigraphy at site DGD2 showing the cultural phases, the eleven radiocarbon dates (the number in brackets refers to table 5.3), and by colour, the time period of each layer.

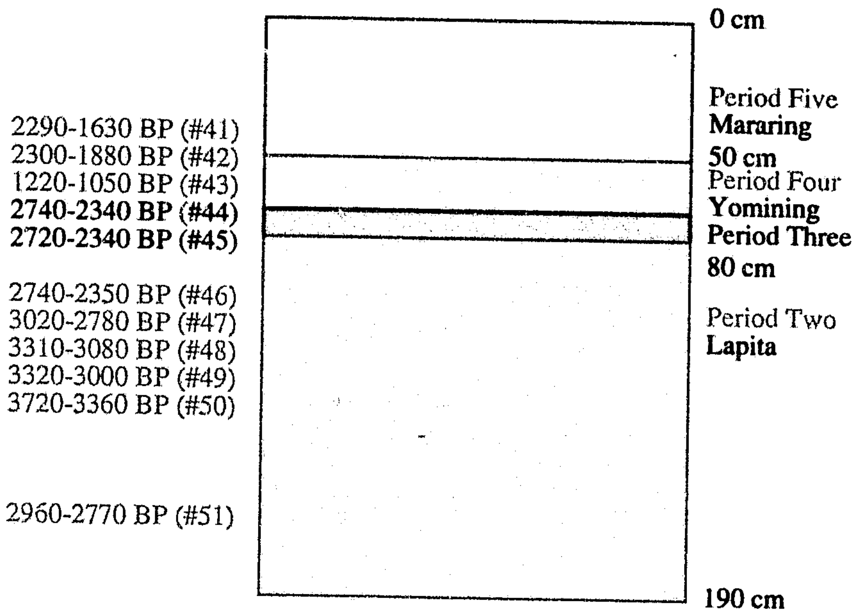


Figure 5.9.
An idealised view of the stratigraphy at site DGW showing the deep stratigraphy with three cultural phases, the ten radiocarbon dates (the number in brackets refers to table 5.3), and by colour, the time period for each phase.

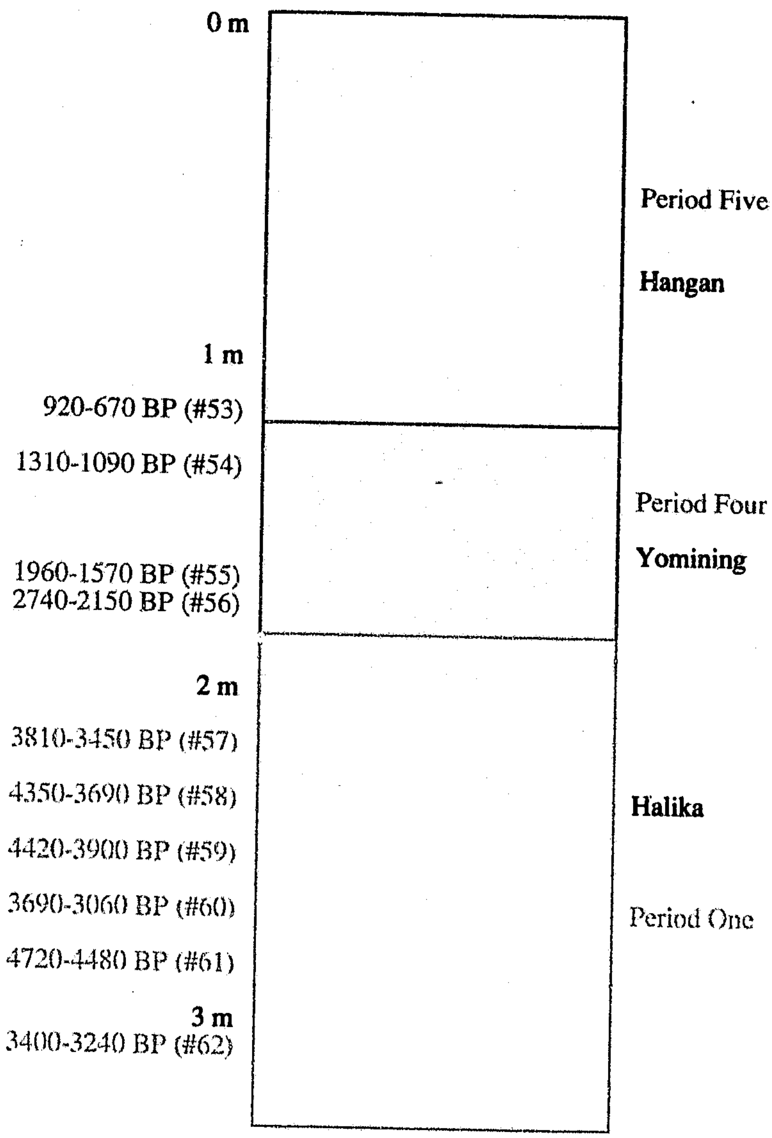


Figure 5.10.
An idealised view of the stratigraphy at site DFV showing the two layers, the five radiocarbon dates (the number in brackets refers to table 5.3), and by colour, the time period for each layer.

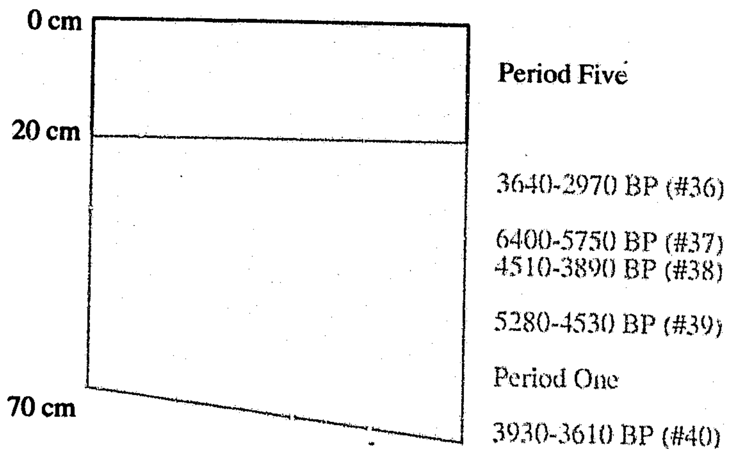
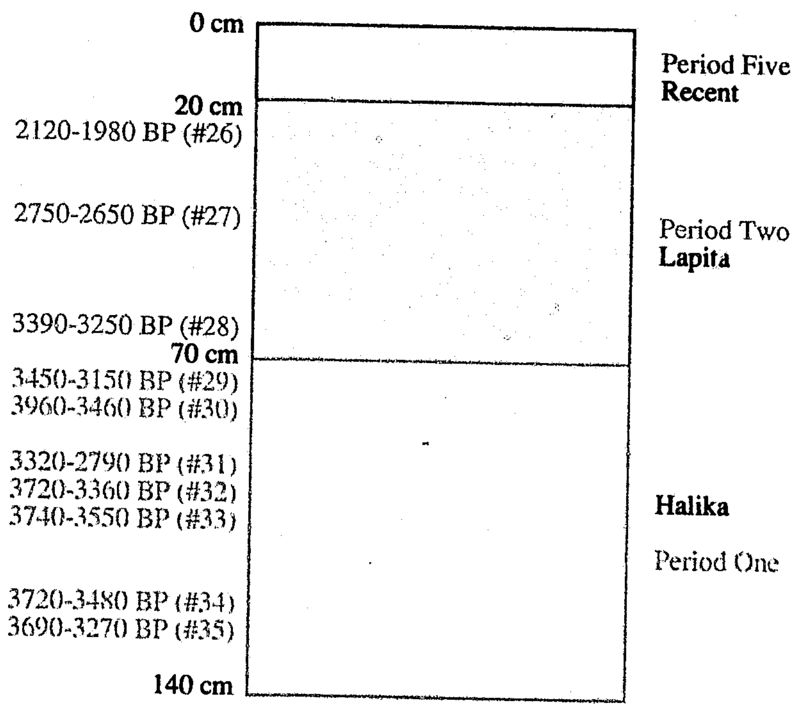


Figure 5.11.
An idealised view of the stratigraphy at site DFF showing the cultural phases, the ten radiocarbon dates (the number in brackets refers to table 5.3), and by colour, the time period for each phase.



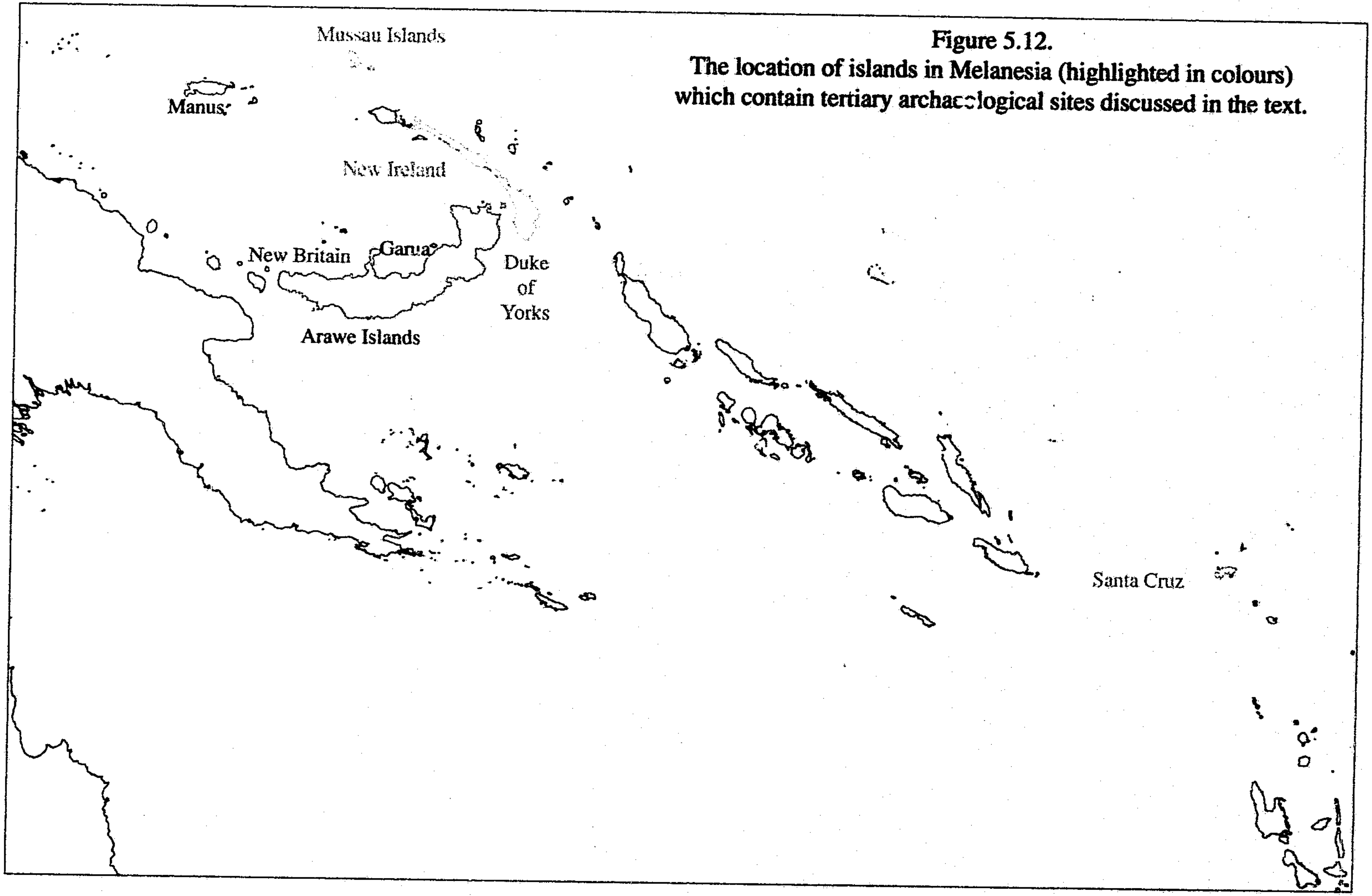


Figure 5.12.
The location of islands in Melanesia (highlighted in colours) which contain tertiary archaeological sites discussed in the text.

§6

Obsidian source determination

This chapter begins the methods section of the thesis. Chapters six through eight detail the procedures used to study the obsidian, and the results obtained from them. While the standard template of a scientific paper locates results in their own section after the methods it was impossible for me to divorce the two. As many of the procedures rely on their outcome for development of further investigations and evolution of the procedure themselves, it proved awkward to write about either in isolation.

Chapter four introduced the utility of sourcing determinations to archaeology. In this chapter, after a consideration of the many obsidian sourcing techniques available, I review the merits of each of the four sourcing techniques commonly employed in Melanesian prehistory. The outcome of all this sourcing work is presented for my primary sites and then for all sites with available data.

A feature of working with obsidian is its unique chemical 'fingerprint' that allows relatively simple and accurate attribution of a sample to its source. The relationships for known South West Pacific obsidians are quite complex (Bird et al. n.d.), but can be sorted out mathematically in multi-dimensional space. Interest in obsidian as a trade good really emerged worldwide only in the late 1960s after sourcing became a practical option proven by a decade of initial experimentation (Ammerman 1979). Prior to that it was often overlooked in site reports.

Obsidian proves to be a great marker of Lapita sites. It was transported across great distances, but never in large quantity (Green 1979). While Talasea sources are known to have been exploited for thousands of years prior to the Lapita period (e.g., Balof site), no appearances of Admiralty Island's sources in a site outside the Admiralties are known to predate the Lapita period.

Like most things that rely on current technology, the techniques by which sourcing has been accomplished have varied over the past twenty-five years with changing technologies. Nearly thirty years ago, one of the first attempts made at identifying obsidian sources, using spectrographic techniques, was reasonably successful (Key 1968). Among these early source determinations were efforts made in Melanesia (Ambrose and Green 1972). The use of spectrography to identify elemental concentrations was the method applied until more practical alternatives came along in the mid-1970s (Smith, Ward and Ambrose 1977). In Europe and the USA obsidian sourcing via neutron activation analysis (NAA) and spectrochemical analysis was tried on many sites (Ammerman 1979). NAA has continued its development enough to have remained the method of choice in Europe (Tykot and Ammerman 1997), but other procedures are preferred elsewhere. A good review of some older sourcing techniques is found in Earle (Earle and Ericson 1977). The early use of X-ray fluorescence (XRF) to identify obsidian sources was improved by the additional data provided through NAA to secure more accurate source attributions. Sources that appeared to overlap with XRF could be separated out via NAA technology (Ericson 1977).

Proton induced gamma ray emission (PIGME) is cited (Bird et al. 1978) as a rapid and efficient method of determining elemental ratios in the archaeological obsidians for the purposes of linking them to their original source area. PIGME measures concentrations of fluorine, sodium and aluminium and subsequent statistical analysis allocates obsidian artefacts to source areas on this basis. The need to identify all sources is revealed by the allocation of some obsidians to an unknown source volcano (Bird, Ambrose et al. 1981). Ambrose and Duerden (1982) used the three elemental quantities in obsidian samples and found this sufficient to separate the four main source regions of concern to Melanesian archaeology (see below for a list of those sources). Twenty years ago, this was a new technique and the initial application to seven hundred artefacts proved its efficacy, however the ability to discriminate between broadly similar sources was lacking.

PIXE (proton induced X-ray emission) was added in to the PIGME routine to determine variations in other elements in the obsidian which eliminated some dimensions of overlap and allowed identification even of sub-sources. The CLUSTAN program is one example of a multi-dimensional statistical analysis software package essential to separating the streams of raw data into sensible groupings. Both the gamma ray and the X-ray emissions can be tracked during the same proton bombardment episode, so the two are normally employed together. PIXE-PIGME sourcing techniques identify up to twenty-one elements contained in the obsidian pieces. Discrimination is possible now between individual volcanic episodes (Ambrose, Bird and Duerden 1981). The methodology is reviewed by Fullagar et al. (1989) and they discuss the nine elemental ratios typically used, and their use of non-parametric clustering statistics to complete the analysis.

Bird et al's article reports the first attempt at creating a catalogue of Pacific sources for obsidian; to allow rapid and simple identification of sources for future archaeological investigations (Bird, Duerden et al. 1981). Leach and Warren note in their article

(1981) that it is not enough to show that the chemical makeup of an artefact is 'like' that of a specific source, but one also needs to show that it is 'unlike' any other plausible source as well. This is a potential problem with all known sourcing methods. Shackley has written extensively on this topic (1987, 1992, 1995, 1998a, 1998b, 1998c) pointing out that unless one has completely characterised the source materials in the region of study, conclusions made from sourcing studies can be gravely incorrect. The development of the chemical characterisation techniques described above has produced such accuracy that the limiting factor in determination of source is now the regional source database (Shackley 1998b).

There are four source regions that are of concern to archaeologists in Melanesia. Given the island environment the limits to our knowledge of the obsidian sources are quite well understood. The Admiralty Islands sources (occasionally this will be referred to in a casual manner as the Lou source because virtually all 'export' obsidian derives from Lou) in Manus province of Papua New Guinea consist of four source islands and numerous sub-sources. Pam Lin and Pam Mandian islands, three individual sources on Lou Island, and two sources in the south-west of Manus Island all provide good quality obsidian suitable for flaking (Fredericksen 1994).

The Talasea sources on the Willaumez Peninsula of West New Britain province, Papua New Guinea are numerous distinct sources and sub-sources. Kutau/Bao, Baki, Hamilton and Gulu are the main, but not the only, sources (Summerhayes et al. 1998). Also within West New Britain is the Mopir source located across Kimbe Bay from Talasea. Not all of these obsidians are of equal quality for flaking. Note that 'Talasea' source obsidian includes all of the sources on the Willaumez Peninsula and does not include Mopir source, "West New Britain" obsidian includes both the Mopir material and the Talasea material.

On Fergusson Island and its smaller neighbours Dobu and Sanaroa in the D'Entrecasteaux group just off the eastern tip of mainland Papua New Guinea are found several source volcanoes of variable quality for flaking (Green and Bird 1989). The density of these Fergusson obsidians is potentially quite problematic because it overlaps with the ranges for both Admiralties and West New Britain obsidians. Despite the importance of Fergusson as a source generally, this proves to be less of a problem in this research as only two identifications of Fergusson obsidian have been made in all the assemblages examined.

Finally the Banks Islands sources in Northern Vanuatu encompass two distinct sources of generally lesser quality obsidian on Vanua Lava and Gaua islands (Ambrose, Bird and Duerden 1981). Limited to these four, it is highly unlikely that wrong source allocations will be made for Melanesian sites. It is conceivable that obsidian could come from elsewhere, such as the currently unlocated source 'X' that is believed to be in the Fergusson source area but could prove to be located elsewhere (Ambrose n.d.).

One problem with the PIXE-PIGME method is the need for high energy protons. Expense and availability of appropriate facilities limits the universal application of the technique. Fredericksen applied EDS (Energy Dispersive Spectroscopy) in a modified scanning electron microscope (SEM) to identifying major elements in obsidian artefacts and found that it, too, usually allows identification of sub-source flows (Fredericksen 1994: appendix G). EDS proved able to discriminate between the Admiralties sources as well as the PIXE-PIGME system. It does however require a mounted and polished section of each piece, and is therefore more destructive than PIXE-PIGME which is usually performed on the whole artefact. EDS does seem to be winning favour amongst some archaeologists however (Tykot and Ammerman 1997).

In chapter three I mentioned a variety of exchange studies which relied on accurate sourcing data for the exchange goods. I will now briefly cover some historical uses of sourcing at this point. One of the earliest uses of chemical sourcing comes from Melanesia, when it was discovered that obsidian on Watom Island came from Talasea, whereas obsidian in the Trobriand Islands came from Fergusson (Key 1969). Elsewhere Singer and Ericson were able to complete a model of regional exchange in California when the sourcing data was provided (1977).

Much of the sourcing research, especially early on but still continuing today, concerned itself with discovery and characterisation of obsidian sources. Ericson (1977) created a database of thirty American obsidian sources with his NAA studies. Specht and Hollis discovered previously unknown sources on New Britain (1982), and Ambrose and Johnson eliminated the island of Unea as a likely source (1986); both studies following up on local oral traditions. That sources remain unidentified is highlighted by the number of 'unknown' classifications produced by research such as that of Leach (1985).

Looking specifically at PIXE-PIGME the successes of the earlier work by Ambrose and others (Ambrose, Bird and Duerden 1981, Ambrose, Duerden and Bird 1981, Green 1987) led to the more recent body of work by Summerhayes (Summerhayes and Hotchkis 1992, Summerhayes and Allen 1993, Summerhayes et al. 1993, Bird et al. 1997, Summerhayes et al. 1998). On the basis of many thousands of successful source identifications using PIXE-PIGME, it has become the most important sourcing method for obsidian in Melanesia, and the argument has been made that it should be even more widely utilised (Summerhayes et al. 1998).

The use of density measurements allows for a quick and easy sorting between two of the major Melanesian obsidian sources. While not possessing the accuracy of the elemental methods described above—sub-source determination is beyond their purview—the two density measuring techniques allow for sorting in the field if need be, and are reliable if used within their limitations (Torrence and Victor 1995). Often, one of the density techniques is applied as a preliminary sorting method for PIXE-PIGME.

The relative density measurement procedure uses a heavy salt solution (Sodium MetaTungstate) and carefully chosen reference pieces of obsidian (Green 1987). All of the sites that Roger Green excavated (SAC, SAD, SDI, SZ8, RF2, & RF6) were processed with this procedure, at least in part. Each flake is compared for density to a pair of reference flakes (assuming that this sorting procedure is attempting to separate West New Britain obsidian from Admiralties obsidian). As Talasea obsidian is the less dense of the two, the highest density known from a Talasea source is compared with the lowest density known from an Admiralties' source. The gap between the two is approximately one per cent so determinations are reasonably accurate. A separate salt solution that leaves each reference flake neutrally buoyant is mixed up, and small groups of flakes are placed in the solutions. Those that sink in the more dense solution are most probably from an Admiralties source. Those that float in the less dense solution are most probably from a Talasea source. The problematic pieces in the middle, with density between the expected ranges, are those first nominated for sourcing by another method.

The absolute density method uses a heavy fluid (Per-fluoro-methyl-decalin) and a light fluid (air) to determine the specific gravity of each flake (Ambrose n.d., W. Ambrose pers. comm.). Monitored for variation by temperature, each flake is weighed first in air and then in the liquid. The resulting numbers are fed into a simple formula that returns density with three significant digits (see Appendix). A standard of known density is run every few minutes to compensate for drift (particularly of temperature) through the session. Standards of known source verify the accuracy of results. The density figures can then be compared to known values for each source.

The absolute density procedure has a better chance (compared to relative density) of picking out obsidian that came from other than the two expected sources (Admiralty Islands or West New Britain), because it uses individually measured values, rather than a comparison method. As established by Ambrose, I used a range of 2.345 to 2.363 g/ml to cover Talasea sources (see figure 6.1). For Admiralties' sources, I used the range of 2.379 to 2.403 g/ml. Obsidian flakes that lay outside either range were either from another source, or had inclusions in them that altered the density reading. The relative density method, in contrast, only identifies a potential problem for the flakes between the expected ranges and flakes well outside the range often go unnoticed. Fergusson sourced obsidian is a problem for either method in that the density range of this source covers both the ranges of Talasea and the Admiralty Islands. Both density methods have problems with extremely small pieces, because of the potential for huge error in small pieces with inclusions and air bubbles which are heavier or lighter than obsidian (Torrence and Victor 1995).

The currently favoured form of source analysis is the PIXE-PIGME system (Summerhayes et al. 1998). Specificity is excellent, with local sub-source utilisation studies being accomplished easily. This is the favoured procedure due to its accuracy and non-destructive nature.

In summary the four sourcing procedures can be rated in comparison with the other methods. By technical requirements I refer to the level of training required of the operator. Access refers to the equipment required for the procedure. Destructiveness refers to the use-wear traces as the three techniques described as 'low' are all non-destructive to the obsidian. Relative density has low cost, low technical requirements, easy access, low destructiveness, and low accuracy. Absolute density has low cost (though higher than for relative density), moderate technical requirements, easy access, low destructiveness, and moderate accuracy. EDS has moderate cost, high technical requirements, moderate access, high destructiveness, and high accuracy. PIXE-PIGME has high cost, high technical requirements, restricted access, low destructiveness, and high accuracy.

Many of the flakes (almost exactly half) used in this study had been sourced by one of four methods discussed here prior to my receiving them. A few were sourced during my research period by others, and more than two hundred and fifty were sourced by myself (see Appendix). All of the sites had some of the flakes sourced.

Enough of the flakes had multiple runs in the different procedures that I would be certain now, if it had not already been well-established, that each sourcing procedure has acceptable accuracy. Two of the procedures are density based, and therefore quite easy and inexpensive to complete. The other two are based on component element ratio analysis and therefore require sophisticated equipment and experienced practitioners to be completed. The reward for this added complexity is increased discrimination among sources.

The changes in source exploitation through time are apparent from figure 6.2. Figure 6.3 demonstrates the differences in source exploitation for each island group. One obvious problem with both of these diagrams is that they collapse data classes together in the interests of clarity. That is, figure 6.2 ignores location of the archaeological sites to display source versus time. Similarly figure 6.3 ignores time to display source versus location. There are reasons for presenting them at all, however—the sourcing data are highly skewed in their distribution across both space and time. These illustrations summarise the data and remove some of the confusion caused by the skew.

From figure 6.2 it is apparent that the appearance of Admiralties' obsidian initially in period two (accounting for forty-eight per cent of the obsidian from period two) is one of the highlights of these data. The disappearance of Mopir obsidian from period one (fifteen per cent) into period two (zero per cent) is another important feature. Mopir makes a small reappearance (two per cent) in period three. While in figure 6.3 it is obvious that the Admiralty group is the primary source for all but the Reef/Santa Cruz sites, and the only source found at Sohano Island. Both Mopir and Banks sources make minimal contributions except for Banks in the Reef/Santa Cruz group, where Banks obsidian occurs in larger numbers than Admiralty obsidian.

Figures 6.4, 6.5 and 6.6 present alternative views of the same data, a combination of source, time and location. Examining figure 6.4 reveals the difficulties in presenting four dimensional data on two dimensional plots; even in pseudo-3D. It is possible to make the same observations about general obsidian use as from figures 6.2 and 6.3, but more difficult. What is most obvious from this graph is the amount of white space present, signifying zero information locations. By combining the time periods with the sites and plotting against the sources, figure 6.5 is much clearer. While figure 6.5 emphasises the inter-group variations, figure 6.6 is reorganised to emphasise the intra-group variations.

None of these five graphs present a clear trend in source exploitation that "leaps out" at the observer, there are no immediately obvious linear changes in source use. It does not mean that there are no interesting changes to be found. Mopir is a source confined by both time and geography. It is not found from period two deposits because, it is believed, it was buried under metres of ash deposits throughout this period. Neither is it found in the further distant islands. Banks Islands sources are constrained by time because of their geographical setting. No early use of Banks sources are seen because period one populations are not known from that far east. With the seemingly west to east flow of people and goods from period two, this obsidian was unlikely to find its way into the sites of the Bismarcks.

The only island to present data from all five time periods was Nissan. Figure 6.7 contains only the Nissan data. The first four time periods show a steady decrease in the percentage of Talasea obsidian present. Other observations have all been noted above; that Admiralties obsidian appears first in period two and that Mopir obsidian vanishes after period one. Period three was the only period to contain data from all island groups. What might be construed as a gradual increase in the percentage of Admiralties obsidian (fig 6.8) with increasing distance from source is negated by its lack in the Reef/Santa Cruz group during this time. Admiralties obsidian is found in low numbers dating immediately prior to the Period 2/3 boundary (clearly visible in fig 6.4), but not in quantity sufficient to continue the trend in increasing percentage with distance. However, this is a valid observation when confined to the Bismarcks.

A problem plaguing this group as a whole is the fact that it does not represent a unit of any significance in any terms (i.e., these sites are not a culturally or geographically constrained entity). It was simply the collection of sites available to do this research with. Incorporating all of the known sites from the region does create a grouping of cultural relevance. The tables 6.1 through 6.4 present the collation of all of the above data with the published materials available. An attempt was made to produce an exhaustive listing, but it is possible that several sites will have been missed.

Without fine dating information that is often difficult to compile, it is impossible to incorporate all the sites into my five time periods. The result then is three time periods that are broadly equivalent to my periods one, two-three-four, and five. Period one data is presented in table 6.1. The combination of my periods two, three and most of period four are presented in table 6.2. Table 6.3 contains sites from the past fifteen hundred years, corresponding to part of my period four and all of period five. Finally, table 6.4 presents the data from undated sites.

Table 6.1 reveals only two sites with Admiralties obsidian. All of the obsidian at site GOD (Pamwak) is Admiralty Islands derived· not surprising given its location on Manus in the Admiralties. The single flake at Buang Merabak cave on New Ireland either means that the statement that Admiralties obsidian is not transported beyond that archipelago prior to 3500 years ago is wrong, or that this assignment is incorrect. I believe the latter is the most likely explanation when only one flake is involved.

Mopir is an inconsistent component in the remaining sites, varying from none to all of the deposited obsidian. Only the Talasea area sites themselves (FAO, FAP, FAQ, FRI, FRL) lack any obsidian from Mopir.

The middle period group of sites (from 3500 bp to 1500 bp) present some data (table 6.2) that potentially contradict current thinking. Mopir obsidian is found in more than single flake quantities at site SDP in period two, when no Mopir obsidian is expected to be found anywhere. Also Mopir obsidian is found on several Arawe Islands sites for which the dating spans the whole of this middle time period; i.e., all this Mopir obsidian could derive from period three or later levels, or some could be from period two. Admiralties obsidian appears in virtually every area, although in New Britain it is in low relative quantities. As in the early period, no 'foreign' obsidian sources are found in the vicinity of Talasea, nor am I aware of any Talasea obsidian in the Admiralty Islands. Banks Islands obsidian is found in the sites at the south end of the Solomon Islands in low absolute numbers only, and nowhere else.

The third period, the most recent fifteen hundred years, displays (table 6.3) much Mopir obsidian in the vicinity of New Britain, but none outside this area. Banks Islands obsidian is again found in small quantities at Tikopia only, where it is the only obsidian found. Mixing of the Admiralty obsidian with the Talasea obsidian is seen only in deposits at Nissan and site SEE in the Duke of Yorks. Elsewhere, there is a tendency to either one or the other source with the occasional single flake occurrence preventing this from being a 'rule'.

A quick review of the undated sites (table 6.4) reveals that Mopir source only shows up as a handful of flakes in the sites of the Duke of York Islands. Some Banks Islands obsidian is seen on Santa Cruz. The remainder are mostly a mixture of Talasea and Admiralties obsidian within each site.

In summary of the source allocation data above, figure 6.9 presents a simplification of the distributions across Island Melanesia during the period of long distance transportation of obsidian. The production of this diagram required a simplification of the known complexities in obsidian distribution. Beginning with an ellipse around each of the Talasea and Admiralties source areas, the rough shapes were manipulated only until most of the data were accommodated. That obsidian from Watom comes primarily from the Admiralties, for one example, is missed by the illustration. As more data become available and the exact boundaries of each distribution are more precisely known, the cultural component in this will be more visible. The fact that the Admiralties distribution tends to be north of the Talasea distribution is a result of geography, rather than culture. An example of a cultural influence in the distribution is the dominance of Admiralties sources in the North Solomons.

Peter White reports his survey work on southern New Ireland and in the nearby Duke of York Islands (White 1997, White and Harris 1997). With a few simple calculations of average mass and sourcing data he presented results that suggested the more common source of obsidian in a given site was used with less care. The obsidian from a source found more frequently was also found to occur in larger pieces.

There are problems in basing conclusions on such small sample sizes where the validity of the comparison is not tested statistically. White, however, merely suggests that this trend is present in the data. Following on with this idea, I calculated mean weights for all the obsidian sourcing data that I had available. Presented in table 6.5, the coverage of sites is different from those used throughout the thesis because some sites had very few flakes of obsidian to begin with, and these were not all sourced completely. There are therefore too few numbers to derive a mean from. For example, site DGD3 probably should not be included with only three flakes.

At Watom site SDI, whether combined across periods three, four and five or just period four alone, the results agree with the trend noted by White. The more frequent source (Talasea) on average produced the larger flake. Site SAC violates this trend with its massive numbers of flakes. More of those that have been sourced come from the Admiralties, while the more massive flakes come from Talasea. This fact might point to another potential anomaly in this procedure.

Selecting a sample of the flakes for sourcing is accomplished by various means depending on the researcher involved. In some cases these samples are just a random selection of an appropriate number for the resources available. In other cases, non-random criteria are used. For EDS sourcing, Fredericksen (1997b) chose the shatter pieces preferentially over the flakes to avoid destroying use-wear data. It is obvious that this might select for more pieces from one source than from another if size is related to source.

The sample from SAC is approximately fifteen per cent of the total; a high enough figure to assume that there is no great skewing of the data due to sampling strategy. It is unusual that some two-thirds of the flakes derive from the more distant Admiralty Islands when located so much closer to the New Britain sources. Site SAD backs up these findings with the fact that Admiralties obsidian is both more common and of larger average size.

Moving out to New Ireland, at EAB2 I found more material from Talasea but larger pieces from the Admiralties. It would be interesting to be able to partition the obsidian into appropriate time periods, however recall from the previous chapter that there were problems with Balof 2 in assigning particular spits to the appropriate cultural horizon. As such, this site represents a long period of time from which presumably Talasea obsidian was available, but Admiralties obsidian would not have appeared until c. 3500 years ago. Also, with only two pieces of obsidian from the Admiralties, one has to assume sampling error effects could occur in terms of mean weight.

At site ELS, the means are too similar to separate. At nearby ELT, the more common source is also the heavier flake.

On Nissan, DFV is complicated by the occurrence of three sources of obsidian instead of two. The most numerous source (Talasea) is the lightest regardless of whether I combine periods one and five together or consider them separately. Sites DGW and DGD2 revert to the suspected trend by displaying the heaviest flakes from the most common source (Admiralties). Site DGD3 with only two flakes from the Admiralties and one from Talasea nevertheless conforms to the trend. At site DFF the problem is that there are near equal numbers of flakes from both sources, however those fourteen Admiralties flakes have a higher mean weight than the thirteen Talasea flakes. The final site on Nissan to display this is DIZ. Sites DES, DAA and DAF (the first found on Nissan and the latter two on Sohano) all contain only Admiralty Islands obsidian.

Out in the Reef/Santa Cruz group the situation is again complicated by the presence of three sources of obsidian—this time from the Banks Islands. Site SZ10 on Santa Cruz has only a handful of flakes, but the lightest are also the least common; the single Admiralties flake is lighter than the average for the other two sources. The converse is not true, because there are more Banks flakes but the Talasea material is heavier. There is no real difference between the means for the few flakes from RF6. At RF2, the heaviest are the most common (Talasea) but the single Banks flake is not the lightest. Finally, at Tikopia where most of the obsidian is from the Banks Islands and three come from Talasea, the heavier mean is found in the Banks obsidians.

Altogether I found that approximately two-thirds of the sites agree and one-third disagree (neglecting the inconclusive ones) with White's suggested trend. In a best case scenario, all of the sites would conform to the predicted trend if it were a genuine behavioural trait. Or half of the sites would conform to the predicted trend if it were simply random chance controlling the size distribution (assuming only two sources). The observations leave us somewhere in the middle.

It would be quite easy to get mired in statistical testing to determine what the truth is. I have avoided this on the grounds that there are so few samples of generally small size. Further complicating matters is the fact that it would be impossible to tell from these sites if the behaviour was occurring in one area and not in another.

If I exclude all those sites with only one source present and those with too few data to provide a reasonably robust sample—at least ten flakes from each source, or at least thirty total flakes—I am left with five out of seven sites which support White's hypothesis. This leaves me no more certain than I was after the initial assessment.

SUMMARY

Chapter five has reviewed some of the history of sourcing procedures, as well as the four currently practiced procedures in Melanesian archaeology. On the basis of the source data it was discovered that there are few trends in obsidian distribution that apply across the whole region, but those trends noted are important. The combination of source data for the primary sites with published data reinforced both the conclusions drawn in this chapter and the pre-existing understanding of the area. In other words, no new ideas developed from this analysis but the status quo was reinforced as correct. Finally, following on from an idea by Peter White, I compared average flake size with source of obsidian for sites across Melanesia. The results were somewhat inconclusive, as fifteen sites supported while eight sites contradicted White's view that the more common source was treated with less care than the rarer source(s).

Table 6.1.
Obsidian source data in percent for sites in Island Melanesia, pre-3500 bp.

Site	Island	Period	n	M	T	A	B	REF
GOD*	Manus	1	?	0	0	100	0	1
FHC	N.Britain	1	3	67	33	0	0	2
FAO	N.Britain	1	10	0	100	0	0	2
FAP	N.Britain	1	2	0	100	0	0	2
FAQ	N.Britain	1	10	0	100	0	0	2
FRI	N.Britain	1	8	0	100	0	0	2
FRL	N.Britain	1	11	0	100	0	0	2
FRK	N.Britain	1	3	100	0	0	0	2
FOF	Arawe	1	29	19	81	0	0	2
FLE	Arawe	1	6	0	100	0	0	2
FLF	Arawe	1	7	43	57	0	0	2
FLQ	Arawe	1	3	0	100	0	0	2
EFR	N.Ireland	1	43	75	25	0	0	2
EFS	N.Ireland	1	49	75	25	0	0	2
EAS	N.Ireland	1	1	0	100	0	0	3
EFR	N.Ireland	1	166	5	95	0	0	4
EFP	N.Ireland	1	15	13	80	7	0	5
DFV	Nissan	1	23	17	83	0	0	6

* All the obsidian at Pamwak is Admiralties derived, but at this early date the majority derives specifically from Pam sub-source.
n – number of flakes sourced at each site; M – Mopir source; T – Talasea source; A – Admiralty source; B – Banks source; REF – reference number: 1. (Fredericksen 1997a), 2. (Summerhayes et al. 1998), 3. (Marshall and Allen 1991), 4. (Gosden and Robertson 1991), 5. (Rosenfeld 1997), 6. Chapter five, this thesis.

Table 6.2.
Obsidian source data in percent for sites in Island Melanesia, 3500–1500 bp.

Site	Group	Period	n	M	T	A	B	REF
GOD*	Manus	2	?	0	0	100	0	1
SDP	D.of York	2	72	10	36	54	0	2
DGD2	Nissan	2	16	0	25	75	0	3
DFF	Nissan	2	27	0	48	52	0	3
ECA	St Matthias	2	?	0	48	52	0	4
ECA	St Matthias	2	?	0	32	68	0	4
ECA	St Matthias	2	?	0	42	58	0	4
ECB	St Matthias	2	?	0	41	59	0	4
EHB	St Matthias	2	?	0	26	74	0	4
EKO	St Matthias	2	37	0	4	96	0	4
RF2	Reef	2	18	0	72	22	6	3
TK1	Tikopia	2	5	0	0	60	40	3
SAC	Watom	3	792	0	32	68	0	3
SAD	Watom	3	7	0	14	86	0	3
ELS	N.Ireland	3	67	0	10	90	0	3
ELT	N.Ireland	3	55	0	4	96	0	3
DAF	Sohano	3	178	0	0	100	0	3
RF6	Reef	3	5	0	80	0	20	3
TK36	Tikopia	3	2	0	0	0	100	3
TKTP	Tikopia	3	1	0	0	0	100	3
FAO	N.Britain	2–4	10	0	100	0	0	5
FAQ	N.Britain	2–4	5	0	100	0	0	5
FEA	N.Britain	2–4	28	0	100	0	0	5
FRI	N.Britain	2–4	15	0	100	0	0	5
FRL	N.Britain	2–4	5	0	100	0	0	5
FSZ	N.Britain	2–4	25	0	100	0	0	5
FGT	N.Britain	2–4	16	19	81	0	0	5
FHC	N.Britain	2–4	1	0	100	0	0	5
FNY	Arawe	2–4	33	0	97	3	0	5
FOF	Arawe	2–4	12	0	100	0	0	5
FOH	Arawe	2–4	146	0	99	1	0	5
FOJ	Arawe	2–4	74	99	0	1	0	5
FOL	Arawe	2–4	6	0	100	0	0	5
FFS	Arawe	2–4	19	39	61	0	0	5
FFT	Arawe	2–4	27	14	86	0	0	5
FLB	Arawe	2–4	5	60	40	0	0	5
FLF	Arawe	2–4	22	23	77	0	0	5
FLQ	Arawe	2–4	29	21	79	0	0	5
EAS	N.Ireland	4	3	0	67	33	0	6
SDI	Watom	4	41	0	93	7	0	3
DGW	Nissan	4	31	0	10	90	0	3
TKTP	Tikopia	4	1	0	0	0	100	3

*** All the obsidian at Pamwak is Admiralties derived, but by this period Lou Island sub-source is also exploited.**

n – number of flakes sourced at each site; M – Mopir source; T – Talasea source; A – Admiralty source; B – Banks source; REF – reference number: 1. (Fredericksen 1997a), 2. (White and Harris 1997), 3. Chapter five, this thesis, 4. (Kirch et al. 1991), 5. (Summerhayes et al. 1998), 6. (Marshall and Allen 1991).

Table 6.3.
Obsidian source data in percent for sites in Island Melanesia, post-1500 bp.

Site	Island	Period	n	M	T	A	B	REF
TK1	Tikopia	4	1	0	0	0	100	1
FAO	N.Britain	4–5	9	0	100	0	0	2
FAQ	N.Britain	4–5	5	0	100	0	0	2
FRI	N.Britain	4–5	10	0	100	0	0	2
FRL	N.Britain	4–5	5	0	100	0	0	2
FHC	N.Britain	4–5	36	6	94	0	0	2
FGT	N.Britain	4–5	9	33	67	0	0	2
FSS	N.Britain	4–5	11	0	100	0	0	2
FOF	Arawe	4–5	4	25	75	0	0	2
FOH	Arawe	4–5	37	5	95	0	0	2
FOJ	Arawe	4–5	6	0	100	0	0	2
FON	Arawe	4–5	53	2	98	0	0	2
FST	Arawe	4–5	13	8	92	0	0	2
FFS	Arawe	4–5	13	17	83	0	0	2
FFT	Arawe	4–5	12	56	44	0	0	2
FLB	Arawe	4–5	4	0	100	0	0	2
FLE	Arawe	4–5	11	36	64	0	0	2
FLF	Arawe	4–5	11	36	64	0	0	2
FLQ	Arawe	4–5	15	40	60	0	0	2
SEE	D.of York	5	122	2	45	53	0	3
EAS	N.Ireland	5	6	0	0	100	0	4
EKU	St Matthias	5	?	0	1	99	0	5
EKS	St Matthias	5	9	0	0	100	0	5
DIZ	Nissan	5	17	0	12	88	0	1
DGD3	Nissan	5	3	0	33	67	0	1
DGD2	Nissan	5	19	0	32	68	0	1
DGW	Nissan	5	9	0	22	78	0	1
DFV	Nissan	5	-14	0	93	7	0	1
TK1	Tikopia	5	3	0	0	0	100	1

n – number of flakes sourced at each site; M – Mopir source; T – Talasea source; A – Admiralty source; B – Banks source; REF – reference number: 1. Chapter five, this thesis, 2. (Summerhayes et al. 1998), 3. (White and Harris 1997), 4. (Marshall and Allen 1991), 5. (Kirch et al. 1991).

Table 6.4.
Obsidian source data in percent for sites in Island Melanesia, undated.

Site	Island	Period	#	M	T	A	B	REF
SDP	D.of York	?	136	10	57	33	0	1
SDP	D.of York	?	58	0	10	90	0	1
SEE	D.of York	?	92	1	93	6	0	1
SEE	D.of York	?	14	0	93	7	0	1
EPE	N.Ireland	?	34	?	94	6	0	2
EPG	N.Ireland	?	27	?	100	0	0	2
EPK	N.Ireland	?	70	?	78	22	0	2
EPQ*								
EPR*	N.Ireland	?	29	?	96	4	0	2
EPS*								
EQA	N.Ireland	?	10	?	40	60	0	2
EQH	N.Ireland	?	34	?	85	15	0	2
EQB	N.Ireland	?	70	?	87	13	0	2
EQC	N.Ireland	?	13	?	64	36	0	2
EQD	N.Ireland	?	13	?	31	69	0	2
EQE	N.Ireland	?	50	?	62	38	0	2
EQF	N.Ireland	?	60	?	74	26	0	2
EPZ	N.Ireland	?	15	?	100	0	0	2
EAB2	N.Ireland	?	22	0	91	9	0	3
EKN	St Matthias	?	?	0	14	86	0	4
EKP	St Matthias	?	8	0	0	100	0	4
DES	Nissan	?	9	0	0	100	0	3
DAA	Sohano	?	7	0	0	100	0	5
SZ10	Santa Cruz	?	14	0	36	14	50	6

* The data for these three sites were reported together.
n – number of flakes sourced at each site; **M** – Mopir source; **T** – Talasea source; **A** – Admiralty source; **B** – Banks source; **REF** – reference number: 1. (White and Harris 1997), 2. (White 1997), 3. Chapter five, this thesis, 4. (Kirch et al. 1991), 5. (Wickler 1995), 6. (Green 1976).

Table 6.5.
For each site listed, the data are: time period(s) for which the obsidian is derived, the mean weight in grams for flakes from the source under which they are listed, and the number of pieces from which that mean is calculated. Those sites listing multiple periods are groupings across the listed periods.

Site	Period	Talasea	n	Admiralties	n	Other	n
SDI	2,3,4	1.76	42	0.74	4	-	0
SDI	4	1.83	38	0.65	3	-	0
SAC	3	0.58	253	0.38	539	-	0
SAD	3	0.64	1	1.17	6	-	0
EAB2	?	0.43	20	0.98	2	-	0
ELS	3	0.69	7	0.67	60	-	0
ELT	3	0.14	2	0.53	53	-	0
DES	?	-	0	2.53	9	-	0
DFF	2	0.27	13	0.39	14	-	0
DFV	1,5	0.27	32	1.91	1	0.36*	4
DFV	1	0.23	19	-	0	0.36*	4
DFV	5	0.33	13	1.91	1	-	0
DGD2	2,3,4,5	0.34	11	0.83	29	-	0
DGD2	2	0.25	4	0.73	12	-	0
DGD2	5	0.46	6	1.09	13	-	0
DGD3	5	0.13	1	0.36	2	-	0
DGW	4,5	0.36	5	0.51	35	-	0
DGW	4	0.40	3	0.53	28	-	0
DGW	5	0.30	2	0.46	7	-	0
DIZ	5	1.22	2	3.15	15	-	0
DAA§	?	-	0	1.27	7	-	0
DAF	3	-	0	2.16	178	-	0
SZ10¥	?	2.99	5	0.98	2	1.76†	7
RF6	3	0.51	4	-	0	0.53†	1
RF2	2	1.22	13	0.85	4	1.11†	1
Tikopia+	2,3,4,5	-	0	0.99	3	1.52†	17

? Dating uncertain
* Mopir source
† Banks source
+ Sites TK1, TK4, TK36 and the test pits combined
§ (Wickler 1995)
¥ (Green 1976)

Figure 6.1.
The density range observed for obsidian from each of the four main sources of obsidian in Melanesia. Talasea (2.345-2.363), Lou (2.379-2.403) and Banks (2.421-2.469) obsidians are all distinct in their densities. All three overlap with the wide range of variation seen in Fergusson (2.353-2.430) obsidian. After Ambrose (n.d.).

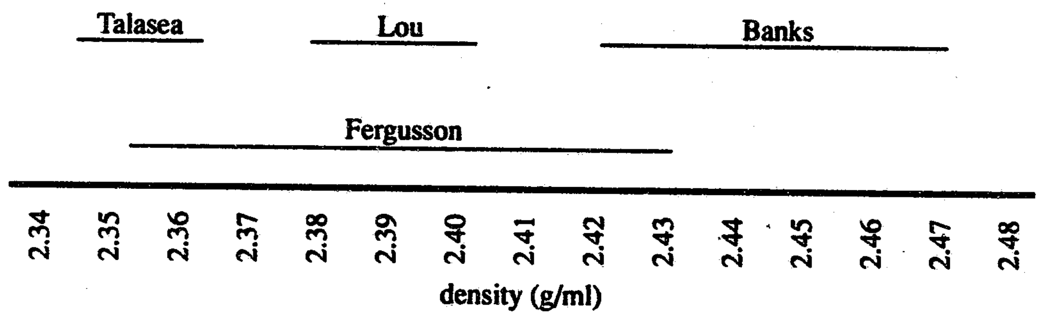


Figure 6.2.
Obsidian source exploitation per time period.

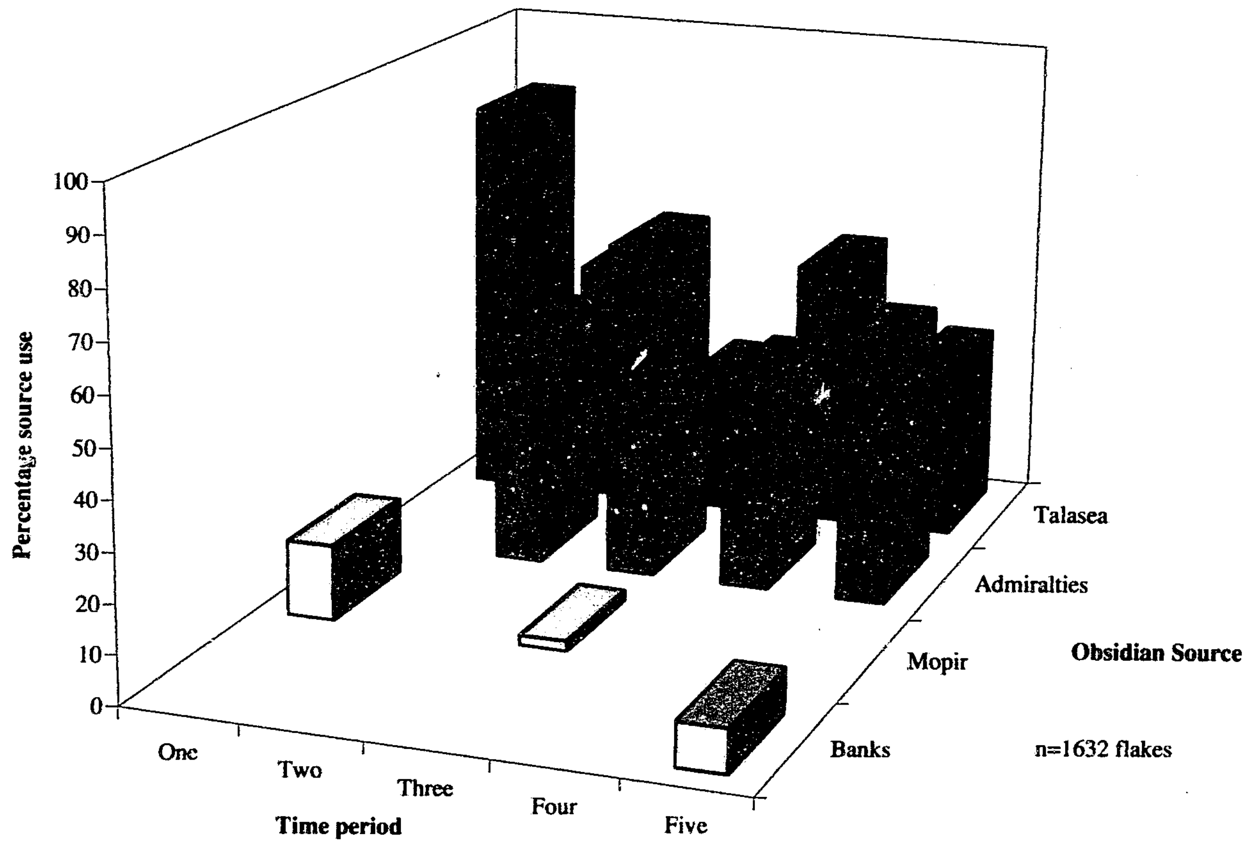


Figure 6.3.
Obsidian source exploitation per island.

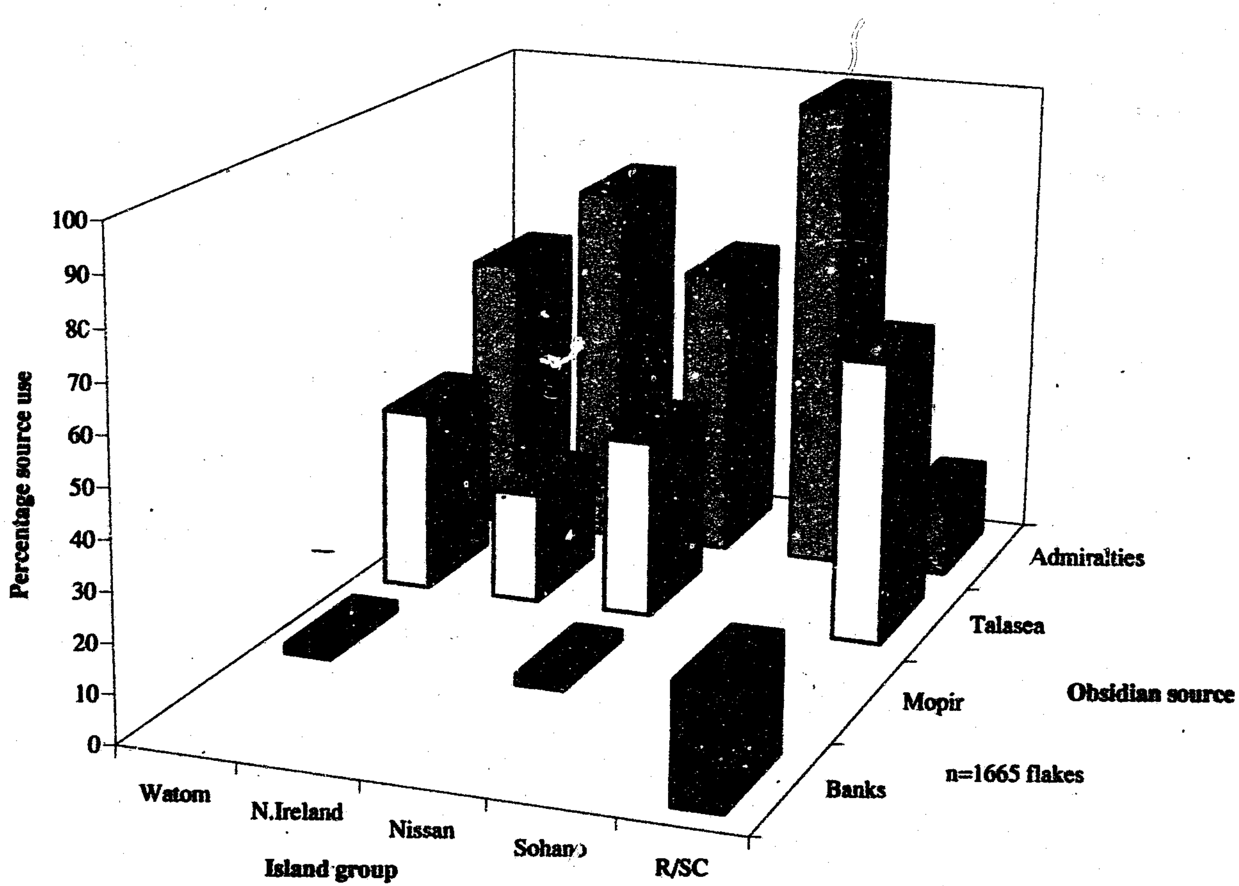


Figure 6.4.
Obsidian exploitation for each source and island group per time period.

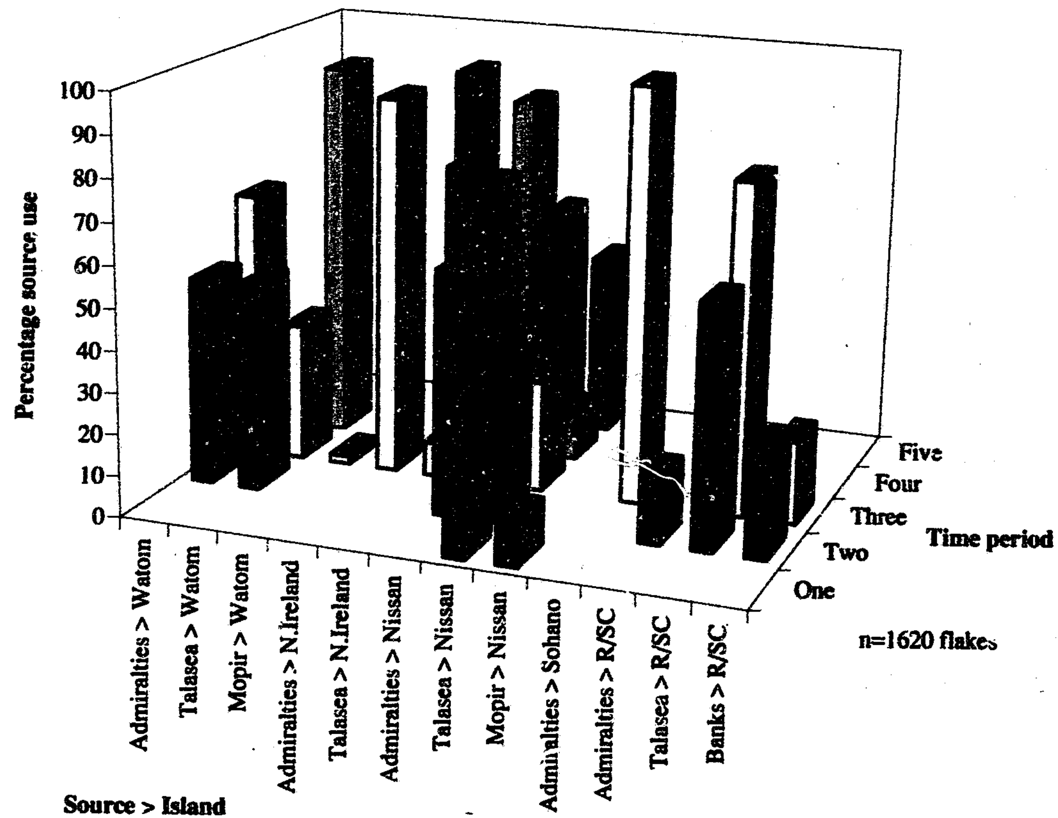
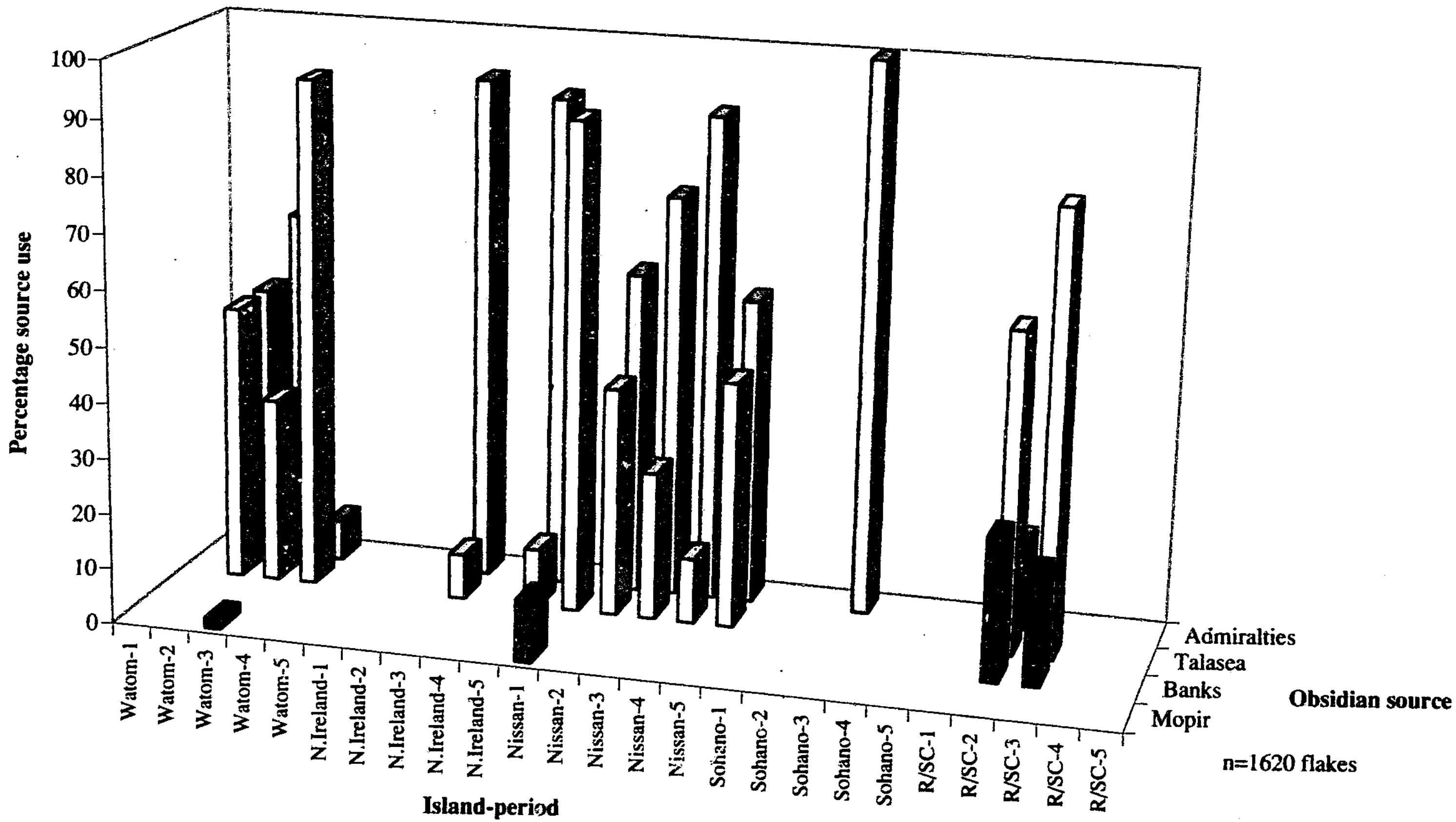


Figure 6.5.
Obsidian exploitation for each island group and time period.



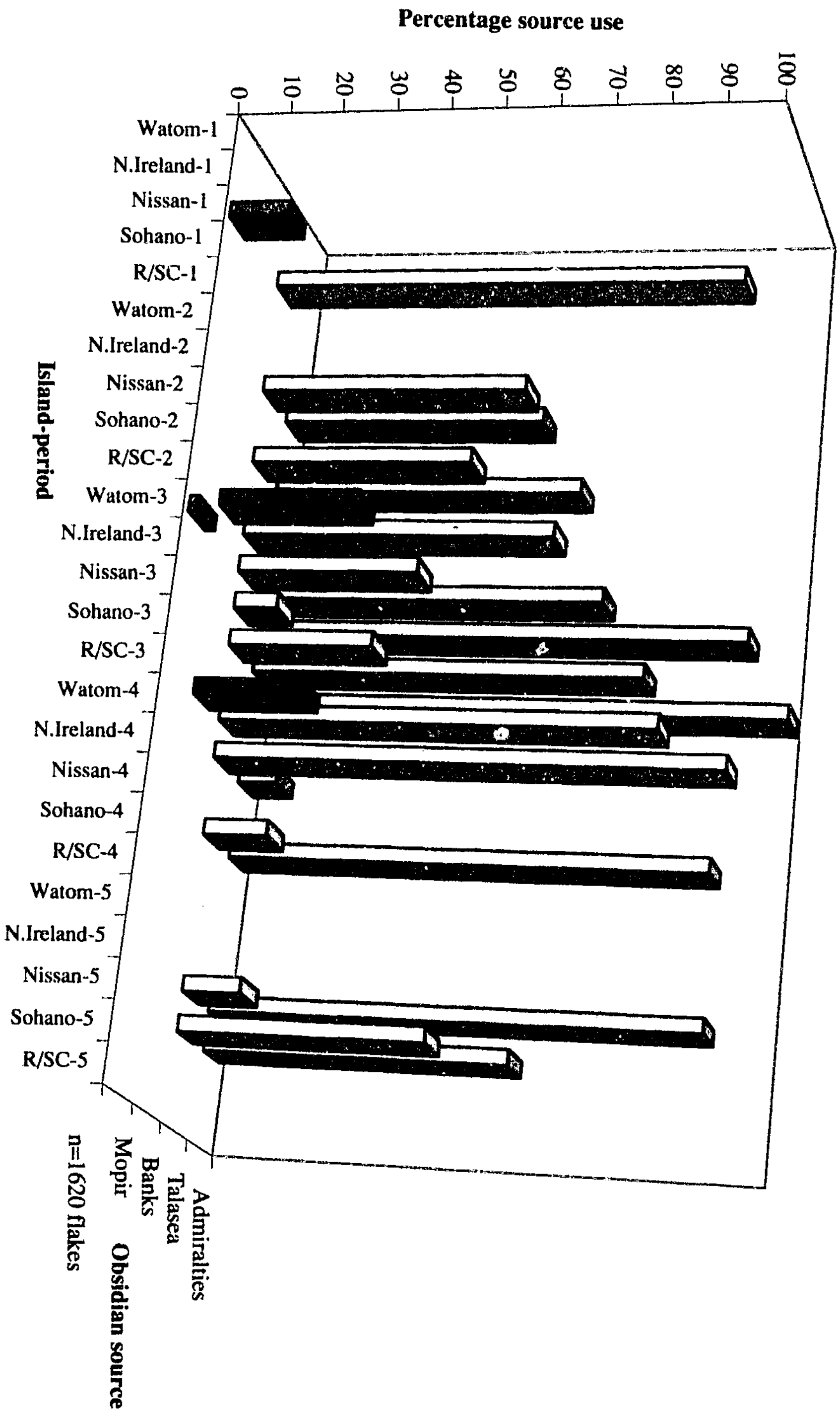


Figure 6.6.
Obsidian exploitation for each period and island group.

Figure 6.7.
Obsidian exploitation on Nissan.

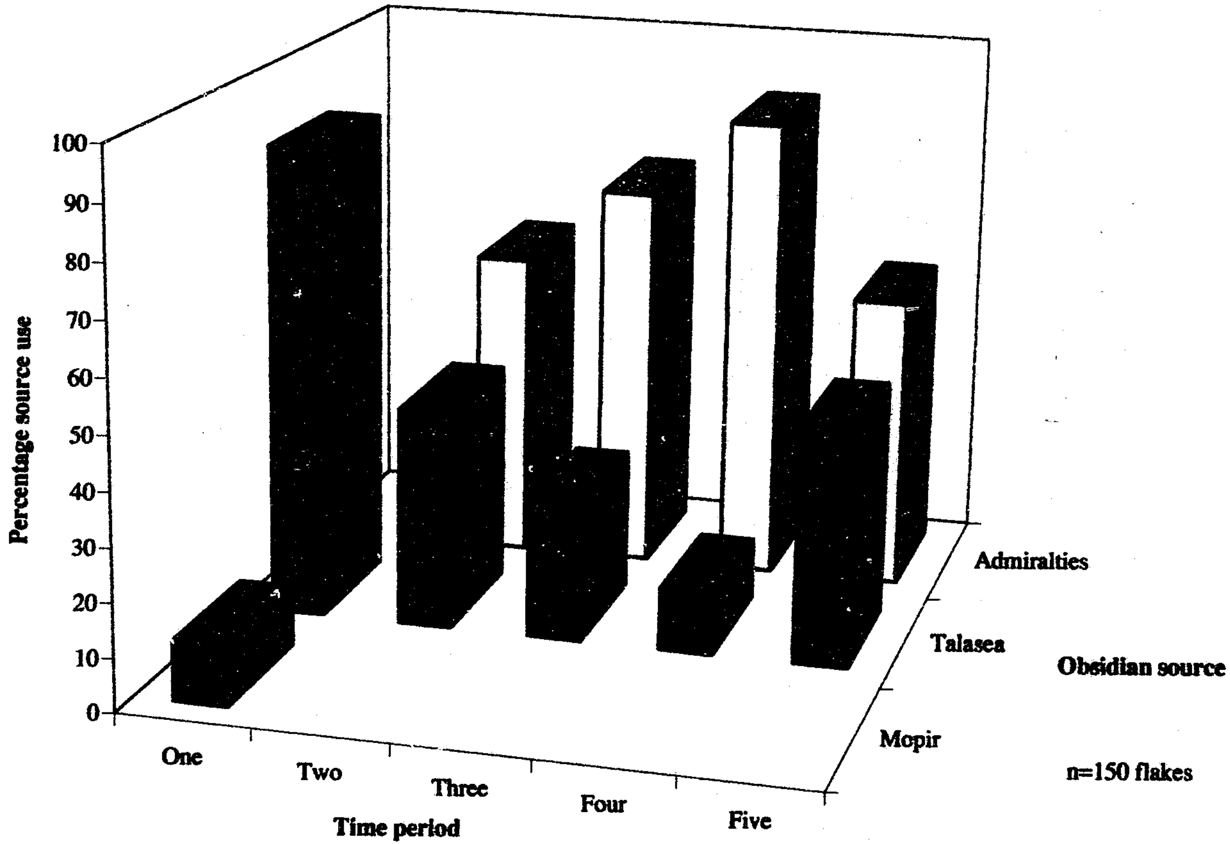
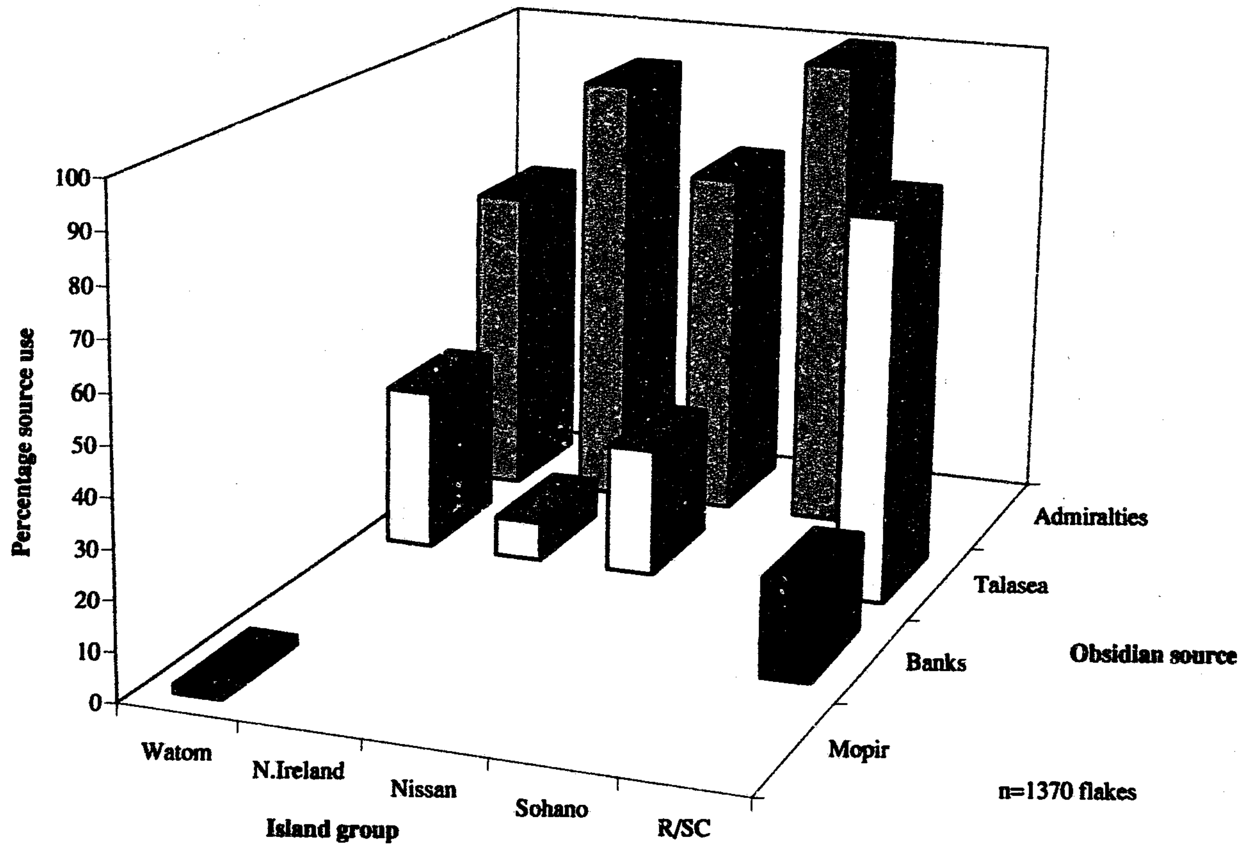


Figure 6.8.
Obsidian exploitation during period three.



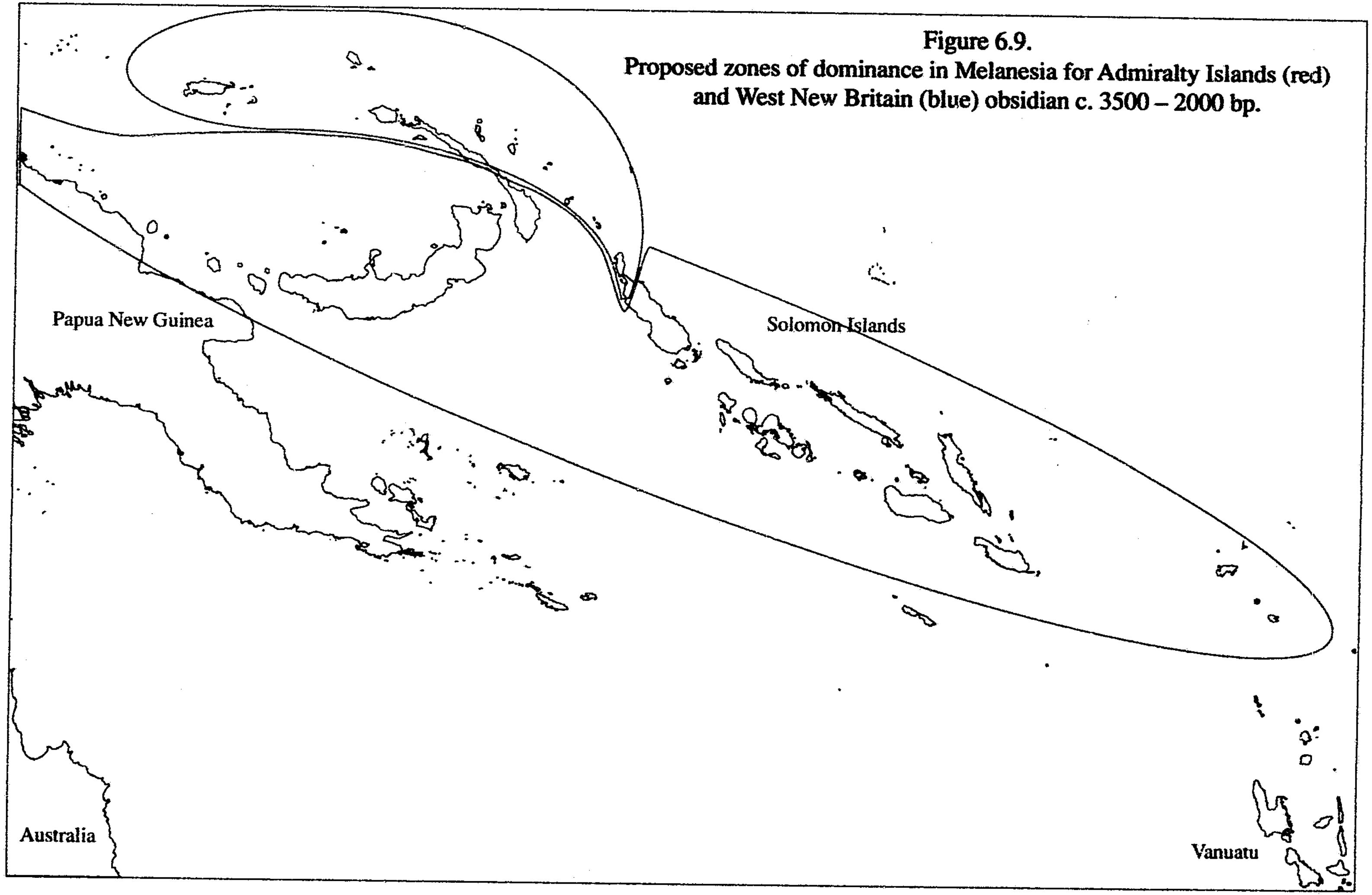


Figure 6.9.
Proposed zones of dominance in Melanesia for Admiralty Islands (red) and West New Britain (blue) obsidian c. 3500 – 2000 bp.

§7

Exploitation of obsidian resources

Chapter seven continues with the presentation of methods and results begun in the previous chapter. Two related topics that are concerned with the relative importance of obsidian in a site are considered. First the abundance of obsidian in the archaeological sites is examined as a means of determining the intensity of obsidian use in each site. Then an investigation of reduction processes reveals means (or more correctly the lack thereof) of economising the obsidian in a given site. A separate look at bipolar flaking is made because of the potential importance of this reduction process in Island Melanesia.

OBSIDIAN ABUNDANCE

I looked at several aspects of obsidian abundance at each site in order to examine variations in resource exploitation across the region (especially with regard to distance from source). I refer to the density of obsidian deposits within a site as abundance to avoid confusion with the density (i.e., specific gravity) of a piece of obsidian as discussed in chapter six. In considering obsidian abundance versus distance from source I generally refer to source in the singular, but of course there are two main source areas situated some distance apart from each other. I was able to ignore the dual source problem because there was no difference between the Admiralties and Talasea sources in this regard. It does not appreciably affect the shape of the resultant graph line whether one uses: distance from Talasea for the Talasea pieces and distance from Admiralties for the Admiralties pieces; or distance from Talasea for all pieces; or distance from Admiralties for all pieces; or average distance from both sources for all pieces—so I chose the latter.

Calculation of abundance for each site in terms of mass of obsidian recovered per cubic metre of excavated earth is presented in figure 7.1. The sites are grouped by island (marked by the vertical red lines and red text) and placed on the X axis according to the distance from the source for each island. The range of obsidian abundance in these excavations runs from a low of less than 1 g/m^3 at DGD3-5 to a high of nearly 75 g/m^3 at SAC-3.

In fact, SAC-3 has so much obsidian in it that it greatly skews the mean value upwards (since SAC-3 represents more than 50% of the raw number of flakes). The simple mean (also known as the weighted mean) is derived by calculating the total obsidian mass across all sites (nearly five kilograms) divided by a total excavation volume across all sites (208 cubic metres). The simple mean, labelled as "Mean 1" on figures containing individual site data, is 23.8 g/m^3. Only three sites, ELT-3, SAC-3 and SZ8-2, have above average abundances. The unweighted mean is a "mean of means" which is calculated by summing the average flake abundance of each site and then dividing this number by the total number of sites. The unweighted mean of 13.0 g/m^3 provides a better division between the obsidian-rich sites and the obsidian-poor ones being independent of the size of each site. It is labelled as "Mean 2" in the figures containing individual site data. This latter figure is probably slightly too low given the high number of single test-pit sites in the sample; test-pits that by their random location over a site are often low in abundance compared to more fully excavated sites.

I decided to use the weighted and unweighted means to divide the sites into three categories in each of the abundance tests. I did this because it proved no less meaningful than other measures of central tendency I considered, it was easy to calculate, most readers will be familiar with the concept of a mean, and since trends would be obvious in the graphical presentations of the data, I valued the least complicated approach for discussing these graphs.

Those sites with abundance above the weighted mean of 23.8 g/m^3 are obsidian-rich, those below the unweighted mean of 13.0 g/m^3 are obsidian-poor, and those in between are of medium abundance. The high abundance sites are found on Watom, New Ireland and in the Reef/Santa Cruz (ELT-3, SAC-3, and SZ8-2). Medium abundance sites are found on all four islands (at SAD-3, ELS-3, SDI-2, SDI-4, DGW-4, and RF2-3). The low abundance sites also are distributed across the four island groups (sites: EAB1 (all five phases), SDI-3, RF6-3, DFV-1, DFV-5, DGW-5, DGD2 (all four phases), DGD3-5 and DFF-2).

Regardless of how the sites are divided, using the two means listed above, median and quartiles, or mode, there is no readily obvious trend in these data. Some of the sites closer to the source are low abundance and some of the sites further away are medium or high abundance. This appears more like a random scatter however. With the same data replotted as figure 7.2, there is some order to the chaos. Using the unweighted mean for each of the four island groups reveals a very steep fall-off for the three closest islands. The Reef/Santa Cruz group do not conform to this trend by exhibiting the second highest mean.

Figure 7.3 represents the same data organised by time period (same mean values as for fig 7.1). One high abundance site, SZ8-2, falls in period two, while the other two are in period three. The medium abundance sites are also distributed through time, with one in period two, two in period three, and two more in the period four. Higher abundance obsidian deposits are more common in Lapita era sites than in sites either before or after. As there are more Lapita era sites however this is not particularly surprising. Redisplaying the data from figure 7.3 as figure 7.4, where the sites from each time period were averaged produces a very symmetrical plot of abundance. Starting from a low abundance in period one of 5.0 g/m^3 and finishing at a similarly low abundance in period five of 4.7 g/m^3, the peak of 22.2 g/m^3 is reached during period three. This four fold increase in abundance cannot be solely attributed to site SAC because the unweighted mean ensures it contributes equally to the outcome along with the other, smaller sites.

Figure 7.5 is a variation on figure 7.1; the X axis is the same, but the Y axis has been changed from g/m^3 to pieces/m^3. This would produce different results from the above if a site had especially large or small size flakes. Again SAC-3 is the dominant influence on the mean value of 23.3 pieces/m^3 (marked as "Mean 1"). Only ELT-3 and SAC-3 lie above this mean. The unweighted mean is 16.2 pieces/m^3 (marked as "Mean 2"). Any site between these two values is, by my definition, a medium abundance site. These are: DGD2-3, and DGW-4, SAD-3, ELS-3 and EAB1 (periods one through four). The other seventy-five per cent of the sites are low abundance. As was shown in figure 7.1, there is a fairly even spread of sites through the categories. The Reef/Santa Cruz group is not represented in either the medium or high abundance category.

Figure 7.6 displays a fall-off curve of the sort I expected to find in this assemblage—the further the obsidian travels from the source, the less of it is found. In this case it is fewer flakes per cubic metre of excavation. Why it should be numbers of flakes that display this behaviour and not grams of obsidian does not become apparent until more data are considered, and this issue will be discussed in chapter eight. Given that the transport of obsidian in Melanesia is primarily by sea, but with occasional opportunity for land based travel segments as well, it is possible that the relationship between amount recovered and distance from source is based on the difficulty involved in transport. In sailing, a non-linear factor may affect the distance. Replotting this figure (or indeed any of the distance based figures from this section) with log distance on the X axis does not produce a linear fall-off model, in fact it hardly alters the shape of the original plot. Ultimately tens of experimental graphs were plotted using semi-log, log-log (both natural and base 10), exponential and other linear and non-linear transformation functions revealing that there was no easy way to render the plot in a linear manner. That it is possible to transform this data so that it produces a linear plot is addressed in chapter nine.

Figure 7.7 displays figure 7.5 data rearranged into chronological order. Similar to figure 7.3, figure 7.7 puts the emphasis on time rather than distance. None of the sites in time periods one, two or five contain medium or high abundance deposits. There is much more diversity evident after 2700 bp than before this date. If, however, the time period means are plotted for abundance in terms of flake numbers (fig 7.8) the resulting graph is quite similar to the plot of abundance in terms of mass (fig 7.4).

Given that abundance data are so dependent on both the type of site and the luck of the excavation in finding the obsidian deposits in a site, it is also necessary to consider the flake size. Figures 7.9 and 7.10 display the mean flake mass at each site. Figure 7.9 shows the sites arranged on the X axis according to location, while figure 7.10 shows the sites arranged according to date. The weighted mean is 1.02 g/flake and the unweighted mean is 0.80 g/flake. This measurement of size of flake by island group (fig 7.9) reveals the opposite to what I expected; while there are large flake sites on all islands, it is in the distant Reef/Santa Cruz group that the obsidian pieces are all twice the average size.

The sites on Nissan cover the whole range of flake size; DGD2-5 with big flakes, DGD2-4 with medium sized flakes, and the remainder of Nissan sites have small flakes. On Watom, sites SDI-2 and SDI-4 contain large flakes, as does SAD-3. Site SDI-3 is just under the large flake size cut-off point. SAC-3, the site that had the large numbers of flakes in high abundance that was so influential to the means discussed above, actually has quite small average flake size. EAB1-1 is the only site with large flake size on New Ireland. Sites EAB1-5 and ELS-3 contain flakes in the medium size range, between the two mean values. The remaining sites on New Ireland contain small flakes.

With each island group averaged and plotted against distance from source (fig 7.11), similar observations can be made. The Reef/Santa Cruz group sites have much larger mean flake size than sites on the other three islands, with New Ireland and Nissan sites being very close to each other and Watom sites falling in between.

Figure 7.10 reveals an even distribution of sizes through time. Different sites in each of the five time periods represent both extremes of small and large average flake size. On the basis of these data, it is not possible to highlight any time dependent changes in th[illegible]gion. Examination of figure 7.12 confirms this impression—It is almost a flat line from period one through to period five with a slight peak in period two, during Lapita times. Average flake size remains constant throughout prehistory.

To address the concern regarding weighted versus unweighted means, and the influence they might have on the observations of this section, two final figures are presented (7.13 & 7.14). Figure 7.13 combines the three plots of weight and number based abundance and flake size (from figs 7.4, 7.8 & 7.12) versus time period, but with the numbers based on simple or weighted mean rather than the unweighted mean. The influence of site SAC is obvious in the higher values of the period three peak for the g/m^3 and $\#/m^3$ plots, but all three lines display identical shape to their unweighted mean counterparts. Figure 7.14 combines the similar three measures (from figs 7.2, 7.6 & 7.11) as in figure 7.13, but plotted against distance from source using weighted mean. Once again the specific value of the data points varies, especially Watom Island which contains site SAC, but the shape of each plot is the same as with the unweighted mean.

REDUCTION STRATEGY

While both the abundance and size of flakes at each site may reflect resource availability, the size of each recovered flake is more directly related to the reduction strategy that the knappers used to create them. The method or methods of production used at a site can then reflect resource scarcity; for example pressure flaking techniques allow more of a core to be turned into useable tool edges than do direct percussion techniques.

Differences noted above in obsidian abundance for each site can be put into a more secure interpretive frame if something is known about the knapping techniques employed. A given site could have a high abundance of flakes because it contains a lot of knapping debitage, or it could contain a lot of utilised flakes. Both of these possibilities and other potential scenarios give a different emphasis to the recovered obsidian.

For reasons introduced in chapter four, the method that I used to determine what reduction strategy was employed had to consider the context of these Melanesian sites. With the general lack of formal tool types, it was not a situation like that found during the European Palaeolithic; of comparing tool types and their frequencies to fit the site into a well-established chronology of stylistic and functional evolution (Bordes 1968). In this case I was dealing with a single tool type and a very informal production 'industry'. The term "expedient technology" describes very well the strategy employed at every site under consideration. I first heard this term during a discussion with Robin Torrence. Other authors working in Melanesia have used different terms to refer to the same phenomenon. For example, Green (1994: 32) refers to "expediently produced" flakes, and Kirch (1997: 32) to tools of "expedient form", while Fredericksen (1994: 159) refers to production with a low level of systematic approach, and Spriggs (1997: 72) to a "casual obsidian technology". Expedient technology is explicitly not used in the narrowly defined manner used in American archaeology. See Shott (1989, 1998) and Young (1994) for more information on the American usage of the term.

The differences being sought, then, were more subtle and perhaps more simple. The first goal was not to determine the style of reduction used, but to determine if the reduction was carried out *in situ* or elsewhere. The second goal was to consider reduction processes themselves. Two papers that dealt with these tasks were considered, both of them using the assemblage as a whole to provide inter-assemblage comparisons rather than intra-assemblage examinations on a flake by flake basis. The first (Schick 1991) contained a section about site lithic profiles produced through a nested sieve procedure, and the second (Jeske and Lurie 1993) was concerned with being able to distinguish between bipolar and regular percussion techniques.

Schick (1991) ran one hundred and seven experimental trials and measured the outcome of each. One hundred and seven obsidian cores were reduced to exhaustion and the resulting flakes and debitage were run through a nested sieve stack to determine the size of each piece produced by the knapping. The raw count for each size class (corresponding to each screen size) was averaged across the run of experiments producing an 'expected' outcome for an individual knapping event. If the counts are changed to proportion and then plotted, the result is an 'expected' curve. This curve represents the average proportion of flakes one would expect to find in each screen size if all the products of a primary reduction episode remained in a site and were then processed through a nested sieve stack. As mentioned in chapter four, Schick was not interested in distinguishing primary production from other reduction processes, rather she was trying to identify sites where post-depositional processes had not altered the original assemblage composition.

My experience with obsidian knapping suggested that Schick's idea could be exploited with newer archaeological deposits where numerous potential reduction processes co-exist. Alternative processes to the primary lithic reduction investigated by Schick include resharpening, blank preparation, blank reduction, and final use. Without repeating similar experiments for each of these alternate "reduction strategies" it is impossible to know what the expected curve for each would look like, but it is predictable that they would not look like primary reduction, and that they will differ from each other.

To determine if primary reduction was the main activity in any of these sites, I had to have my data in a form comparable to that from the nested sieve trials. Running hundreds of carefully labelled pieces of obsidian through a series of nested sieves is, however, a quick way to dissociate the flakes from their provenience data. At best this procedure would obscure the use-wear details, at worst it would break off portions of many pieces. The exact procedure used is listed in the metrical analysis section of chapter eight, but it is necessary to point out now that I produced all of the profiles presented in figures 7.15 through 7.29 without resorting to the use of a series of nested sieves. Instead I used mathematical manipulations of the data.

Figures 7.15 to 7.19 are screen size profiles for each site. Not all sites are represented in each series of figures because not all sites have enough flakes to fit the requirements for that procedure. For purposes of clarity, there are only a few sites on each figure, but they could all be overlaid and all are compared to the same 'expected' curve for reference. One difficulty of this graphical method is the comparison itself. It is obvious when two line plots are grossly different, just as it is obvious when they are virtually identical. Plots that are quite similar are difficult to pass judgement on and there is no goodness-of-fit test criteria for 'eyeball' conclusions. To deal with this, I have tried to fully explain my position for each test and to err on the side of difference (that the two plots are different). Nevertheless, another person might judge certain outcomes differently to myself. Therefore, despite certain problems with the procedure, I have also included a standard statistical test (the Chi Squared test) for comparison purposes.

The X axis gives the lower limit of the screen size classes Schick used. The smallest of Schick's classes is absent from the graph because at no site in my study did I find any flakes in this range (below ten millimetres screen size). I attribute this to the fact that most of these sites had the primary screening completed with quarter inch screens (~6.5 mm) and the most recently excavated remainder used five millimetre screen. The variation in screen sizes equates with the adoption of the metric system in Australia and New Zealand. The earliest excavations (EAB1, SAC, SAD, RL2, RL6 and SZ8) used quarter inch screens for sieving. All the remainder (EAB2, all the Nissan sites, SDI, ELS, ELT, and portions of EAB1, and SAC) used 5 mm screen for sieving. The lack of any flakes in the sub-ten millimetre size class (there should have been some between the five millimetre or quarter inch cut off and the ten millimetre class interval) may reflect on inaccuracies in the method used to calculate these data, or actual absence of any flakes at all. This question is considered below. Schick's totals were recalculated without the smallest class so that the archaeological data and the experimental data could be directly comparable.

The Watom sites, in figure 7.15, reveal no observed profiles like the expected profile. Two sites, SDI-4 and SAD-3, are quite similar to each other and somewhat similar to sites SZ8-2 and RF2-2 (fig 7.19).

In figure 7.16, the New Ireland sites, all the profiles are quite similar to the expected profile. This example highlights the difficulties with graphical comparison methods; how similar must they be to be considered the same. Only site EAB1-1 is similar enough, with only a lack of the five and six centimetre size class to make it questionable. I believe it is representative of a site with on-site reduction represented.

Based on figure 7.17, the Nissan sites are a mixture of activity types. Sites DGW-4 and DGD2-5 are similar to each other and unlike the expected profile. The other four sites are not unlike the expected profile with the larger classes removed. Possible explanations for a profile like this are the reuse of larger pieces, the removal of large pieces to elsewhere, or non-primary reduction (i.e., reduction on blanks or prepared cores).

Figure 7.18 reveals the effects of wave action on the reef-top sites. All of the small pieces are absent, and the larger pieces can be presumed to have been removed in a differential manner; more loss of medium sized pieces and fewer of the larger pieces. DAF-3 on Sohano and DIZ-5 on Nissan have both been affected by wave action. Not only do they not compare to the expected profile, but they do not compare to each other. It is impossible to make any conclusion on the basis of figure 7.18.

Finally, in figure 7.19 the Reef/Santa Cruz sites, there is no similarity between the expected and observed profiles. This indicates a lack of on-site reduction (not its total absence, only that it is not the dominant activity), but does not permit me to conclude what was occurring. Notice how sites SZ8-2 and RF2-2 have nearly identical profiles, while RF6-3 is different again.

Turning attention towards sites of similar chronology rather than island groups results in the next five figures (7.20–7.24). Figure 7.20 displays the two period one sites. Both site profiles are similar in shape to the expected profile, but as in figure 7.16 only site EAB1-1 could be considered the same. Figure 7.21, period two, reveals two pairs of profiles similar to each other but neither is identical to the expected. Sites SZ8-2 and RF2-2 from the Reef/Santa Cruz group are alike, as are sites DGD2-2 and DFF-2 from

Nissan. On figure 7.22, period three, none of the sites look much like each other or the expected curve. Figure 7.23, period four, has site EAB1-4 that is closest in its profile to the expected one, but lacks any large sized flakes. Sites DGW-4 and SAD-4 are quite similar to each other. Finally, figure 7.24 displays the most recent sites (period five), which are unlike each other or the expected profile.

Of all the sites, only EAB1-1 is at all like the expected profile. Many of the others appear as though they might be based on a similar starting profile, with the larger flakes removed. Such activities could be the result of resource scarcity, or the different profile might reflect different reduction activity. To increase the comparability between expected and observed profiles, possibly even to the extent of including the reef-top sites, at the cost of less discrimination I reduced the range of the profiles even further. These "short profiles" deal only with the two most populous classes of data from each site (1 and 2 cm, with exceptions for the two reef-top sites as noted below). The proportions were recalculated using only the chosen classes as the one hundred per cent sum. The result is not really a profile as such, because it has only two data points, but sites with similar slopes potentially share the same reduction sequences.

Figure 7.25, for Watom sites, reveals that none of these sites were primary knapping centres, nor are they much like each other. Figure 7.26, containing the New Ireland sites, shows that all but EAB1-3 could be primary reduction sites. Nissan's sites are compared in figure 7.27. Sites DGD2-2 and DFV-5 are close to the expected curve. Sites DGW-4 and DGD2-5 are like each other. Sites DFV-1 and DFF-2 also group together with their similar observed curves.

Figure 7.28 has two different expected curves. The first is the one covering the two and three centimetre size classes for comparison with site DIZ-5, the second is a four and five centimetre size classification for site DAF-3 because more of the small pieces were removed here. Site DIZ-5 is enough like the Expected-1 curve to suggest that reduction of obsidian could have occurred there. Site DAF-3 is not at all like the expected curves for lower values, but compares quite favourably to the Expected-2 curve; almost identical in fact. I make two conclusions from this: 1- the four centimetre mesh size is probably right on the limit of size sorting ability of ocean activity in that area, and 2- on site reduction probably occurred at DAF-3.

Another possibility that must be mentioned is that site DAF-3 deposits (also true for site DIZ-5) could have been produced by some other process (other than primary reduction activities) and either this process produces overlap with the expected curve in the two size categories examined, or the ocean activities altered the original composition to what appears now. Clearly there is a problem with discrimination using the short profile as the only basis for judgement.

Finally figure 7.29 shows that sites SZ8-2 and RF2-2 are alike as suggested by figure 7.19. None of the Reef/Santa Cruz sites contained primary reduction activities as a major task.

As mentioned above, these graphical comparisons can also be done mathematically with the Chi Squared test (Siegel 1988: 453–461, Mendenhall and Beaver 1991: 494–497). I present this alternative approach for those more comfortable with numbers, but after much exploration of the data set, it was evident that I could not rely on X^2 alone. This parametric method attempts to exactly duplicate the analyses completed with the non-parametric or graphical method. There are too many underlying assumptions made by the X^2 test that may not be met with these data (Y. Pittelkow pers. comm., Stuart and Ord 1987: 506). In other words, it might be perfectly valid to use the Chi Squared test, probably it is acceptable with an unknown error, but exactly what the situation is, is indeterminate.

The Chi Squared tests are presented in table 7.1 with the results summarised in table 7.2. First each site was taken as a whole and a X^2 value calculated. This is reported as Test Statistic 1. The degrees of freedom range from one to six. If each site had sufficiently large numbers of flakes and an even distribution, one would expect all sites to have six or more degrees of freedom. The fact that most have only one or two reveals the skewed distribution present in these sites. Then each site was re-run after reducing the size categories considered to only two (Test Statistic 2 in table 7.1). For most sites those size categories were one centimetre and two centimetres. For the two reef-top sites, different categories needed to be included. In all cases, these categories were the same used in producing the short profiles (figs 7.25–7.29). This procedure was followed to exactly duplicate the approach used in the graphical method above.

In general, the X^2 agrees with the graphical analyses. Sites like SZ8-2, that were so different in profile, have huge critical values from the Chi Squared test. One typical (but breakable) requirement of the Chi Squared is that the expected values of every cell must be equal to or greater than five. Many cells in the table do not meet this criterion, and were discarded with the whole table recalculated to reflect this absence. These small values were discarded rather than used because of the uncertainty regarding the underlying assumptions mentioned above. Critical values for the X^2 test at ninety-five per cent confidence level were compared to each test statistic. Those statistics that exceeded the critical value at ninety-five per cent were then compared to the ninety-nine per cent critical value. The null hypothesis in all cases is that there is no significant difference between the distribution of flake size at the site and in the experimental data. The test hypothesis, therefore, is that there is a difference between the site data and the experimental data. See table 7.2 for the results.

To compare the outcome from the graphical analyses with the X^2 outcomes, we turn to table 7.3. There are six disagreements between the full and short profile graphs and two more responses in the short profiles column because of the reef-top sites. I suggested that the short profile test would be more flexible at the cost of some discriminatory ability which I think has been revealed by the six cases of disagreement; all of which are negative in the case of the long profile and positive in the case of the short profile. I do not believe they should overcome the negative result received in the long profile test. This makes interpretation of the two reef-top sites more difficult as well.

I feel satisfied in saying that the sites which displayed positive correlation with the long expected curve are primary reduction centres, because this is the only task that takes large blocks of obsidian and reduces them to working flakes. These 'sites' are in fact singular—only EAB1-1 passed this test. All I can conclude about the other sites, however, is that they were not obviously primary reduction centres. Any conclusions that can be drawn from these data are left to chapter eight, where all the data can be put together.

The agreement between the two runs of the X^2 tests is good, but not complete—there is one outright difference (site ELS-3) and one difference using the extended confidence interval (site DFV-1). I think both of these reflect the fact that other reduction processes apart from primary reduction will have a portion of their expected curve overlap with the primary reduction one.

A comparison of outcomes between the graphical method and the X^2 is in quite good, but not total, agreement. There are eight disagreements between the long profile test and the Chi Squared test. All of these are again cases of the long profile having a negative outcome and the Chi Squared having a positive result. This points to problems with both techniques. All of the disagreements come from sites with small numbers of obsidian flakes (fewer than sixty). The expected curve has a proportion of less than 0.1 for all size classes above three centimetres. Even in relatively numerous assemblages, the presence or absence of these rare types makes for a very poor diagnostic factor (Hiscock in press). With an 'n' of thirty flakes, the expectation is for fewer than three flakes in each size category above the three centimetre size class. There is not much difference between one and zero in terms of the Chi Squared test, but the graphical comparison makes the difference look larger.

This 'problem' is one of the reasons that the Chi Squared test requires an expected value of five or more in each cell to make the test a valid one (Kendall and Stuart 1979: 455). There are techniques available in statistics that reduce the minimum expected number below five. In the case of these data with the partially unanswered questions about just how well the population fit the requirements of the test, I decided these techniques were inappropriate. Random variations can easily account for a difference of one unit between runs of an experiment. Note that if the expected is only one, that is a one hundred percent difference between expected and observed, whereas if the expected is five, that is reduced to a twenty percent difference. Chi Squared tests are much more straightforward in application with a large 'n'. The test statistic for the Chi Squared test, like most statistical tests, is dependent on the 'n' of the tested sample and tends to correct for these proportional differences. It still remains a fact that for tests with many small expected outcomes, the result of the test is open to error. In comparing a site with fifty flakes against one with five hundred flakes and another with five thousand flakes it becomes obvious that the graphical method lacks the ability to discriminate on the basis of sample size. Somewhere between the graphical comparison and the Chi Squared statistic lies the truth.

While I have established that there are some problems with the short profile test and the limited Chi Squared test in their discrimination abilities, they do agree with each other quite well. Four outright differences and one smaller difference exist. The smaller difference being the result of variable confidence intervals in the Chi Squared test.

BIPOLAR REDUCTION

I felt it was important to include a mention of the Jeske and Lurie (1993) paper for three reasons. First, to highlight their use of blind testing as a means of determining what is possible. Many studies on stone tools are based on specialist examination of traits that are seemingly indistinguishable to the non-specialist. Only through blind testing can the accuracy of a specific technique be assessed. The reality of the archaeological tools can never be known with certainty, but it can for replica assemblages created for the purposes of the blind test. I think this fact sometimes gets lost and is therefore worth restating explicitly.

Second, their use of whole assemblages to characterise a site supports my belief in this approach. The best method of examining an assemblage for characteristics of reduction strategy is to examine the assemblage as a whole (as in the section immediately above). Their research reveals the potential problems of trying to assess reduction processes on a flake by flake basis and extrapolating to the whole group.

Finally, their specific dealings with bipolar reduction provided a new mode of inquiry to be pursued. It has been suggested that bipolar flaking is a key feature in certain Melanesian obsidian assemblages (White 1972, White, Downie and Ambrose 1978, Goulding 1987, Freslov 1989, Kirch 1997: 233, White and Harris 1997: 104). Not that it is necessarily the primary technique, nor even a major one, but that many cores are found further reduced from their smallest size (the smallest practical size for direct percussion flaking) via bipolar techniques.

I was initially sceptical of this on the basis of the obsidian flakes I had examined because I noticed very few markers of bipolar reduction and only one clear bipolar scar on a core fragment. This scepticism was further reinforced by the observations of others. Fredericksen (1994: 101) cited a lack of bipolar reduction in the extensive assemblages from Manus province he analysed. Specht and Koettig (1981) reported a large flaking centre at Talasea with no formal tool types and only a rare occurrence of any evidence of bipolar reduction. Patterson and Sollberger (1976) concluded that evidence of bipolar reduction often occurs towards the end of a core's production by accident because the small core must be supported somehow and occasionally this support causes a bipolar incident. In other words bipolar flakes can be produced without intentional utilisation of the technique, when a core is reduced to its smallest possible state.

Bipolar technique uses a hammer and an anvil (both are often rocks, but can be other hard materials) to produce flakes. The core rests on the anvil and the hammer is struck against the top of the core. Since it is resting on an anvil and not supported only by the knapper's hand, the core can be smaller than would otherwise be possible. The classic evidence of bipolar technique is two bulbs of percussion on the resultant flake and corresponding negative features on the core. One bulb results from the hammer and the other from the rebound shock off the anvil.

My knapping experience suggests that the classic two bulb flakes do not appear very often when using this technique. Other, more obscure, traits of bipolar knapping may have passed unnoticed by this investigation as I was not specifically looking for them. These other traits mostly relate to the morphology of the debitage. The scarcity of the most diagnostic trait also meant that perhaps bipolar flaking was a contributor to Melanesian tools and I had missed the evidence. More likely is the case that bipolar reduction is not universal amongst Melanesian assemblages.

Jeske's blind tests suggest that when dealing with individual flakes even a very experienced analyst diagnoses bipolar reduction about on a par with random chance; i.e., when seeking to separate flakes produced via bipolar technique from ones produced some other way, guessing is as good as experience. Looking at portions of the reduction sequence (such as that found in the vast majority of archaeological cases) did not enable Lurie to determine if bipolar flaking had been employed. Only when she had access to the entire reduction sequence and looked at it in statistical terms could she determine when bipolar flaking had been used to reduce a core (Jeske and Lurie 1993: 146).

Some of the discussion regarding reduction processes involved consideration of the core as opposed to the flakes removed from the core. I have made no reference above to the core pieces found at these sites. This requires some explanation. The sites on Buka and Nissan definitely contained no cores. Site SAC on Watom contained several probable cores, but they were in such degraded state (both water rolling and surface pitting) that it was difficult to even determine if any particular one was a core or just a block of obsidian. Other sites have had cores noted in the lithic assemblage. I made a decision to ignore the few cores present in some of the sites—which seems erroneous in hindsight—because I was seeking comparable data classes across all the sites.

SUMMARY

Chapter seven has presented two main analyses, both dealing with the reduction sequence of flake tools. Both also utilised innovative methods of handling the obsidian pieces. Two measurements of abundance, by mass and count, were combined with measurement of flake size to characterise the amount of obsidian present at each site. The expectation was to discover a decrease in abundance with increasing distance from source locations. Generally, this was found to be the case. Changes in flake size across the region were noted; when combined with the reduction strategy analysis these allow some conclusions to be drawn (see chapter nine).

The examination of reduction strategy used a novel approach designed to minimise damage to the artefacts and to lessen the time required in the process. Graphical profiles of each site were compared to a theoretical, experimentally derived profile.

The inclusion of the Chi Squared test reinforced most of the observations made with the graphical method. Finally, a separate but related discussion regarding the use of bipolar reduction was put forth. No indications of bipolar flaking were noted in the assemblages examined. The examination of reduction sequence leads directly to the topics of the next chapter: tool shape and tool function.

Table 7.1.
Chi Squared values for each site. Results of the tests are in table 7.2.

Site	d.o.f.	Size classes	Test statistic 1	d.o.f.	Size classes	Test statistic 2
SZ8-2	5	1-6	386.76	1	1-2	367.82
SDI-4	2	1-3	55.48	1	1-2	51.18
SAD-4	3	1-4	48.22	1	1-2	29.84
SAD-3	5	1-6	455.21	1	1-2	359.04
RF6-3	1	1-2	12.69	1	1-2	3.15
RF2-2	5	1-6	715.55	1	1-2	644.92
ELT-3	2	1-3	7.55	1	1-2	0.41
ELS-3	3	1-4	13.54	1	1-2	2.80
EAB1-4	2	1-3	5.34	1	1-2	0.00
EAB1-3	1	1-2	2.29	1	1-2	3.38
EAB1-2	-	n/a	-	1	1-2	0.00
EAB1-1	1	1-2	0.77	1	1-2	0.82
DIZ-5	1	2-3	30.25	1	2-3	29.25
DGW-5	-	n/a	-	1	1-2	0.00
DGW-4	2	1-3	16.49	1	1-2	9.54
DGD2-5	1	1-2	5.75	1	1-2	3.90
DGD2-4	-	n/a	-	1	1-2	10.67
DGD2-3	-	n/a	-	1	1-2	0.48
DGD2-2	1	1-2	1.45	1	1-2	0.24
DFV-5	1	1-2	0.89	1	1-2	0.24
DFV-1	2	1-3	12.76	1	1-2	5.83
DFF-2	1	1-2	5.55	1	1-2	2.84
SAC-3	6	1-7	988.27	1	1-2	364.30
DAF-3	5	2-7	2464.35	1	4-5	1.99

Statistic 1 is calculated for all flake size classes present at each particular site (where $E \geq 5$). Statistic 2 is calculated for only two flake size classes per site. An entry of '-' indicates sites with a distribution too small to run a Chi Squared test. See text for explanation. d.o.f. = degrees of freedom.

Table 7.2.
Summary of outcomes from the Chi Squared test series. The null hypothesis: The archaeological assemblage comes from the same population as the experimentally derived assemblage.

Site	Accept hypothesis? run #1	Accept hypothesis? run #2
SAC-3	no	no
SAD-3	no	no
SAD-4	no	no
SDI-4	no	no
ELS-3	no	yes
ELT-3	no†	yes
EAB1-1	yes	yes
EAB1-2	n/a	yes
EAB1-3	yes	yes
EAB1-4	yes	yes
DFF-2	no†	yes
DFV-1	no	no†
DFV-5	yes	yes
DGD2-2	yes	yes
DGD2-3	n/a	yes
DGD2-4	n/a	no
DGD2-5	no†	no†
DGW-4	no	no
DGW-5	n/a	yes
DIZ-5	no	no
DAF-3	no	yes
SZ8-2	no	no
RF2-2	no	no
RF6-3	yes	yes

† For these cases the null hypothesis is rejected at 95% confidence level but is accepted at 99% confidence level.

Table 7.3.
Comparison of results of the four procedures used to identify on-site reduction.

Site	Long Profile	Short Profile	Chi Squared test	Limited Chi Squared test
SAC-3	≠	≠	≠	≠
SAD-3	≠	≠	≠	≠
SAD-4	≠	≠	≠	≠
SDI-4	≠	≠	≠	≠
ELS-3	≠	◊	≠	◊
ELT-3	≠	◊	◊†	◊
EAB1-1	◊	◊	◊	◊
EAB1-2	≠	◊	n/a	◊
EAB1-3	≠	≠	◊	◊
EAB1-4	≠	◊	◊	◊
DFF-2	≠	≠	◊†	◊
DFV-1	≠	≠	≠	◊†
DFV-5	≠	◊	◊	◊
DGD2-2	≠	◊	◊	◊
DGD2-3	n/a	n/a	n/a	◊
DGD2-4	n/a	n/a	n/a	◊
DGD3-5	≠	≠	◊†	◊†
DGW-4	≠	≠	≠	≠
DGW-5	n/a	n/a	n/a	◊
DIZ-5	n/a	◊*	≠	≠
DAF-3	n/a	◊*	≠	◊†
RF2-2	≠	≠	≠	≠
RF6-3	≠	≠	◊	◊
SZ8-2	≠	≠	≠	≠

◊ On-site reduction indicated
≠ On-site reduction not indicated
* Only indicated with modified profile
† Only indicated with larger confidence interval
n/a Procedure not applicable

Figure 7.1.
Obsidian abundance (grams of obsidian) per site arranged by island.

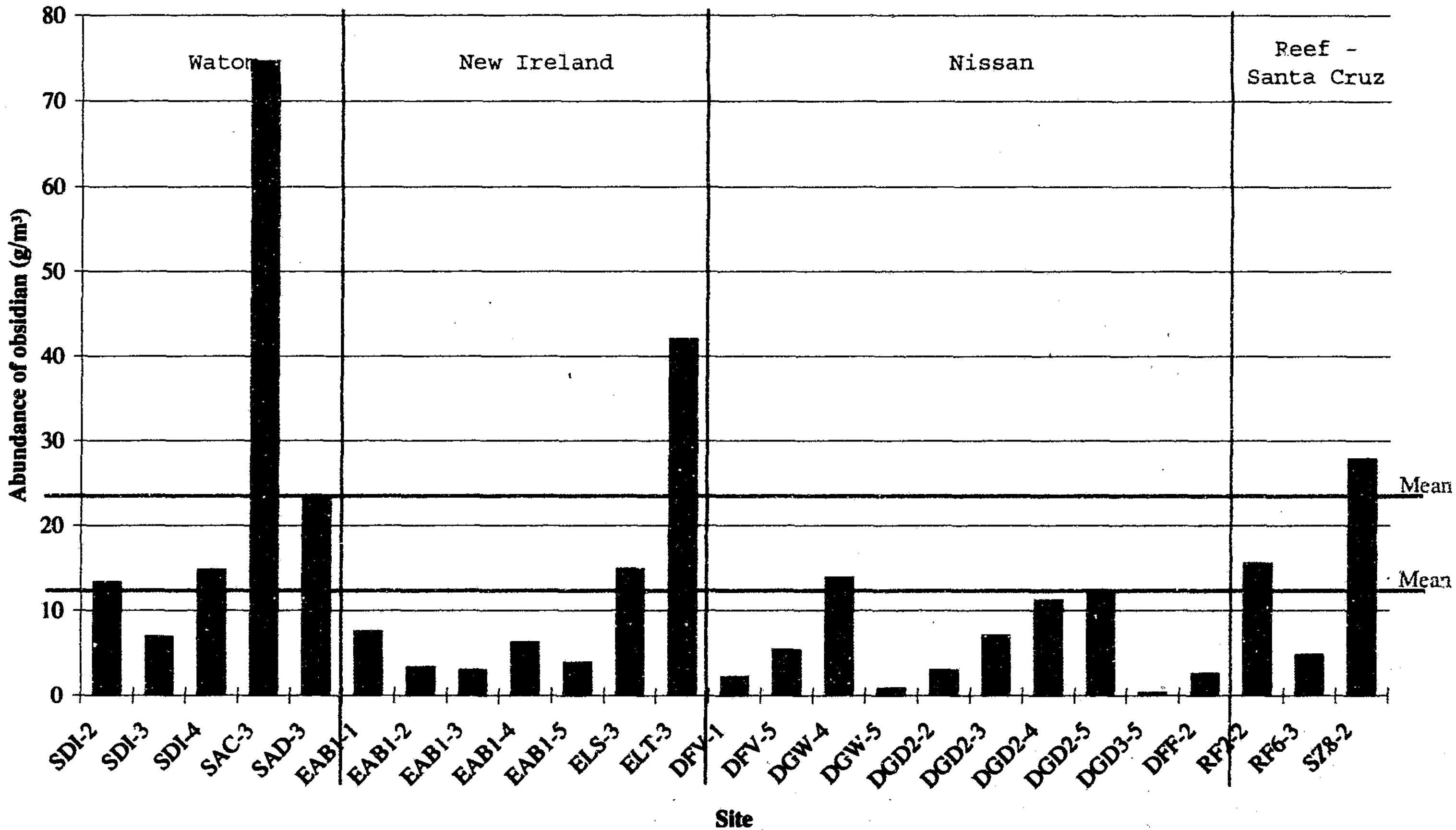


Figure 7.2.
Mean obsidian abundance (grams of obsidian) per island plotted against average straight line distance from the Admiralties and Talasea sources.

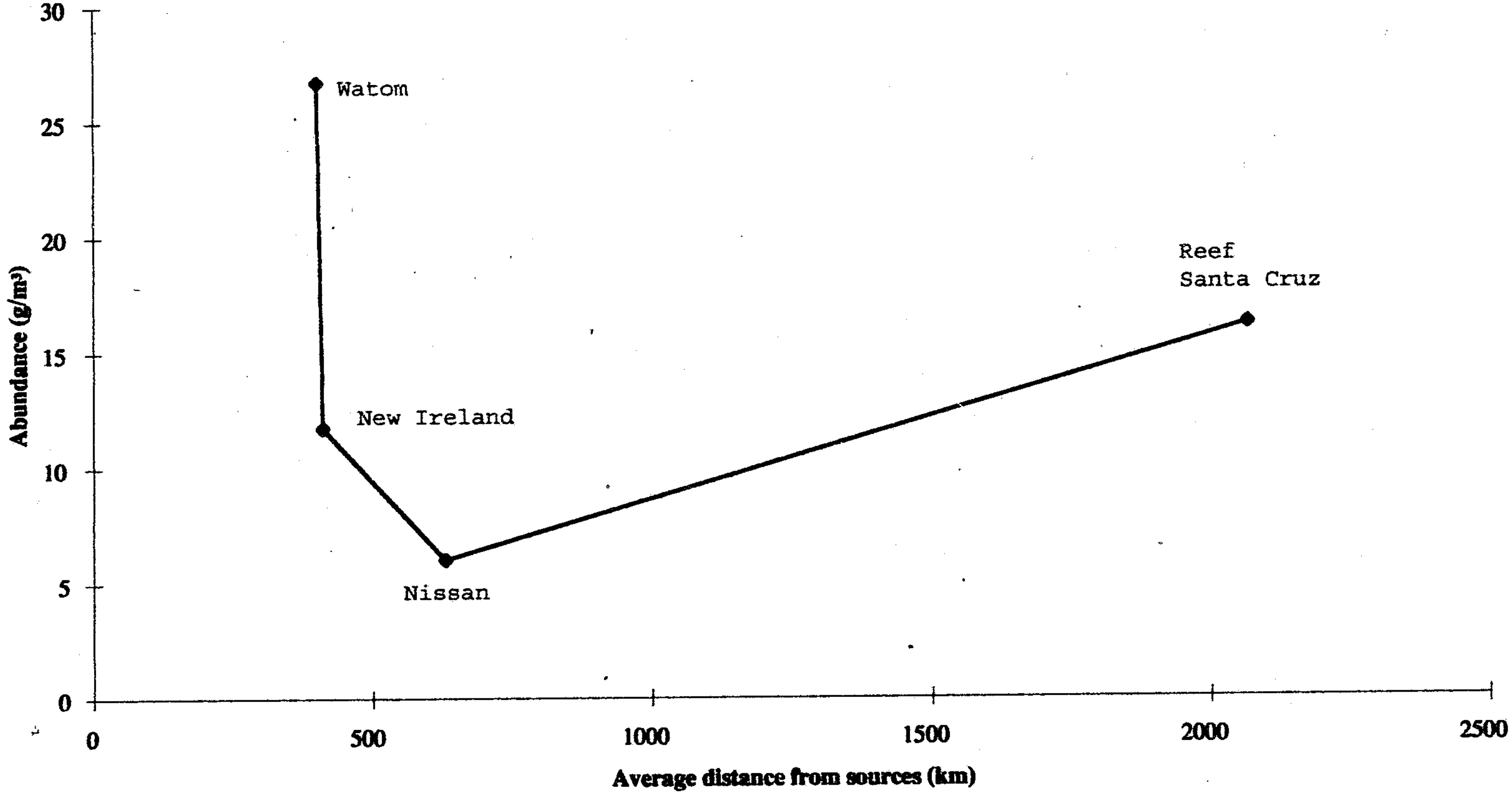


Figure 7.3.
Obsidian abundance (grams of obsidian) per site arranged by time period.

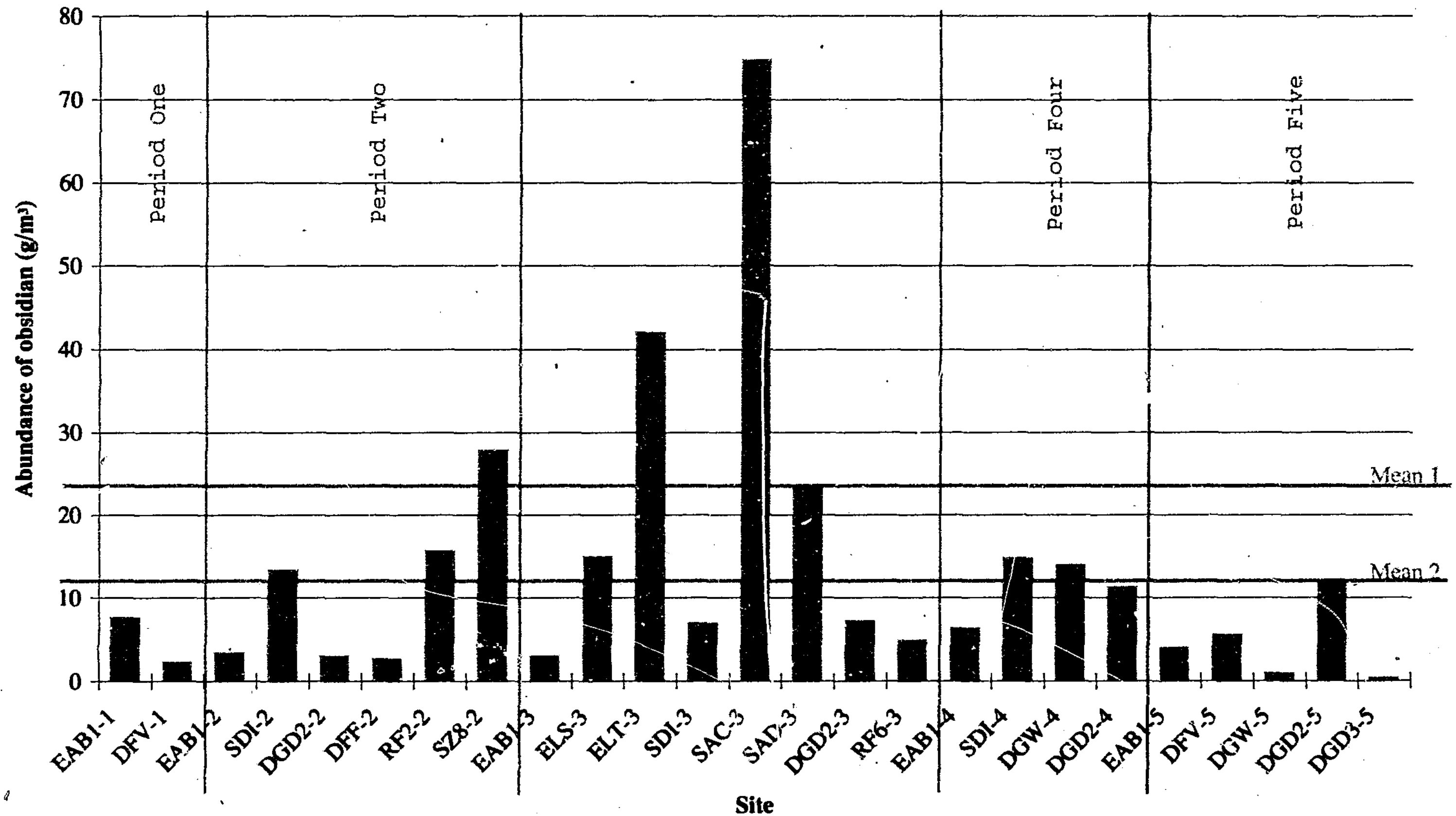


Figure 7.4.
Mean obsidian abundance (grams of obsidian) per time period.

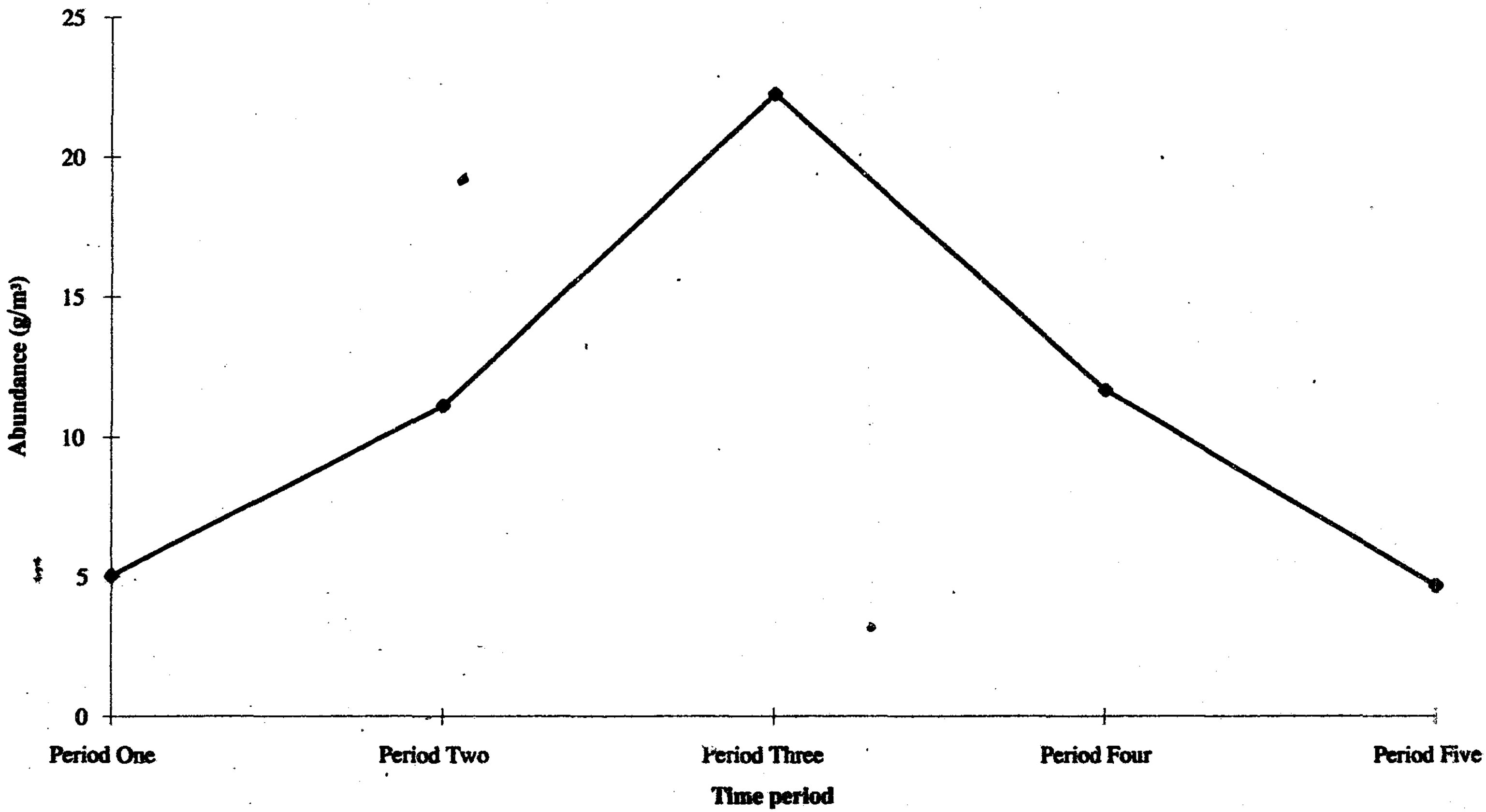


Figure 7.5.
Obsidian abundance (number of flakes) per site arranged by island.

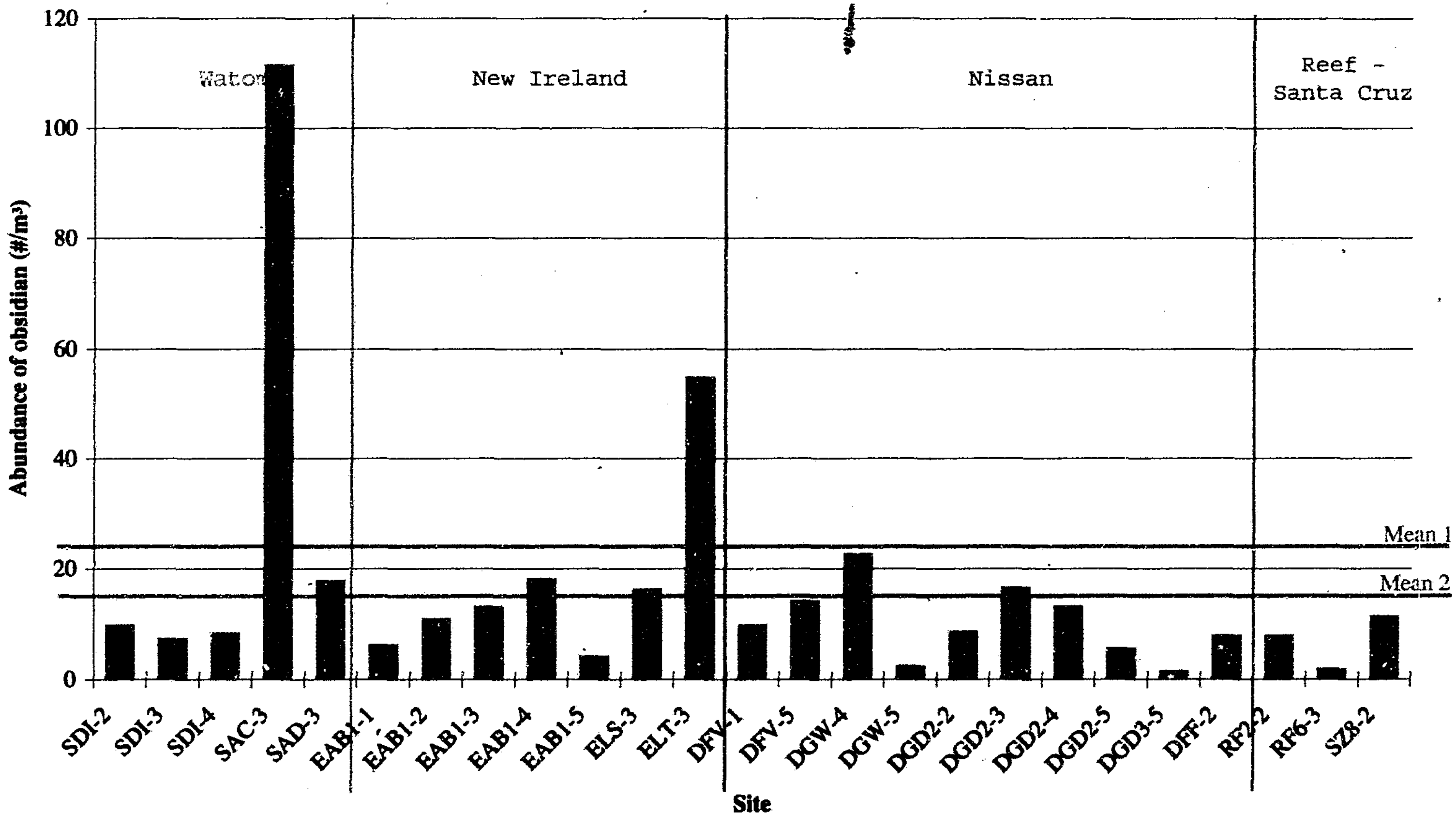


Figure 7.6.
Mean obsidian abundance (number of flakes) for each island plotted against average straight line distance from the Admiralties and Talasea sources.

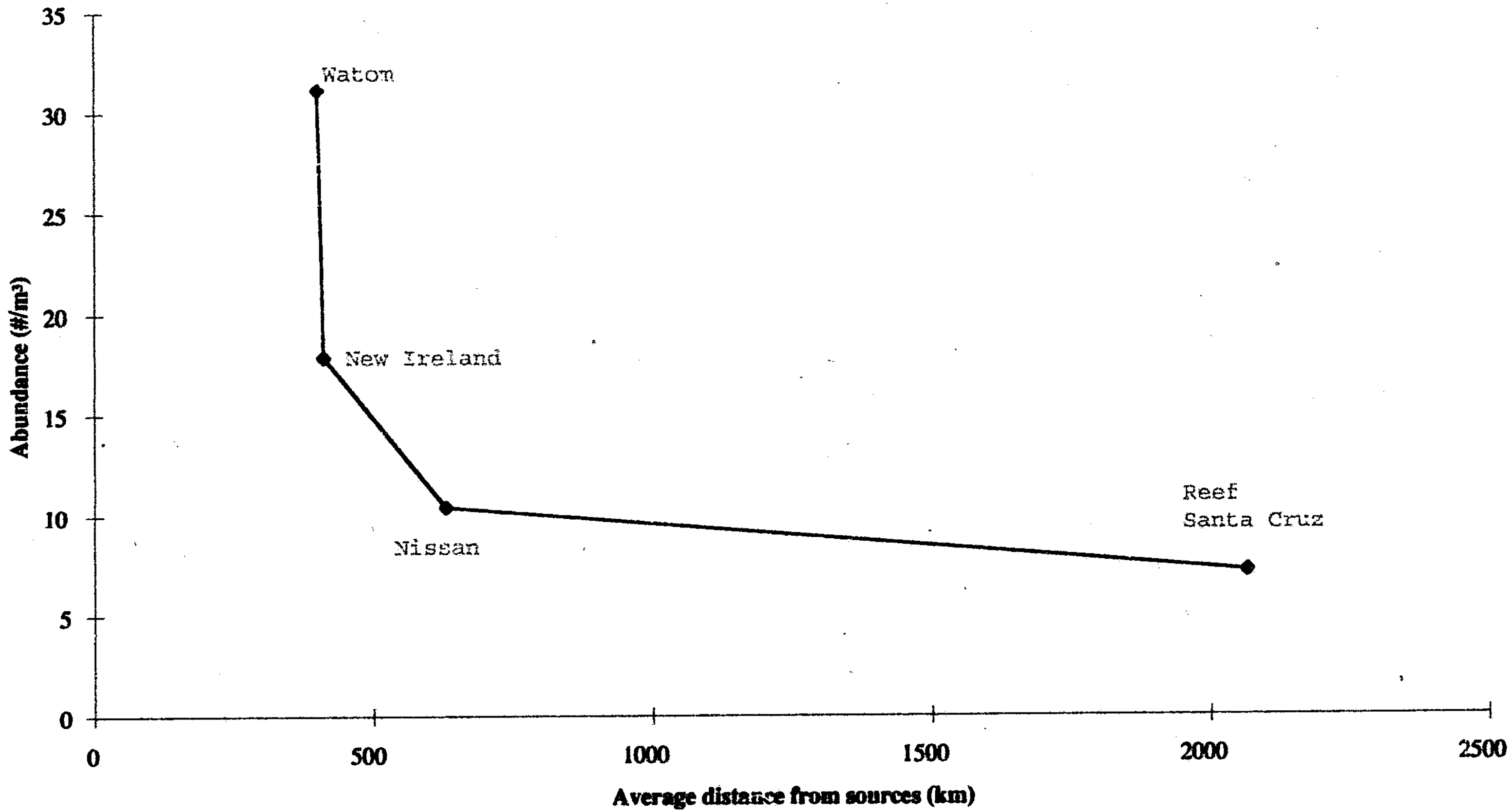


Figure 7.7.
Obsidian abundance (number of flakes) per site arranged by time period.

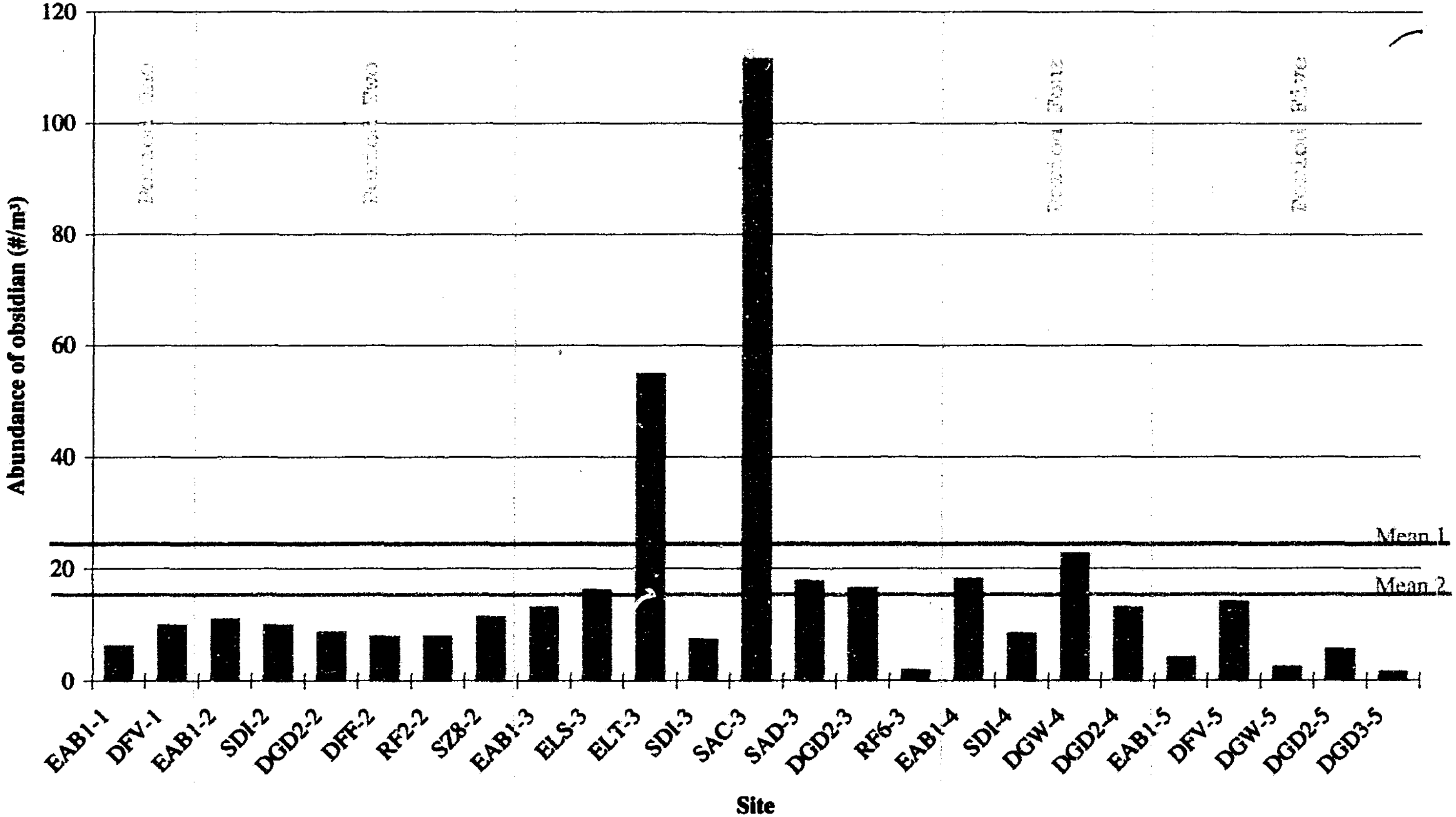


Figure 7.8.
Mean obsidian abundance (number of flakes) per time period.

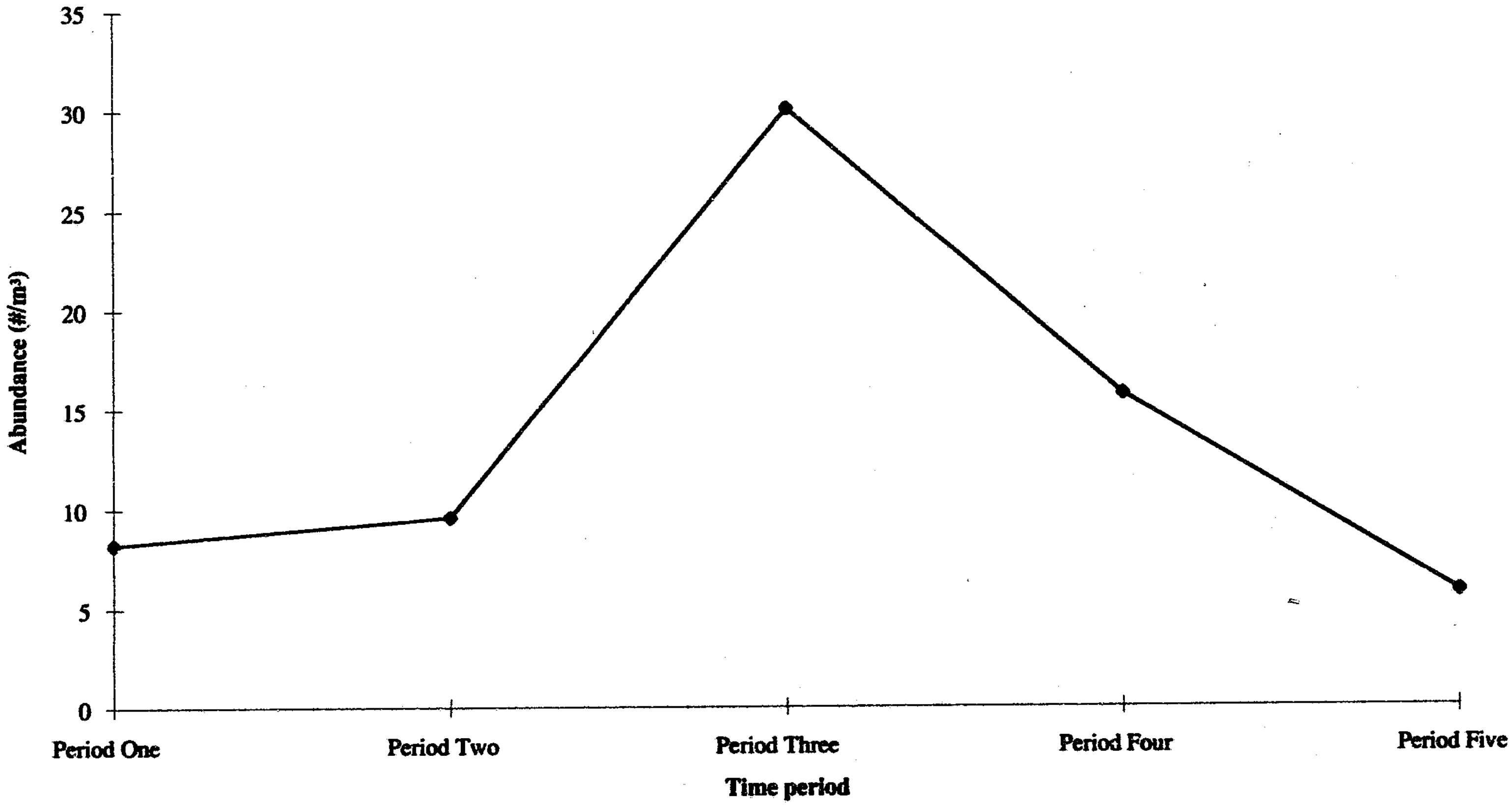


Figure 7.9.
Mean flake weight per site arranged by island.

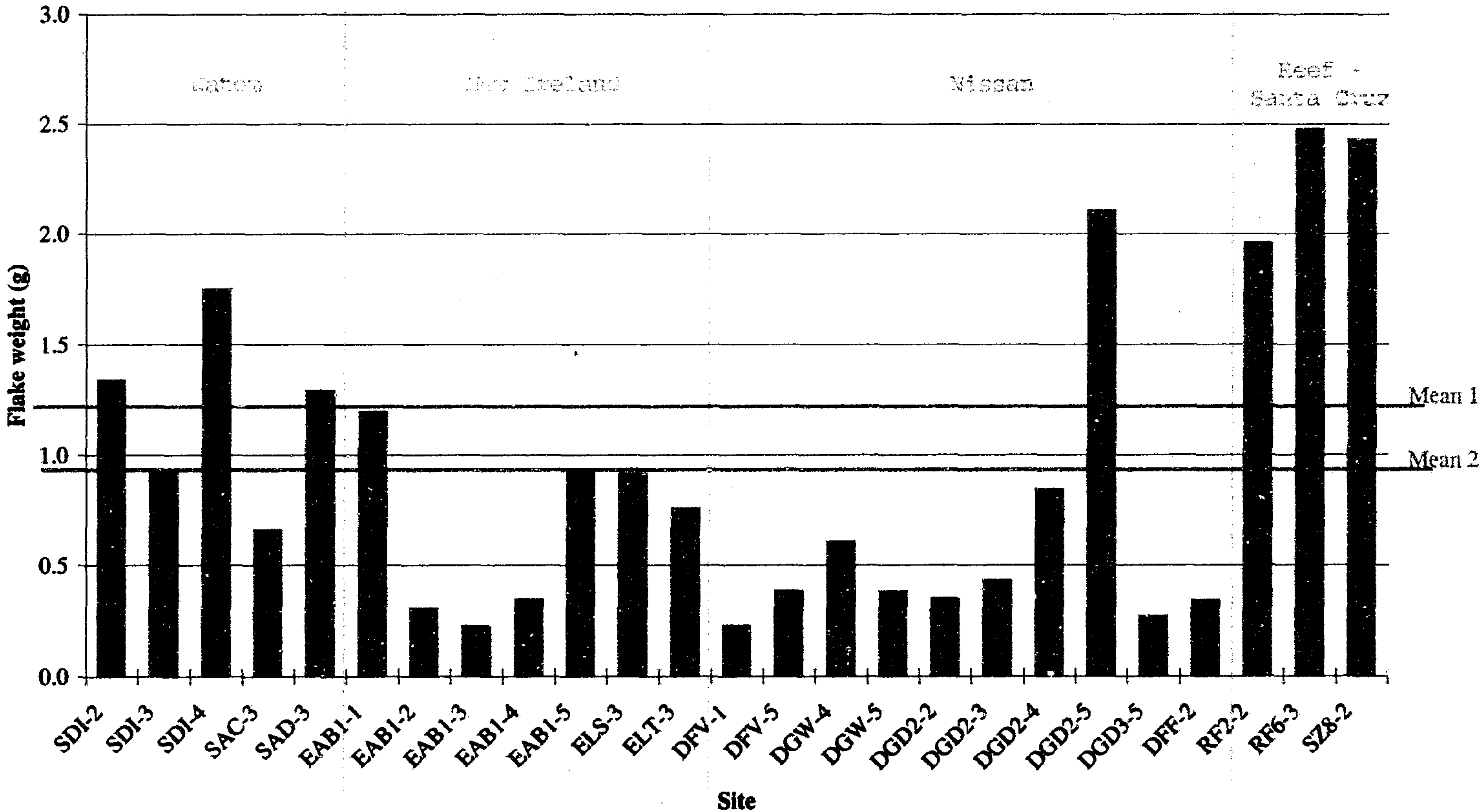


Figure 7.10.
Mean flake weight per site arranged by time period.

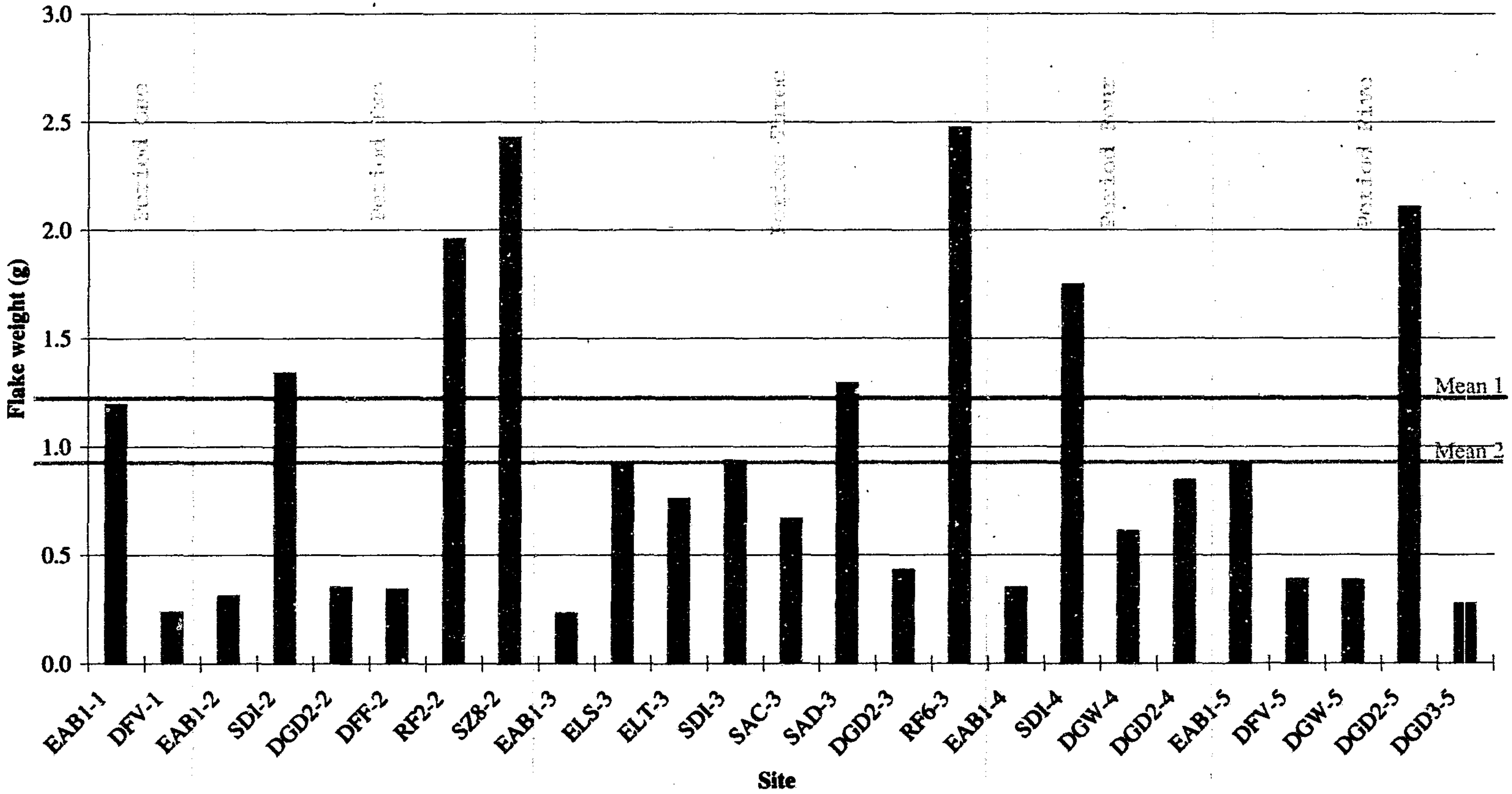


Figure 7.11.
Mean flake weight per island plotted against average straight line distance from the Admiralties and Talasea sources.

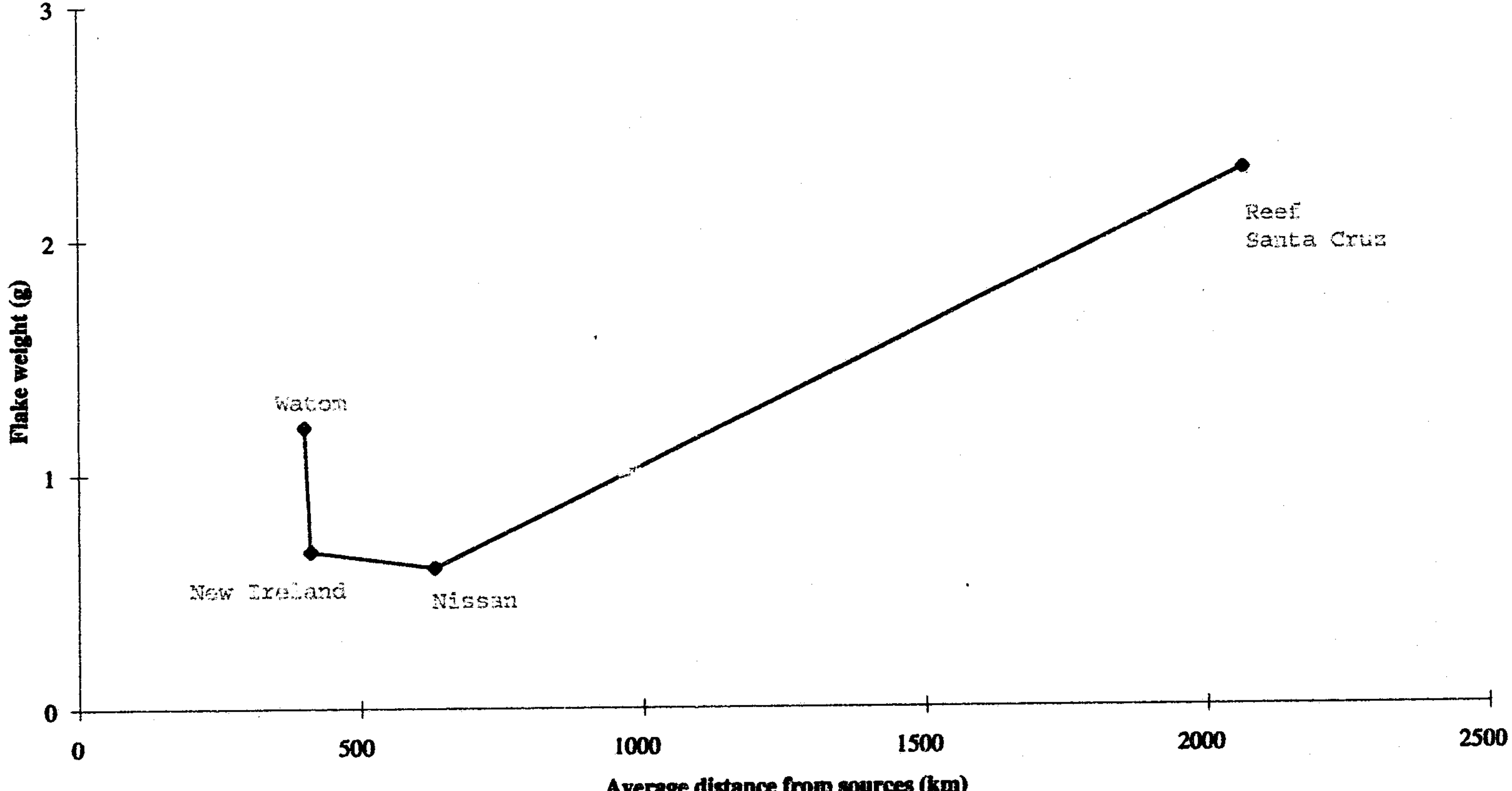


Figure 7.12.
Mean flake weight per time period.

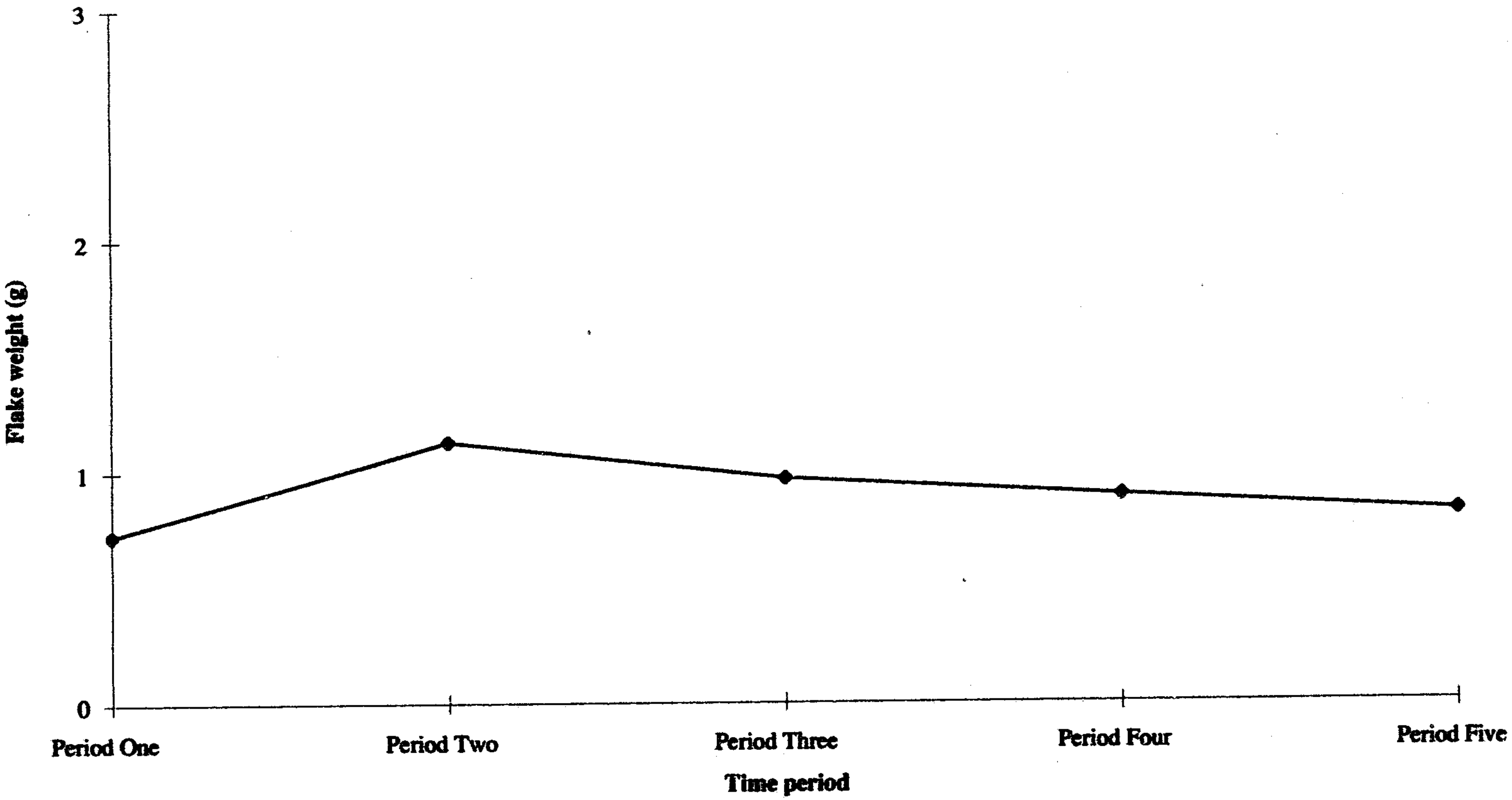


Figure 7.13.
Mean obsidian abundance per time period for all three measures of abundance (grams, numbers of flakes and flake size) calculated as simple mean.

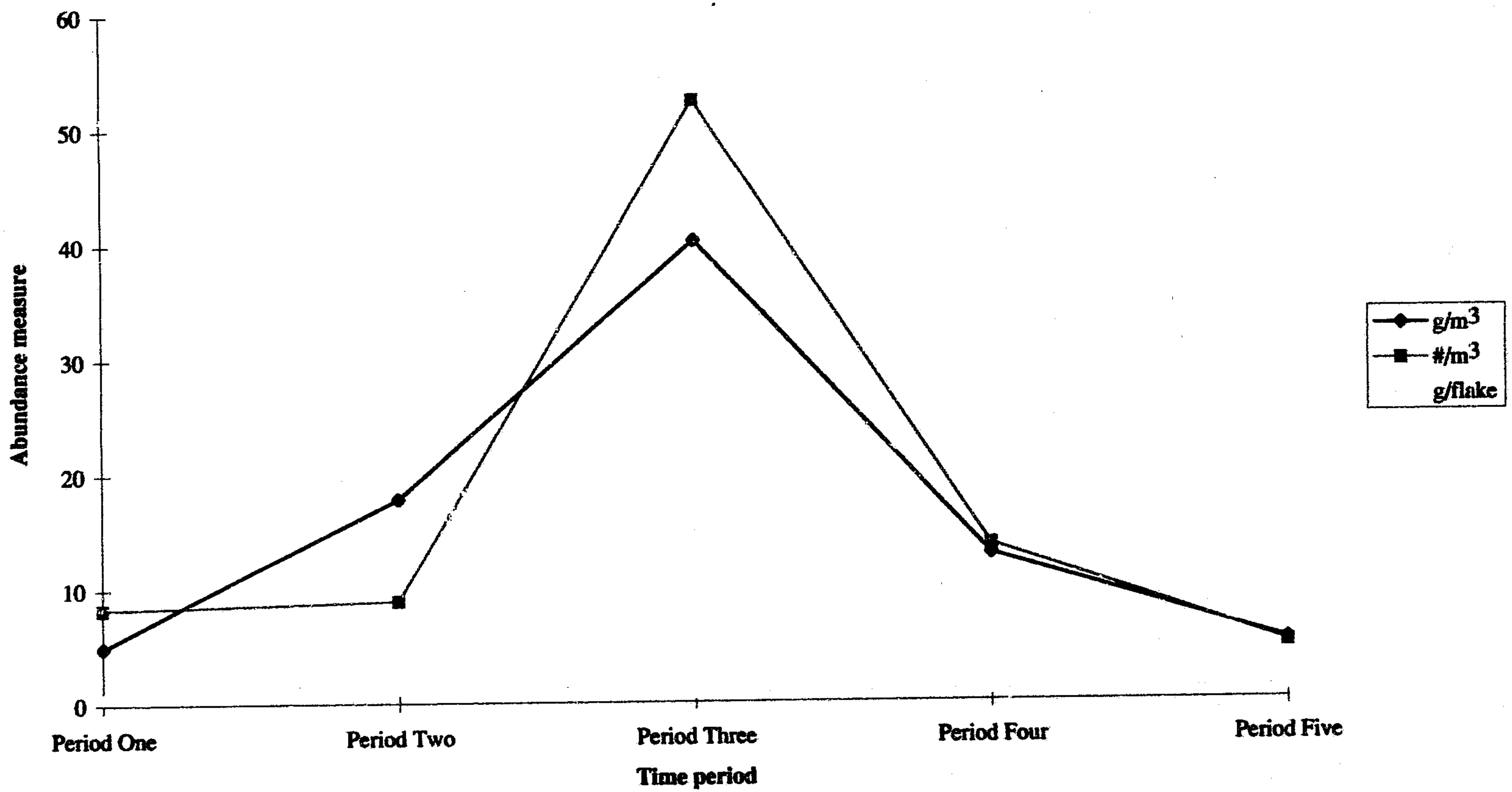


Figure 7.14.
Mean obsidian abundance per island group plotted against average straight line distance from the sources for all three measures of abundance (grams, numbers of flakes and flake size) calculated as simple mean.

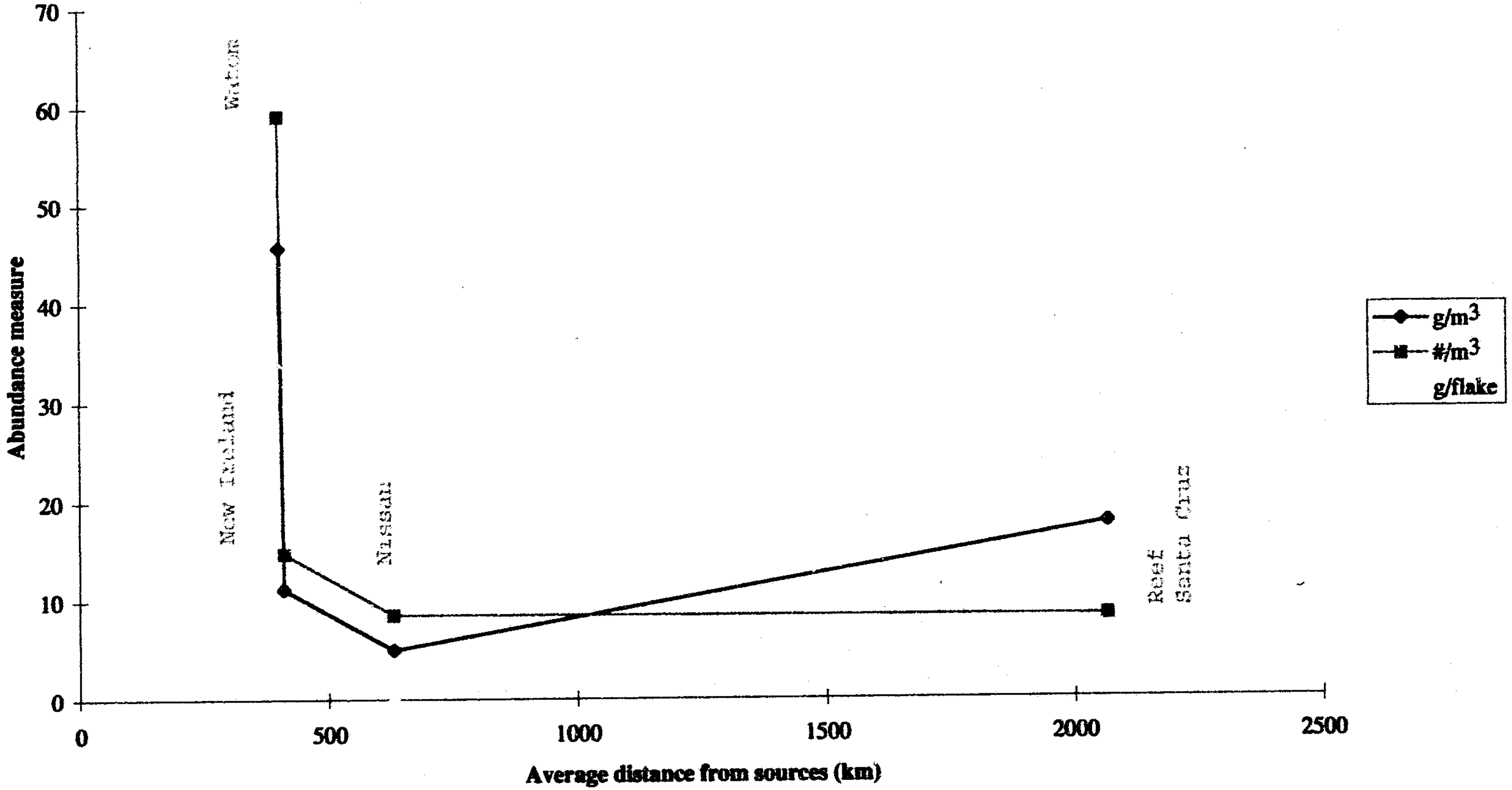


Figure 7.15.
Flake size distribution profile for Watom sites.

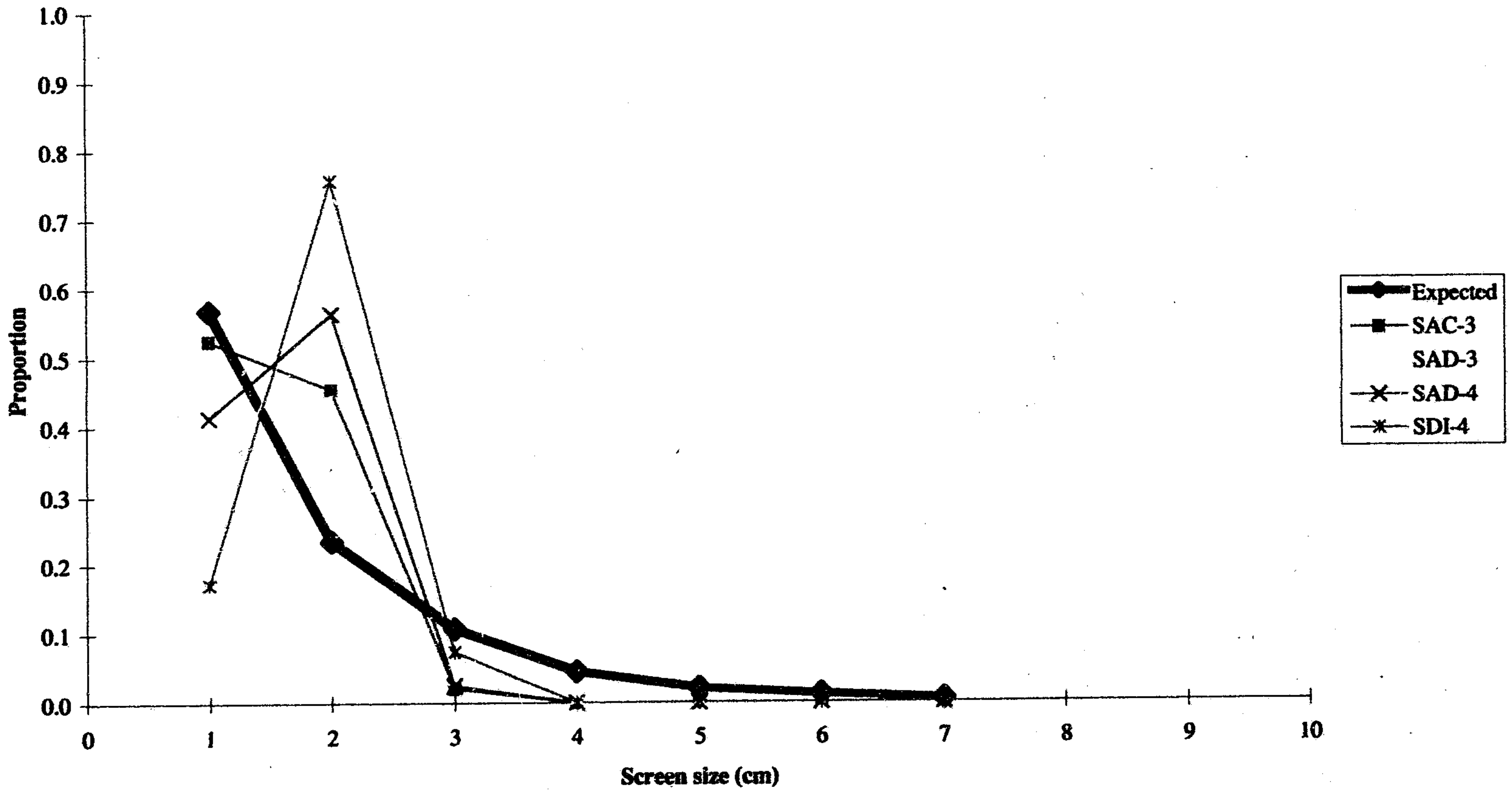


Figure 7.16.
Flake size distribution profile for New Ireland sites.

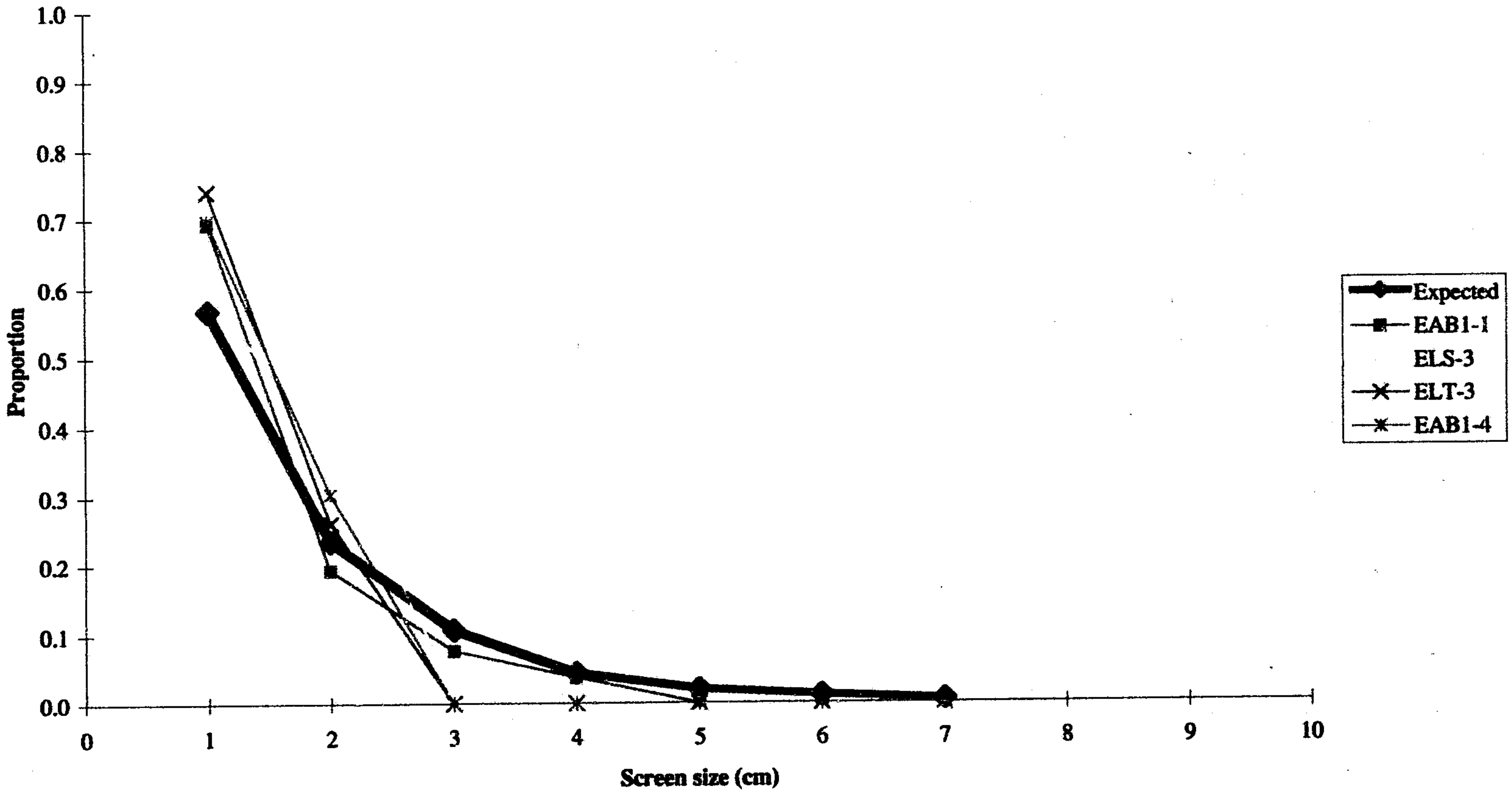


Figure 7.17.
Flake size distribution profile for Nissan sites.

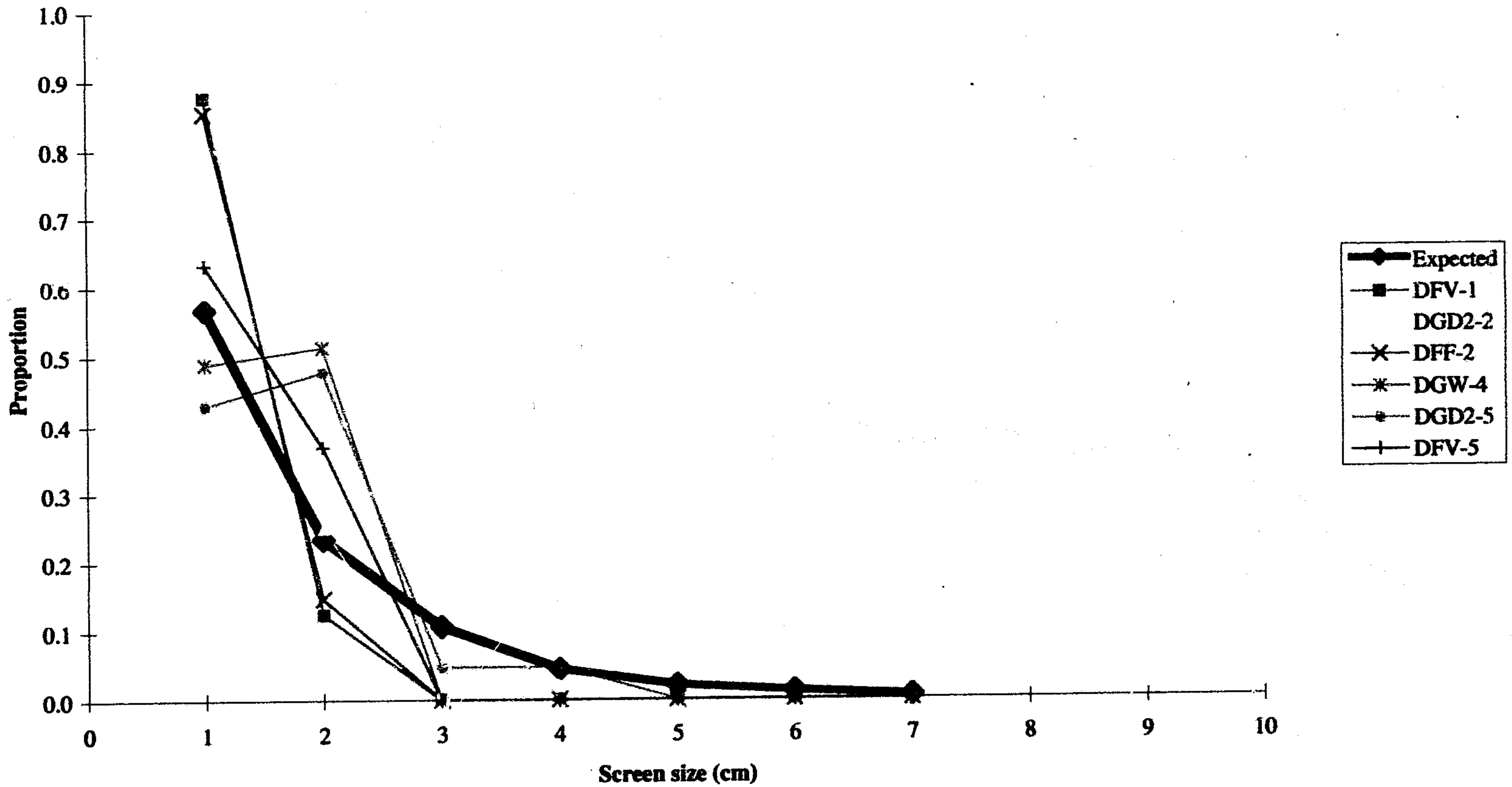


Figure 7.18.
Flake size distribution profile for reef-top sites.

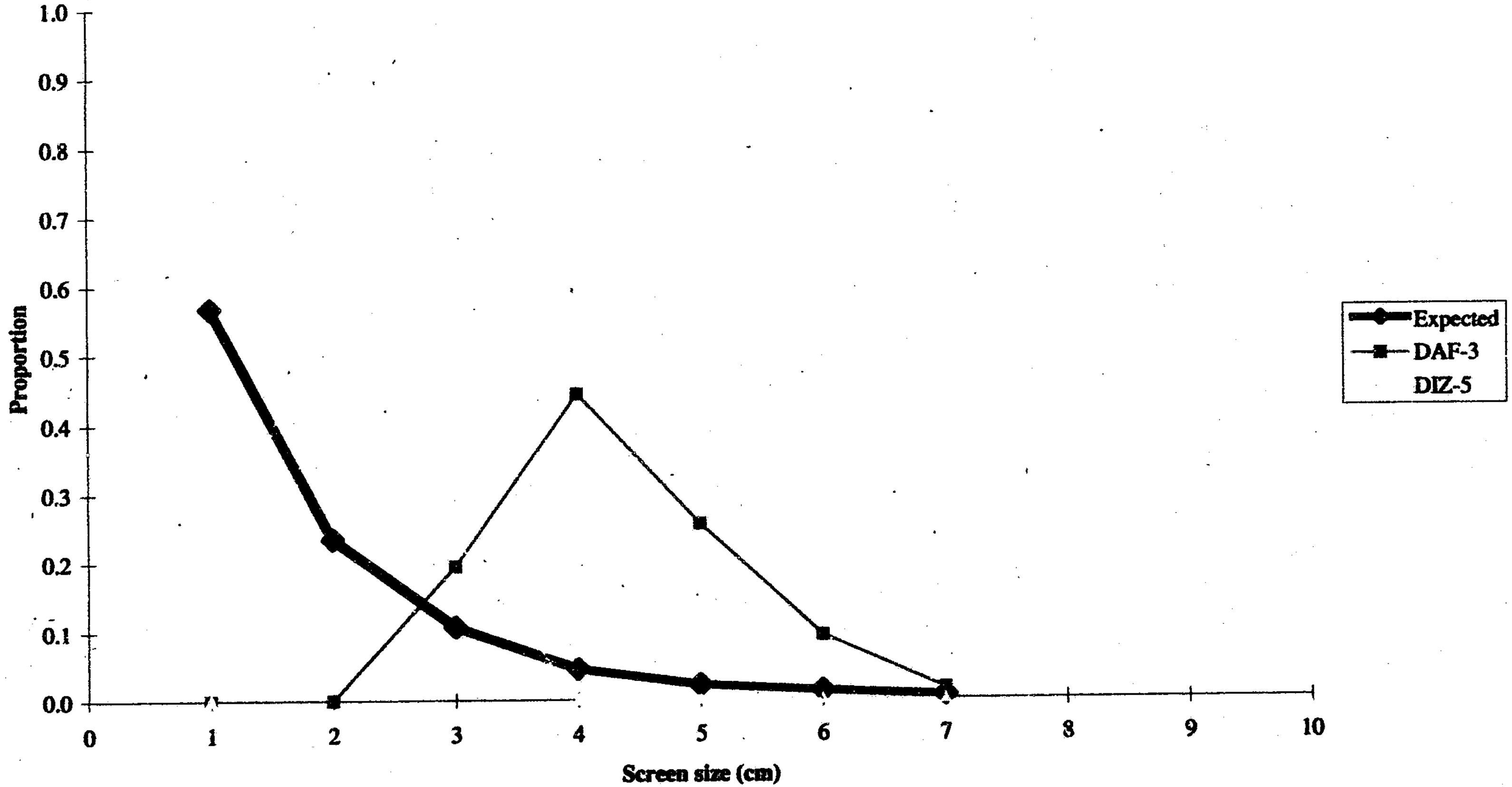


Figure 7.19.
Flake size distribution profile for Reef/Santa Cruz group sites.

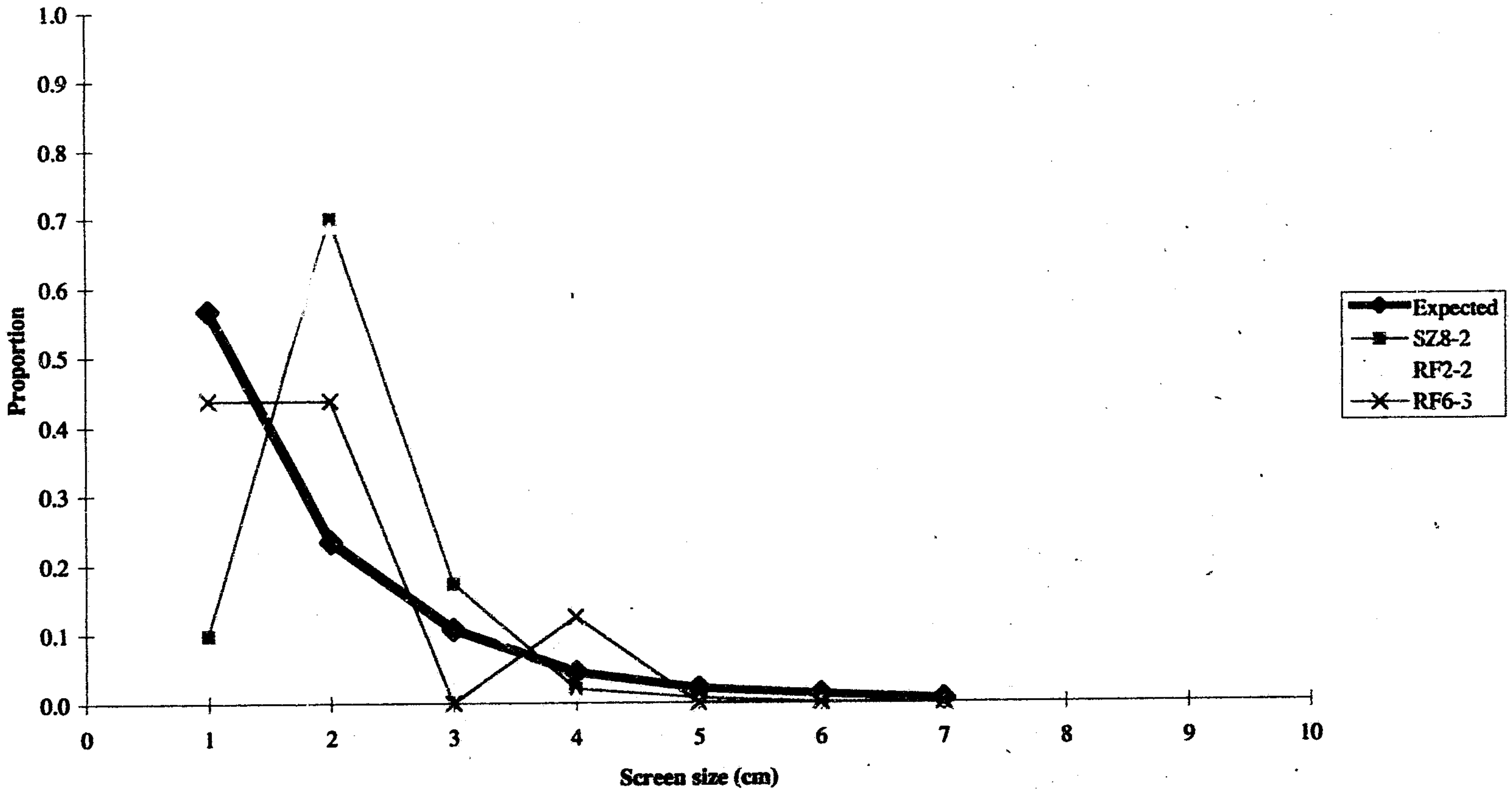


Figure 7.20.
Flake size distribution profile for Period One sites.

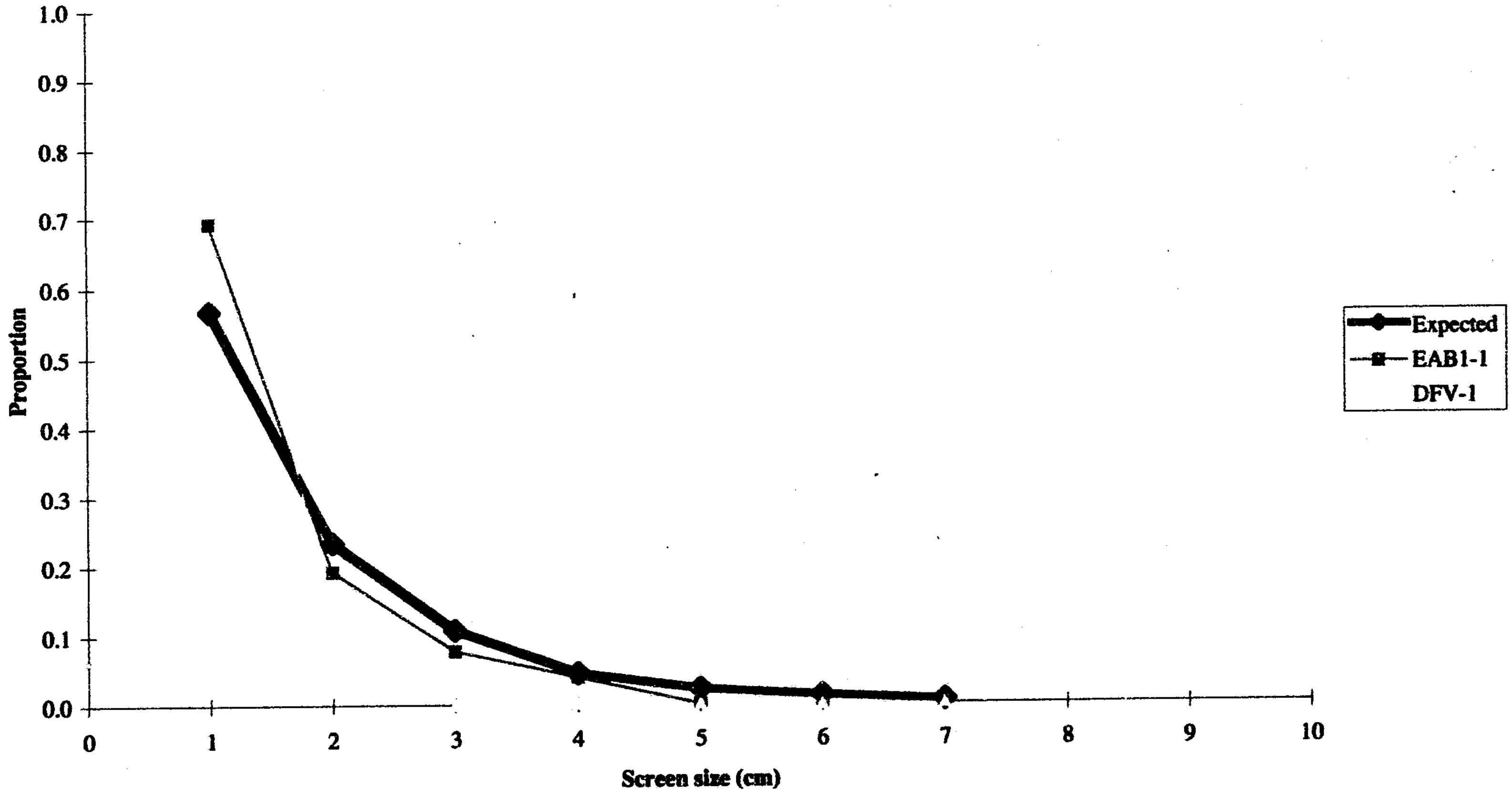


Figure 7.21.
Flake size distribution profile for Period Two sites.

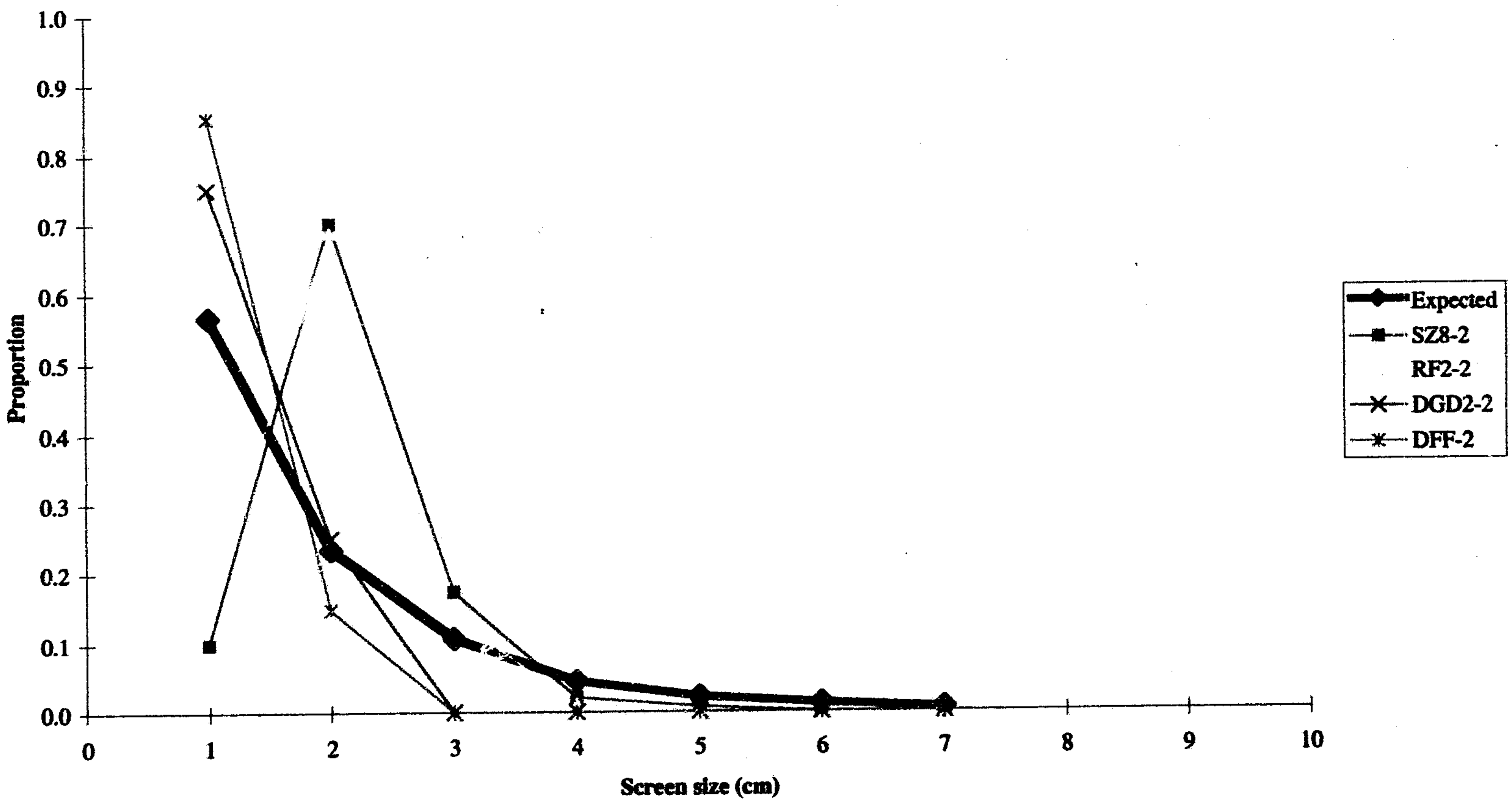


Figure 7.22.
Flake size distribution profile for Period Three sites.

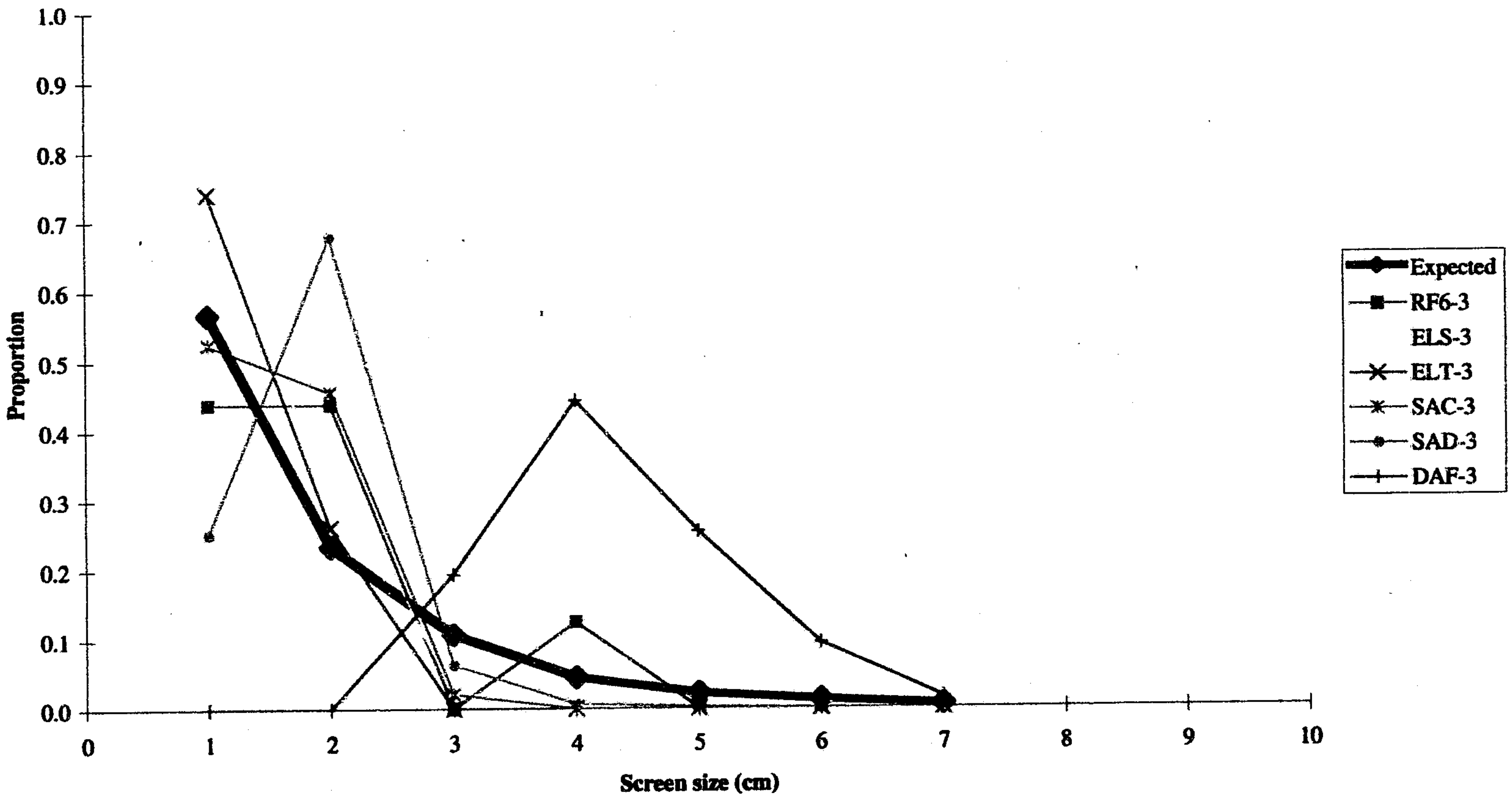


Figure 7.23.
Flake size distribution profile for Period Four sites.

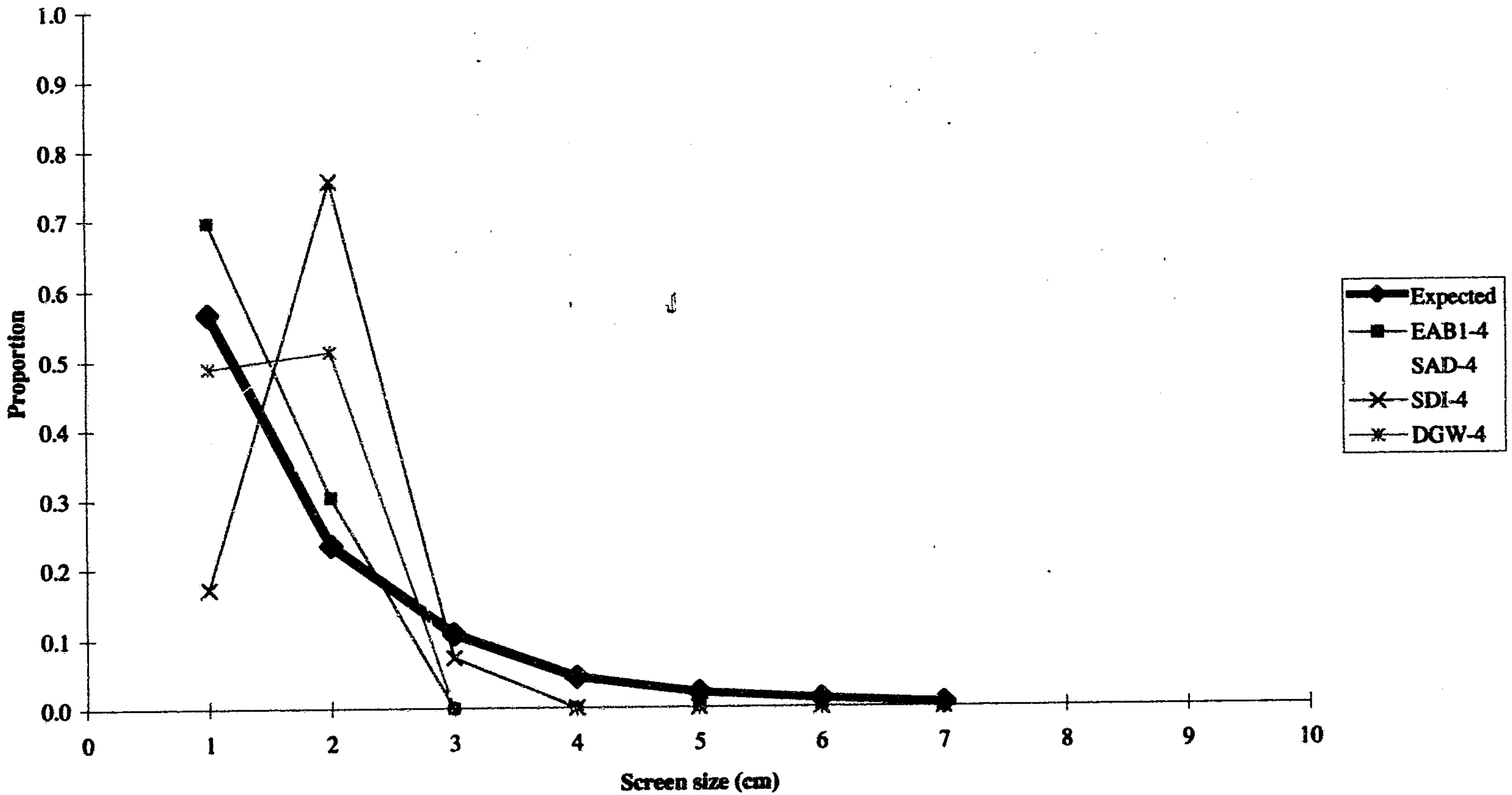


Figure 7.24.
Flake size distribution profile for Period Five sites.

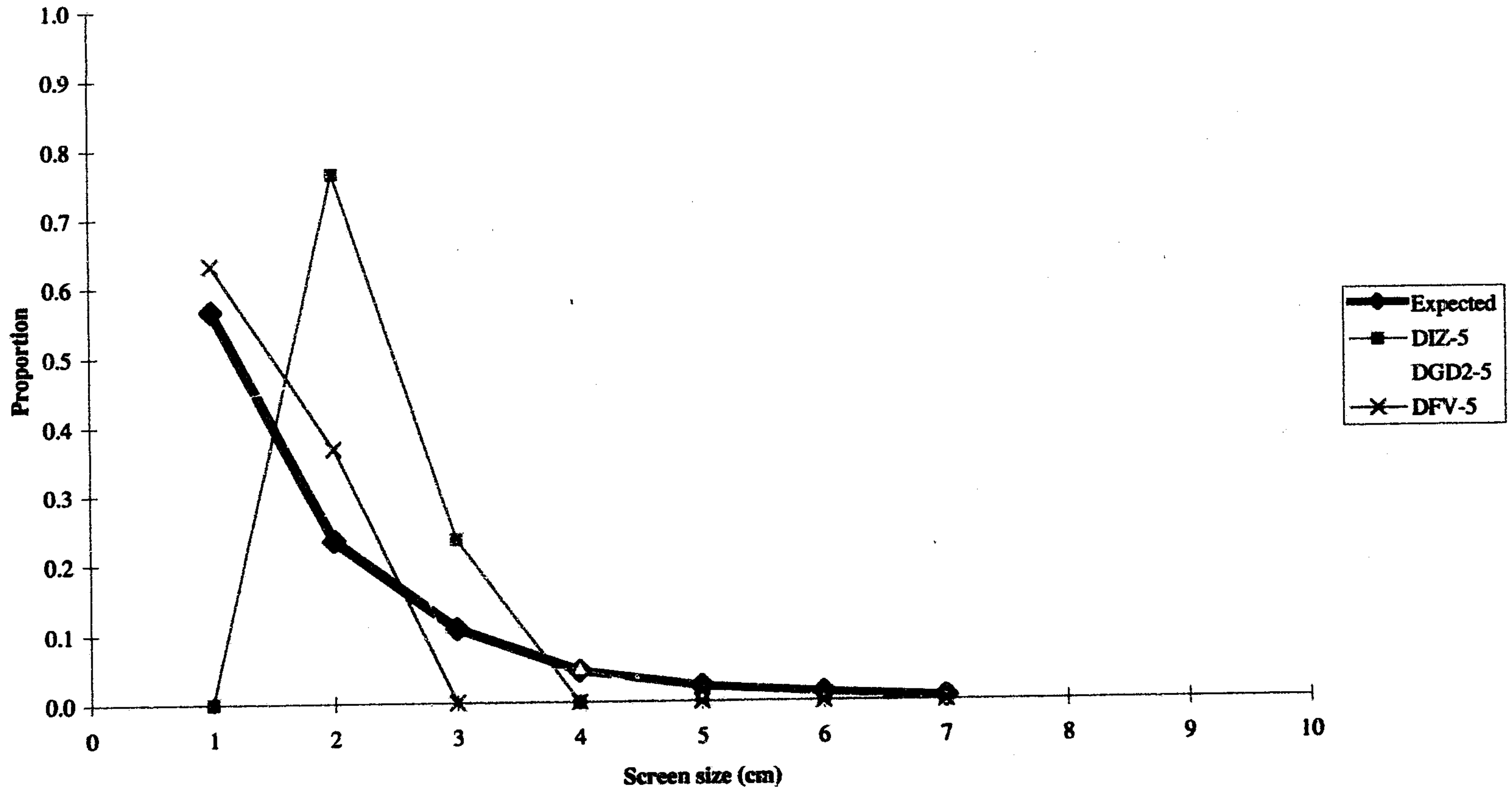


Figure 7.25.
Short flake size profile for Watom sites.

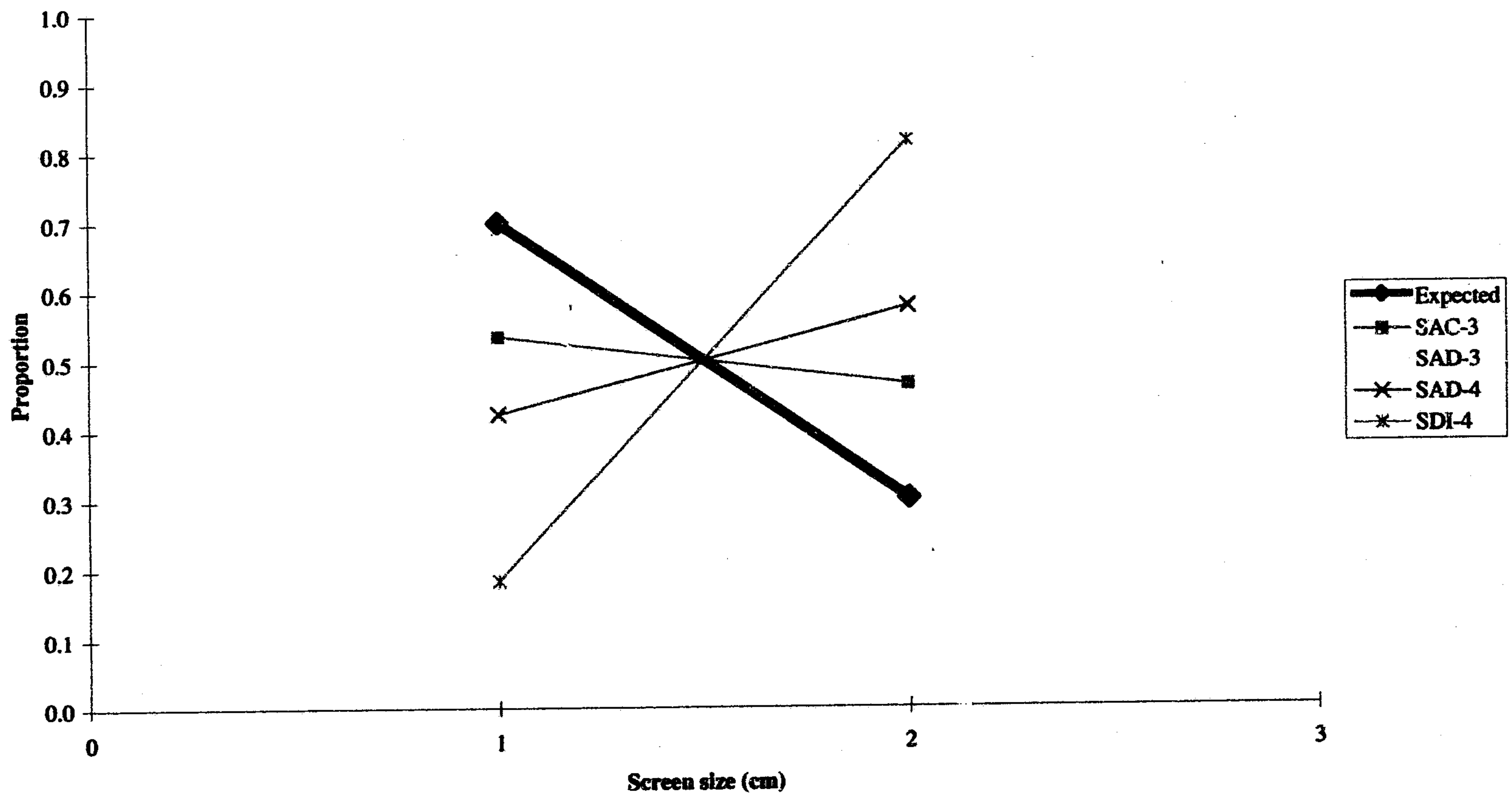


Figure 7.26.
Short flake size profile for New Ireland sites.

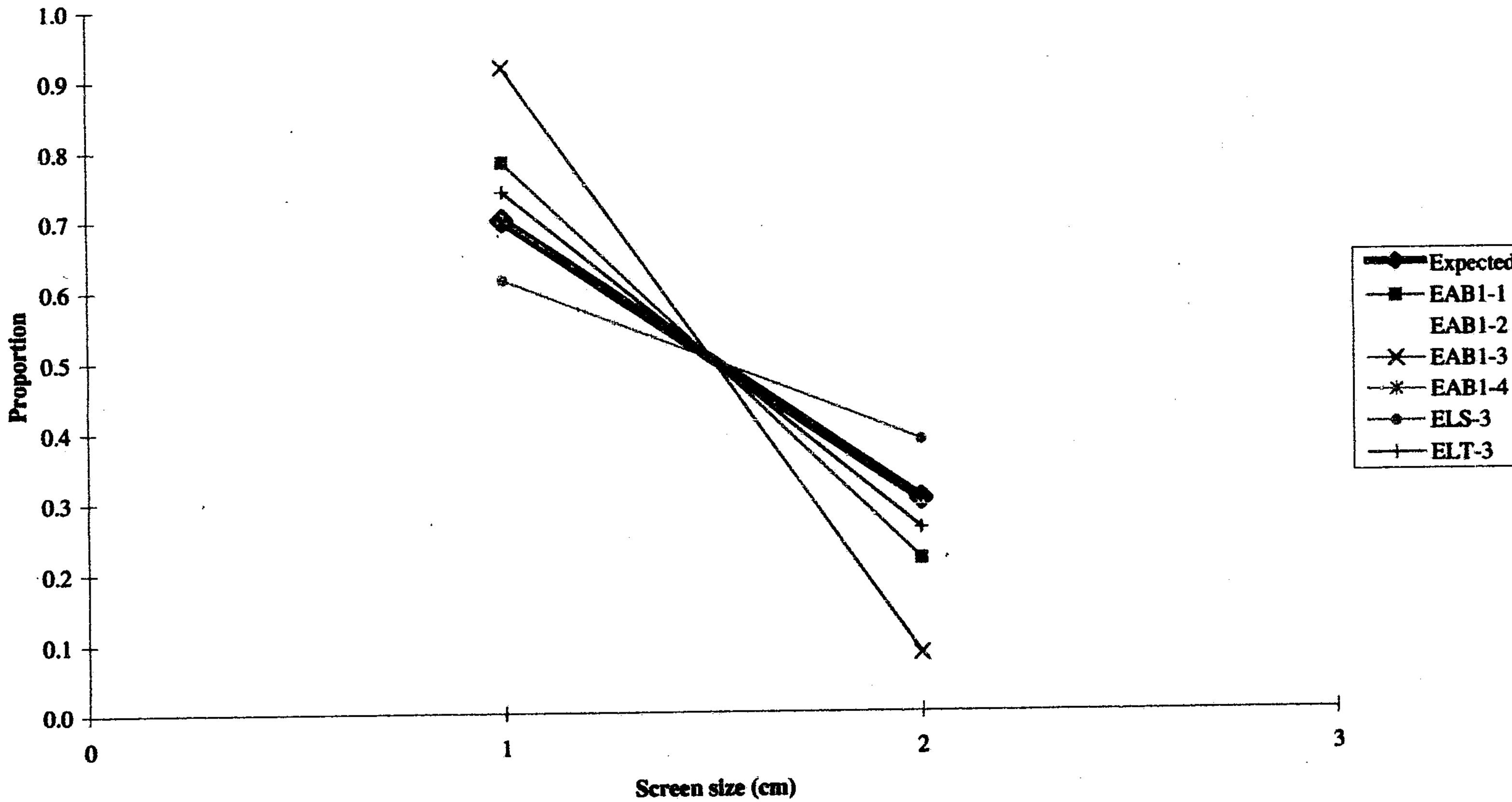


Figure 7.27.
Short flake size profile for Nissan sites.

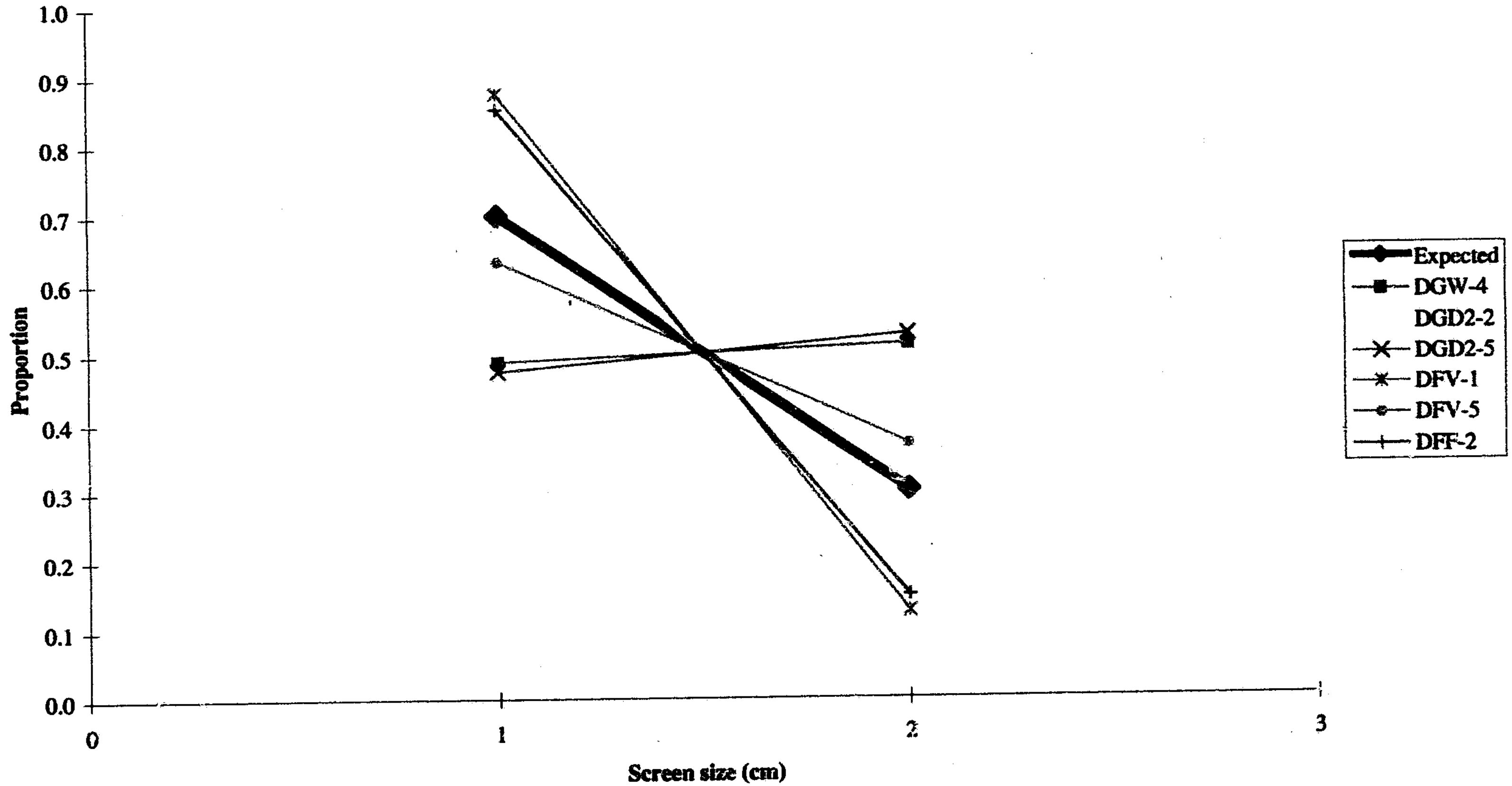


Figure 7.28.
Short flake size profile for reef-top sites.

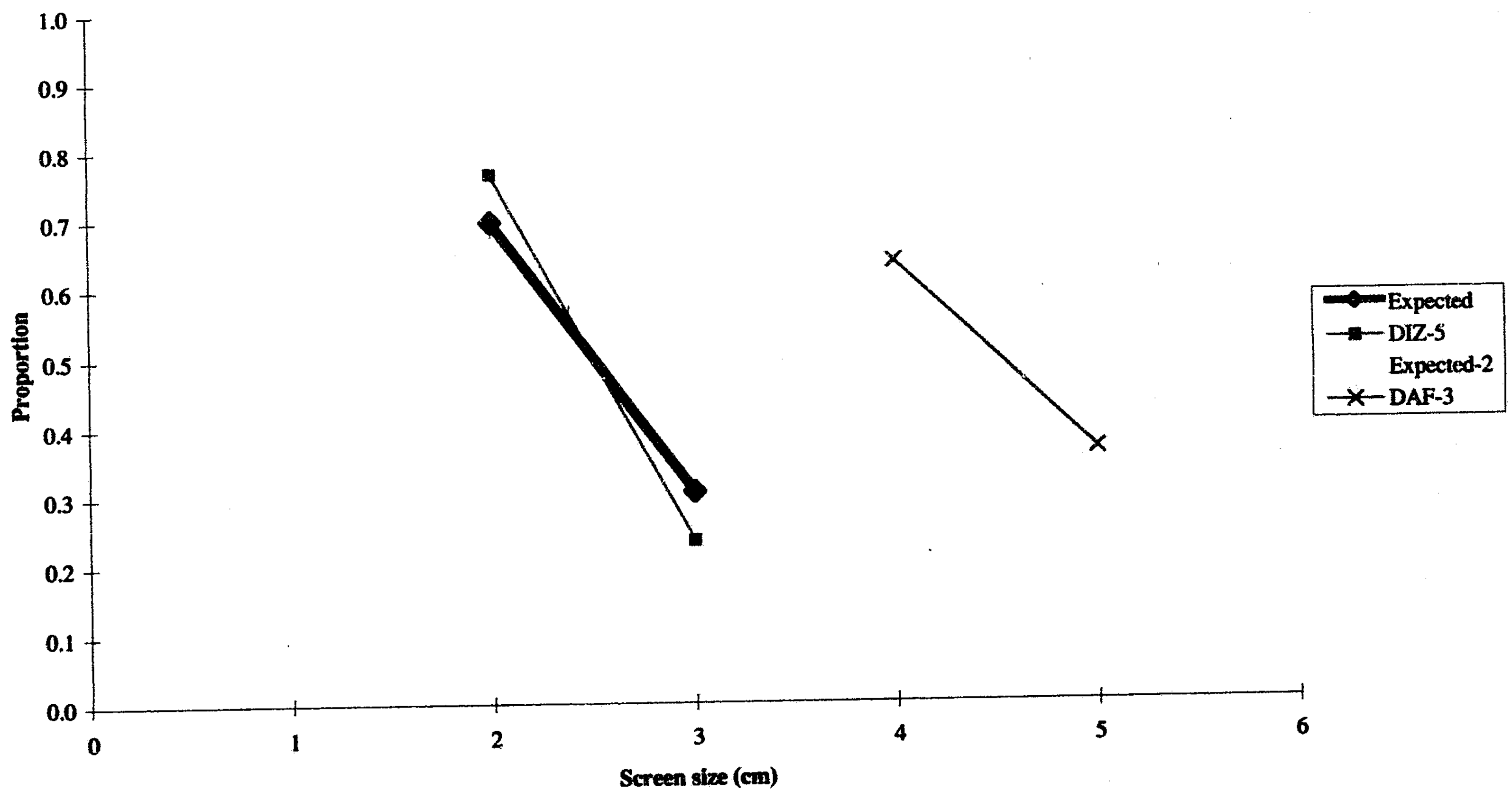
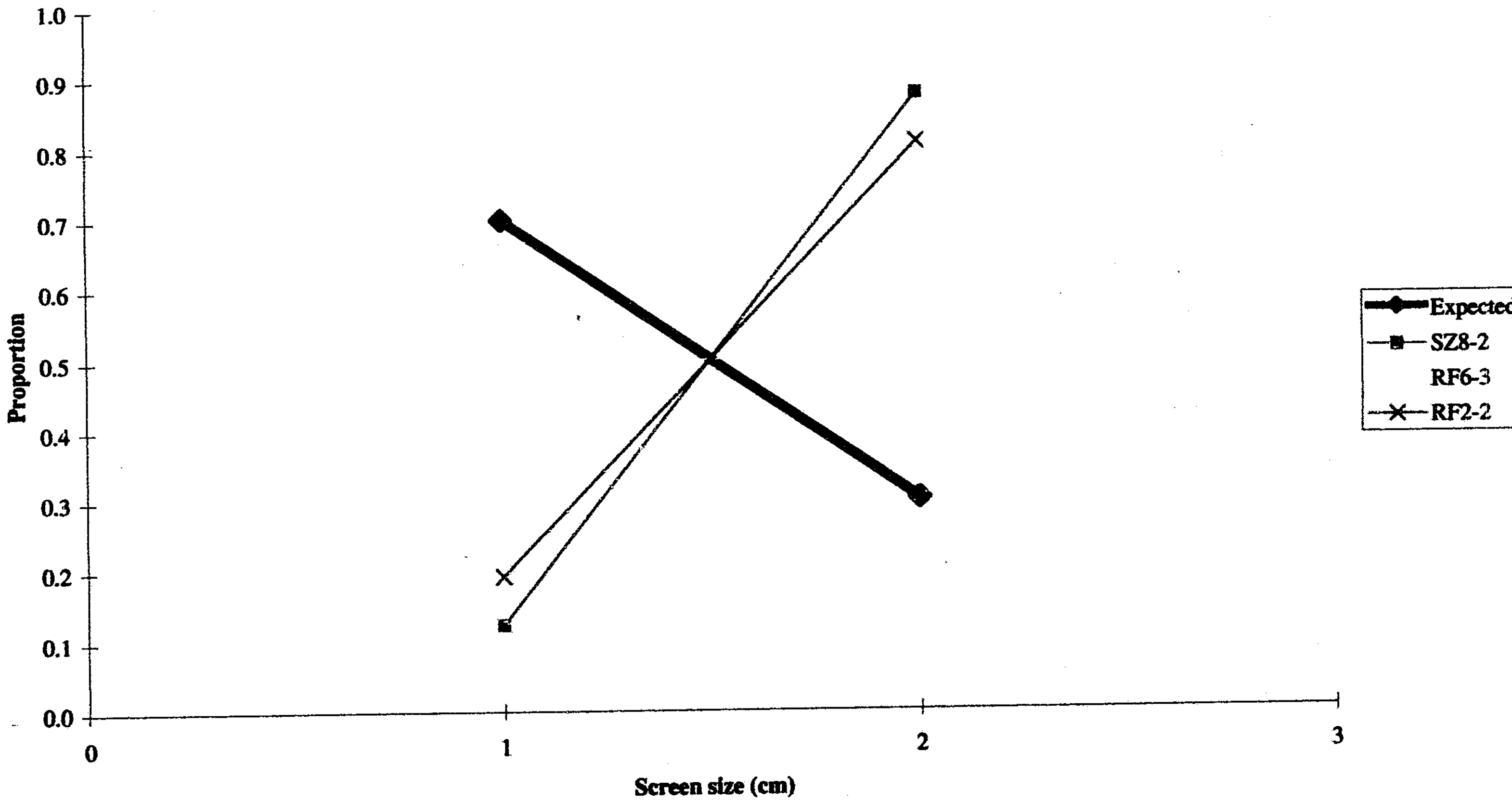


Figure 7.29.
Short flake size profile for Reef/Santa Cruz group sites.



§8

Function and form in obsidian tools

This chapter is the final one in the method and results section of the thesis. Primarily concerned with the use-wear study undertaken on the obsidian, it also includes the closely related topics of image recording and glass hydration and concludes with the analysis of flake size. Use-wear occupies the bulk of this chapter and covers the development and application of the technique before addressing the outcome and the reasons it was so unsuccessful. As use-wear involves long periods of visual inspection, especially at various levels of magnification, the imaging section follows on from this in logical order. In order to understand why the use-wear study was not useful in the way it was planned, it is necessary to look into the hydration processes of glass. Obsidian hydration dating and the normal weathering behaviour of obsidian both depend on this chemistry. Metrical studies typically go hand-in-hand with use-wear because the function is often tied to the shape and/or size of the flake. For this reason the chapter concludes with the study of flake morphology.

Obsidian makes good tools for archaeologists to look at: it produces sharp edges, it is rare, it is durable, it has a short use life, it is chemically homogenous, and even small artefacts can be sourced (Ambrose, Bird and Duerden 1981). While worldwide obsidian use includes purely decorative objects, surgical style knife edges, and general usage tools (for examples of all three see Berrin and Pasztory 1993: catalogue entries 169–71), in Melanesia it is confined mostly to the category of general flake tool (Watson 1986). A distinctive form of stemmed tool is found on New Britain from excavation levels older than about 3500 years ago (Fullagar 1993), and a triangular spear point is found on Manus from more recent occupation levels (Fredericksen 1994). They are however two examples of the few formal tool types found in Melanesia made from obsidian.

Two bifaces of obsidian found by Specht, tools quite unlike the typical ones of the area, raise questions for future investigation about lithic developments (J. Specht pers. comm.). Perhaps the concept of risk management can explain something of what is going on with obsidian in the Pacific? We know from the example in chapter four that bifacial points are a good inclusion in an anti-risk toolkit.

While obsidian is found in Lapita and later sites in Polynesia, it is in such small quantities that it could not have been an important source of tools (Davidson 1978). Obsidian abundance declines markedly away from the source areas within Melanesia and it is unlikely that obsidian was an 'important' source of tools anywhere more than a few tens of kilometres from the source.

FUNCTIONAL ANALYSIS

There is much that one can hope to learn from the study of the obsidian tools, whether found in large or small numbers. The method of production, and the manner of use are both data sets one can piece together. With patience, practice and skill, it is possible to reconstruct individual reduction activities. Even when this is not done (or possible), much can be learned about the reduction techniques employed by examining the debitage of the process.

Of much potential, but controversial utility, are use-wear studies. Debates raged in the journals through much of the 1980s regarding not only the methodology but even if microwear could be studied at all (Newcomer, Grace and Unger-Hamilton 1986, 1988). Bamforth's article (1988) reviews some of the problems and counters with some of the demonstrable bright points. The conclusion is that when carefully carried out, there is definitely a place for use-wear studies in archaeology.

Use-wear studies are of two varieties. Those that examine the damage done to the stone itself from the use, and residue analyses that examine the deposits on the surface of the tool. Fullagar (1991) looked at the former when he experimentally determined the importance of amorphous silica in the formation of 'polishes' on stone tools. Loy and Hardy (1992) provide an example of the latter style, examining blood residues on the tools.

Polishes are often essential in the assignment of function in the course of use-wear studies. Keeley's pioneering work (1980) on flint tools relied totally on the form and type of polish observed to indicate the worked material and motion. While the idea that polish types vary with use was discounted by many researchers (especially Grace 1989) more recent work by chemists have returned to the idea that the worked material leaves identifiable and indelible traces on the surface of the stone tool (Shackley 1998b).

More important than what the tools were specifically used for are the larger implications this has for the social group under study. At Thatcham in Berkshire, England, an analysis of the flint tools showed that the site was a home-base type camp with a majority of plant processing going on (Healy et al. 1992). Loy's examination of the tools of the famous Ice Man showed that he was carrying a toolkit of well-used gear that would be typical for someone hunting (1998). Shea (1993) found microwear evidence for certain parts of the Mousterian toolkit to be hafted spear points. A use-wear study (Fullagar 1993) on the stemmed tools of Phase one New Britain prehistory led to the conclusion that the stem is a hafted end. Halsey (1995) examined the assemblage from the Makekur site (Adwe Island) with the intent of comparing her results with those from the Reef/Santa Cruz assemblages to 'check' Sheppard's hypothesis that the obsidian in the Solomons, so far from the source, was being knapped and used without regard to resource maximisation. Intensity of use and reduction strategy were the traits examined, and the conclusion was that efficiency was similarly disregarded at both locales.

Almost all use-wear studies produce data, develop theories and summarise results, but not all methods are of equal utility or efficiency. Each of the cases mentioned above employed a design that in part depended on the specific research, but the question of methodology has not been answered to the satisfaction of all. The future in use-wear studies is in systematic approaches, such as that of Grace (1988, 1989), which have predictable errors and consistent outcomes. It is essential to keep in mind too that use-wear traces can be altered by the post-depositional history of a stone tool, possibly obscuring or confusing the interpretation for that piece. Shea and Klenck (1993) did an extensive test on the influence of trampling on these use-wear traces, concluding that only a little bit of walking over the disposed tools can radically affect the evidence. Levi-Sala (1984–86, 1986, 1987, 1989) devoted most of her research efforts to the study of post-depositional surface modifications (PDSM) on stone tools, finding many forms that appear like use-wear. This issue takes on much significance later in this chapter.

Combination studies are another recent development that provide more output from a similar amount of time input. Use-wear combined with debitage analysis illuminated the problems associated with identifying lithics as having been created by the bipolar flaking technique (Jeske and Lurie 1993). One of the hurdles in working with lithic products is the limited scope for artistic input in the final product. Unlike ceramics, too much variation in the tool form impacts on the function.

Residue analysis is the other primary means to gaining functional interpretations of lithic assemblages (Brown 1988) although results of these studies are even more controversial than for use-wear studies (Custer et al. 1988, Gurfinkle & Franklin 1988, Smith & Wilson 1992, Nelson 1993). Organic residues have been identified on tools around the world. It is not only possible to identify many of these accurately, but also to date them using the latest radiocarbon procedures (Loy 1990, Loy et al. 1990).

Grace (1996), an early opponent of residue analyses, accepts that residue studies which rely on use-wear examination of the tools to accurately identify residues related to the use of the flake are a valid technique. Many successful residue studies have done exactly this (Hall, Higgins and Fullagar 1989, Fullagar, Meehan and Jones 1992, Sobolik 1996, Fullagar and Field 1997, Barton, Torrence and Fullagar 1998, Kealhofer, Torrence and Fullagar 1999). One must conclude that, like use-wear studies, residue analysis as a general approach will slowly become an accepted part of archaeology.

A highly controversial aspect of residue analysis concerns identification of blood found on ancient tools. Before this type of study is more accepted it will require the elucidation of the mechanism by which blood residues are preserved over long periods of burial (Cattaneo et al. 1993, Grace 1996). Having witnessed Loy's procedures for dealing with blood residues while sharing lab space with him, I was transformed from an outright sceptic to a cautious supporter of this pursuit. Despite recent arguments concerning blood residue analysis (Fiedel 1996, Newman et al. 1997) there has been enough successful research in this area to demonstrate the potential it holds (Hyland et al. 1990, Kooyman, Newman and Ceri 1992, Loy 1992, 1993). The extent to which I used residue analyses is discussed later in this chapter.

Ambrose, Bird and Duerden (1981) noted that a systematic study of obsidian use-wear and technology had not been undertaken in Melanesia but would be useful because obsidian properties make it a good indicator of past activity—it is rarely hoarded and reuses poorly. Rathje (1978) compared Olmec and Lapita culture and found a broad series of similarities between them, right down to their utilisation of obsidian. Unfortunately, no one has undertaken a systematic study of the regional Olmec obsidians.

DEVELOPING A USE-WEAR PROCEDURE

In order to compare utilisation of the raw obsidian amongst the various sites, islands and time periods, it is necessary to consider seven variables. These are: the source of the raw material, the morphology of useable flakes, the reduction strategy that created those flakes and whether or not this reduction was carried out on site, the relative ubiquity of the flaked material and the intensity of its use. Perhaps the most difficult to determine and the most revealing to consider is the actual function which the flakes were assigned.

Functional analysis can reveal many things about the obsidian—the range of tasks the flakes were put to, the intensity of use, and the importance of the material to that social group. Functional analysis divides the two classical categories of 'debitage' and 'tools' into subclasses. Used, useable and unused flakes, and used and unused tools. It divides up used flakes and tools based on actual tasks, rather than on morphological grounds. Sometimes a side scraper was used to cut, a blade was used for scraping and many formal tools remained unused. The activities completed within a site can be determined on the basis of a tool use profile, and ranked in order of importance (Healy et al. 1992).

Determination of function using a visual inspection system is quite a specialised task. Given my own background in the functional analysis of flint (Hanslip 1991), I was confident that the change in raw material to obsidian would not present any major difficulties, just the necessity of reorienting on the particulars of wear on obsidian—an attitude reinforced by the work of Kamminga (1982). His pioneering work with Australian stone tools examined fourteen different raw materials, including flint and obsidian, almost interchangeably. With this in mind, I undertook a "blind test" experiment for two major reasons. It gave me the opportunity to work with, handle, and inspect a number of obsidian flake tools. Also, it would allow me, when completed, to know how accurate my archaeological interpretations might be. Systematic and random errors could be exposed through a careful examination of the results.

It is not a trivial matter to just produce a large number of flake tools, especially made out of a raw material unavailable locally. Someone with knapping expertise needs to produce the basic flakes. It is useful if their forms are similar to the archaeological ones. Then a series of tasks need to be devised and carried out: all of this without the knowledge or input of the person to be tested. Clayton Fredericksen made available his collection of blind-test tools. These were prepared and used by an experienced lithics person in New Zealand as part of Fredericksen's MA research (1987). Numbering nearly one hundred, the assemblage was almost perfect for my purposes.

The obsidian is from Mayor Island, New Zealand, is of good quality and dark colour like the Admiralties and Talasea sourced obsidians I studied. The flake production and subsequent tasks were completed with Polynesian culture in mind, rather than

Melanesian, but this cannot be far off in the range of potential tasks given the general cultural similarities, and the shared ancestry. It relied mostly on flakes, with few formal tool types; again a good match. All the tools were used for a single task only, or left unused, and all were well-washed in detergent after use to remove as much residue as possible.

My only knowledge at the start was the above description of the obsidian tools, including the extremely vague statement that the task assignments of the tools were "typically Polynesian". I started off by examining all of Fredericksen's collection and selecting seven flakes that I felt would be valuable as test subjects. I spent several weeks examining these seven flakes at both low and high power magnification, and under stereo and incident light microscopes, for traces of use. I had worked with obsidian before and had quite a lot of knapping and use experience with both flint and obsidian so I adapted (without much adjustment required) the use-wear examination techniques that had worked for flint. This step took a long time for several reasons: the particular microscopes being used were similar to what I had used before, but a period of familiarisation was necessary, especially with the photographic portion of the equipment; I was uncertain which variables of wear were going to show up and then which of these were important—after examining the seven tools I collated the data and then ran through the group again, twice; and finally, after I had made analyses that satisfied me I added the unusual step of returning to the tools to compare the use-wear with the proposed use.

I submitted my results to Fredericksen and we were both pleasantly surprised to find that six of seven were correctly interpreted. I then needed to select a sample from the remainder of Fredericksen's assemblage, as it was unnecessarily large for the task. If I had spent the amount of time on the forty seven chosen flakes that I had spent on the first seven, it could have taken a whole year just to complete the blind test. With the established procedure the total time investment was approximately two hours per tool, but was somewhat quicker at the end than at the beginning. I would estimate the future time investment for use-wear study to be on the order of ninety minutes per tool for tools like these that were in excellent condition.

At the Institute of Archaeology in London where I completed my MA, we used an interpretive program that aided the technician's consistency and speed by taking the observations and calculating from them the functional assignments (Grace 1989). This program, running on a Macintosh computer, was named FAST for Functional Analysis of Stone Tools. It was developed by Roger Grace as part of his PhD research. Such programs are generically referred to as expert systems. In using the program, the user made a series of observations at two hundred times magnification, and entered the appropriate response into the computer. Each response was weighted towards certain functions more than others, and some negatively. For example, an acute edge angle added points to the score of 'cutting' and subtracted points from the score for 'scraping'. Once all observations were noted and entered, the computer tallied the scores. The function with the highest score was the most likely use for that tool. Human judgement was used on tie score cases.

In order to adapt the FAST program from its knowledge base (developed over approximately ten years) of European Upper Palaeolithic flints to Pacific obsidians, I would have had to devote at least one year completely to experimenting with obsidian tools, and even then success was not guaranteed. Unlike human experts, expert systems are very inflexible and unadaptable. An example of this is the limited success I achieved in trying to develop software to work with Neolithic flint tools, even though I had data from some twenty-five thousand experimental tools and a powerful mainframe computer to determine statistically which traits were important. I could consistently outscore the resulting software on Neolithic tool use-wear; something I never succeeded in doing with the original expert system. It is also quite likely that my computer programming ability, particularly that of creating an expert system, is insufficient to utilise the input data optimally. I worked alone, whereas most expert systems are written with the assistance of a specialist programmer (Dayton 1994).

This time investment was not a practical option, particularly given the absence of naturally occurring obsidian in the Canberra region (necessary to complete the large numbers of experiments). So I did my best to adapt the observations list and the process of weighting factors along with my experience of observing use-wear to create a manual system of functional analysis. I modified the system used in London to work with obsidian, meaning some observations were no longer relevant, and a few new ones had to be added.

In the final process, I examined each piece carefully under a stereo microscope at magnifications ranging from ten to fifty times. Observations about the nature of the raw material and the specific macro-level use-wear were recorded. At this point the used edge was usually obvious. In those cases where it was not, a process of elimination was attempted. Occasionally higher magnification was necessary, and in a few cases a used edge was still not readily discernible.

Then the incident light microscope was used. While both fifty times and two hundred times magnifications were used, all observations were keyed to two hundred times.

The micro-level use-wear was characterised during this process, as well as the examination of any residual matter present. Traits examined at this level of detail include:

—quantity, type, and size of fractures present

—edge rounding

—distribution and invasiveness of wear

—directional indicators such as striations and angle of flaking (if any)

—presence of polish, gloss, or most importantly on obsidian, residue films.

The approach to blind testing, and the scoring system, derive from The Institute of Archaeology through Mark Newcomer (Newcomer, Grace and Unger-Hamilton 1986). The Institute scoring system demands that each blank be filled in, even if it is with a guess or hypothesis. While I think that this is a bad idea when dealing with archaeological tools, I retained it in this test for comparative purposes with other blind tests done elsewhere (Bamforth 1988, Newcomer, Grace and Unger-Hamilton 1988). For scoring purposes, an answer is either correct or it is incorrect. There are no part marks assigned for being close. And it is hierarchical in that if one chooses the wrong used edge, the rest of the inferences are based on false data, and so cannot be correct even if they do coincide. Further, it should be obvious that one cannot choose the correct action and the wrong motion, or the correct worked material and the wrong relative hardness.

To the Institute's usual five observations for each tool, I have added a consideration of duration of edge use to this test, so in total there were six conclusions to draw for each piece:

—location of used edge (if a tool has more than one used edge, each is treated separately as a different tool);

—motion of edge relative to worked material (e.g., transverse as in cutting, perpendicular as in scraping, and rotary as in drilling are the main three);

—activity tool was involved in (e.g., cut, scrape, slice, chop, drill, punch, grave, etc. these terms are defined in the section on use-wear recording);

—relative hardness of worked material (soft, medium, or hard; for example plant materials and animal flesh are labelled as soft, most wood and soaked antler are medium, and hard woods, bone, shell, stone, and teeth are considered as hard);

—type of worked material (I do not try to get as specific as species, but limit my choice to animal, bone, plant, wood, shell, etc.);

—duration of use (short; less than ten minutes, moderate; ten to twenty minutes, medium; twenty to forty-five minutes, or long; longer than forty-five minutes, were used as categories).

Despite the fact that the tools had been scrubbed in detergent, there were residual traces on many of them. As this was to be a test of visual analytical abilities, I chose not to subject any of the residues to chemical analyses, which could have improved the accuracy of the functional assignments. As a result, the residue's presence was often inconclusive or even detrimental to identification of use when they covered up wear traces.

Overall I scored 223 correct responses out of 324 possible, for a sixty-eight per cent rating. If I discount duration of use (a variable few use-wear studies have considered), my score goes up to 202 correct out of a possible 270, for a seventy-five per cent rating. I did rather poorly on the time element on the obsidian tools, often underestimating the time by overestimating the amount of damage the use would have imparted on the edge. Edge stabilisation has been noted during obsidian use by other researchers (Anderson 1980, Anderson-Gerfaud 1981, Hurcombe 1992); perhaps this occurs more readily than I had been aware of. This would be improved by more experience with obsidian tools. A forty-four per cent (24/54) correct rating, however, and the errors being systematic under-estimates, reveal that there is something positive there to work with.

A seventy-five per cent score compares quite well with Larry Keeley's original test (Keeley 1980) that initiated such interest in use-wear nearly two decades ago, and is typical of blind tests reported in the intervening twenty years. It does however fall about twenty per cent short of the latest successes of R. Grace (pers. comm.) with his fully computerised scoring system. This must be expected, I suppose, given the introductory and investigative nature of this experiment.

The breakdown of individual class scores is as follows:

—fifty-one out of fifty-four were correct for used edge (ninety-four per cent correct);

—forty-four out of fifty-four were correct for relative hardness of material (eighty-two per cent correct);

—twenty-nine out of fifty-four were correct for specific material (fifty-four per cent correct);

—forty-one out of fifty-four were correct for tool motion (seventy-six per cent correct);

—thirty-four out of fifty-four were correct for tool action (sixty-three per cent correct);

—only twenty-four out of fifty-four were correct for duration of use (forty-four per cent correct).

An analysis of the errors reveals a few trends. The three completely wrong tools were: scraping sweet potato, graving wood, and scraping flax. There are two scraping tools, which I find very interesting and surprising since I have the most experience with scraping tools, and my MA research tools were all scraping implements. The incorrect interpretations were both from use on soft materials. It could be that they are too soft to leave much use-wear, at least wear that I am readily able to identify.

Once I was satisfied that the technique was working correctly, I started on the archaeological flakes. The intent had been to study a sample from each site, starting with twenty flakes per site. This would be one hundred per cent of the smaller sites and less than one per cent of site SAC, but was the most workable compromise. I soon discovered that the flakes were generally in extremely poor condition. Tens of flakes were rejected as unsuitable for study as I sorted through the assemblages.

Except for the very smallest (unusably small) and the most damaged examples (obvious to the naked eye) the remaining flakes (most of them) took fifteen to twenty minutes of examination under a low power stereo microscope to process. Much time was invested here with little return. More flakes were incorporated until eventually one hundred per cent samples were processed for each site.

The preliminary examination consisted of looking at the flake with a variable magnification stereo microscope in the range of eight times to twenty-five times. Each edge and the surface were examined for marks and residues. In this case, those tools that were tagged to go to the next stage of examination were any that satisfied one of three criteria. The first criterion was if the flake had differential damage to one edge, corner, or area of the tool, or more than one area for potentially multi-use flakes, and if it was not completely covered in surface damage that would obscure all traces of use. The second criterion was if the flake was a graver of the style identified by Peter

Sheppard (1992) in his study of Reef/Santa Cruz lithic materials. The final criterion was if the flake was completely pristine, appearing as if created recently. There were only a few of these pristine flakes, but it is often the case that under two hundred times magnification damage becomes apparent that was not visible at twenty times magnification.

USE-WEAR RECORDING PROCEDURE

As each flake was examined, it was entered on a single line of the bulk recording sheet (fig 8.1). This sheet allots one line per flake, as there was not much data to be recorded at this stage of the analysis. First the flake identification number (each one in my collection was assigned a unique number for reference) was entered, then comments about the ventral and dorsal aspects of the flake were noted. If the flake was to be processed further, either because it was obviously used, possibly used, or probably unused it received a Y mark in the 'used' column.

An obviously used flake is one that under the low power investigation has marks on an edge that are consistent with being produced by use. A possibly used flake is one of many possible configurations. It could have some evidence of use that are not clear under low power magnification, or it could have some non-use marks on it that provide conflict with the probable use marks, or it could be a flake with wear that is not easily visible at low magnification (this last option differs little from an unused flake except that it is not clearly unused). A probably unused flake is one that is in such good condition that it would be unlikely at higher magnification to reveal any use-wear. Mostly this latter category of flakes is restricted to those that appear freshly knapped with no surface damage at all. In a normal situation where the vast majority of flakes would be unused and obviously so, this condition would not warrant further examination, but with such poor preservation of use-wear traces in these Melanesian obsidians, I wanted to inspect the few pristine flakes as much as the used ones. In fact, the presence of a Y in the 'used' column of the observation sheet came to refer not strictly to utilised flakes but rather to any interesting ones worthy of closer inspection.

Once all of the flakes from a site were inspected, it was simply a matter of collecting those that had a Y mark in the 'used' column for processing in the next stage. I repeated the use of these one line observation sheets for step two. Using the flakes passed on from step one, more careful examination with higher magnification was undertaken again using the stereo microscope.

Finally, those flakes that were still believed to have been used were passed to the tertiary inspection procedure. Each flake then received its own observation sheet, with room for detailed recording (fig 8.2). In addition to provenience data, known chemical and physical treatments were noted (which ranged from being sectioned for analysis to ultrasonic cleaning and immersion in dense liquids for sourcing) which could affect the integrity of other observations, especially residues present. Also, the source if known was recorded, as I thought that any patterns that might have been present that related to source would readily emerge with these data on the sheets.

The tool *type* was severely limited in choices with these particular tools. Either a *flake* or a *core* or *other*. In practice this was reduced to two choices with cores being ignored (see p. 103). *Other* encompassed the graver type tools, and the occasional chunk or other piece of knapping waste. With obsidian, an acute angle is less than thirty degrees, an obtuse angle is more than sixty degrees, and intermediate angles, that in my experience are less frequently used for working edges, fall in between. *Platform* condition was aimed at identifying systematic knapping procedure. The *retouch* observations completed the non-magnified observations for each flake.

Evidence gleaned prior to this step using the stereo microscope was transferred here to ensure that I could use it to locate the important edges easily. This constituted the macro evidence. The micro evidence is the important set of observations with regards to the functional conclusion.

Fracture numbers along the used edge fall into three categories: none, some or many. The recording sheet term of ≤5 refers to "per field of view at two hundred times magnification". If there are any fractures, then the *fracture types* became important: *flakes*, *steps* and *snaps*, their logical combinations, and *other* are the important variations. *Other* encompasses all but the specifically mentioned types (e.g., comminution and flute fractures are non-existent in these assemblages) and all other mixtures as well. *Small*, *medium* and *large* refer to the apparent *fracture size* at two hundred times—all are invisible to the naked eye. *Macro* fractures are those visible with the naked eye (from just visible to very large).

Edge rounding occurs when the edge is abraded, rather than flaked away. If it happens at all (very rare on brittle obsidian) the critical difference is if it is *light* or *heavy*. *Light* is confined to the very edge, *heavy* rounding changes the contour of the edge and the surface behind it.

Striations have proven very diagnostic in obsidian use-wear studies before (Hurcombe 1992), and are always so with flint. They proved, however, to be almost universally absent in the flakes studied here. When *parallel, perpendicular* or *oblique* in alignment to the edge, striations are strongly indicative of the tool's direction of movement during use. Virtually all PDSMs leave *random* striae on the surface. This random pattern is, in fact, diagnostic of PDSM because no normal tool uses leave random marks on the tool.

Invasiveness is a measure of how resilient the worked material was. Rigid objects are non-invasive, while very flexible objects are much more invasive. This is roughly comparable with hard and soft materials, but they are not exact synonyms. A very soft material worked over a rigid backing is still soft but not so flexible. Invasiveness becomes diagnostic of flexible object working at an extent of 650 to 750 micrometres. With the Olympus microscope I was using, seven hundred micrometres corresponds to half the field of view during two hundred power magnification observations. Therefore this easy observation compared to "half the field of view" is used. Reference to invasiveness refers to all forms of use-wear. Striations, polish, etc. can be confined to the very edge, or as in the case of sickle gloss on grass harvesting blades (for example) the evidence of use-wear can cover the entire tool.

Distribution differs from *invasiveness* in that it measures use-wear along the edge and not inwards from it. *Intermittent*, as the name implies, is a discrete form of distribution with two or more areas of wear interspersed with bare patches. *Continuous* is a self explanatory category. *Asymmetric* varies in its invasiveness along the length of the distribution. *Away from the edge* is wear that has a bare gap between the edge and the use-wear.

Polish does occur on obsidian, though I dislike the use of the word polish itself to refer to this phenomenon. On flint and other stone, the freshly fractured surface is somewhat rough. Under high magnification it appears to have (in good quality material) a uniform but visible topography. Use-wear reduces the high points, flattening the surface and increases its reflectivity; hence the name polish. It is this polish that stimulated use-wear studies in the first place (Semenov 1964, Keeley 1980). Obsidian fractures with a smooth mirror-like surface. Even if percussion waves and micro-topography are visibly present on a complex surface of a piece of obsidian, it is impossible to imagine how it could reflect more light. A "polished surface" on an obsidian tool has a more matte appearance than a fresh surface does. The processes that create these changes are the same in the case of flint and obsidian, and they can be easily replicated in a laboratory situation (Yamada 1992).

Finally, I noted anything of interest to do with residual matter on the stone surface including: colour, structure, location, arrangement, and invasiveness. All of these help to determine if the residues are a product of use or not. Often a flake will pick up things present in the soil it was buried in, and in some cases the residues are post-excavational in origin.

Unlike Barton and White's (1993) results, virtually all of the residues I observed were non-diagnostic of use for one of three reasons: First, they were uniformly distributed across the tool, on used and unused areas alike. Second, many residue types from plants in particular (pollen and fibres) were ubiquitous amongst used and unused flakes from a single locus. Finally, frequently observed residues were determined to be post excavational in origin with the two biggest offenders being glove starch (from handling with 'care') and ball point pen ink (from tracing the outline of the flake with a biro). Residues indicative of how the flake was held during use (almost like fingerprints) were observed on many flakes.

Functional attributions were assigned from the following (non-exhaustive) list:

adze - motion incident to edge, tool at acute angle to worked surface, percussive

chop - motion incident to edge, tool perpendicular to worked surface, percussive

cut - motion parallel to edge, tool perpendicular to worked surface, non-percussive

drill - motion is rotary, tool perpendicular to worked surface, confined to projecting point, non-percussive

grave - motion perpendicular to edge, tool perpendicular to worked surface, confined to projecting point, non-percussive

pierce - motion incident to edge, tool perpendicular to worked surface, confined to projecting point, non-percussive

scrape - motion perpendicular to edge, tool perpendicular to worked surface, non-percussive

shave - motion incident to edge, tool at acute angle to worked surface, non-percussive

slice - motion incident to edge, tool perpendicular to worked surface, non-percussive

projectile - motion incident to edge, single impact

punch - motion incident to edge, tool perpendicular to worked surface, confined to projecting point, tool struck from opposite end by hammer.

About nine thousand flakes altogether, averaging eight minutes per flake, makes for a total of roughly thirty weeks. Fewer than five hundred pieces in total were identified as worth examining further because they were potentially used flakes. These were further examined more carefully using higher power magnification (twenty to sixty times on the stereo microscope and fifty times on the incident light microscope). With an average of ten minutes more per flake, this step took about three weeks. Ninety-five per cent of flakes were rejected in step one, and another ninety per cent were rejected at step two. The result was one hundred and forty-eight flakes that were categorised as probably used. These were looked at under two hundred times magnification and subjected to full analysis. This third step involved eighty to one hundred minutes per tool for tools that were assigned a function. Forty-five 'functions' were assigned. The remaining one hundred and three flakes were less damaged than ones rejected at earlier stages, but still had too much PDSM to be given a functional assignment.

Of the forty-five, two of these were not used, eleven were cutting tools, eight were scraping tools, one a shaving tool, two were piercing tools, and the remaining twenty-one were gravers. The gravers will be dealt with separately, but I must mention here that there were more than twenty-one typological gravers, but only twenty-one of them were in good enough condition for use-wear analysis. The rest were too damaged to know if they were ever used or not. The two unused tools are also interesting, because it would be considered normal for approximately two-thirds of a European Palaeolithic collection to be assigned a 'function' of unused (Healy et al. 1992, R. Grace pers. comm.). The proportion of unused pieces in other settings would be subject to much variation from this amount, but one would still expect a high proportion in any flake assemblage to be unused. There were a vast amount of the tools unclassified, but I would not expect that preservation conditions were dependent on function (or lack of it). It does not seem, therefore, unreasonable to assume that two out of forty-five (4%) is approximately the proportion of unused tools in the entire collection.

While the functional analyses included all the sites in the study, the forty-three used flakes came from five sites; SAC (one), EAB1 (eight), DGD2 (three), RF2 (twenty-two), and SZ8 (nine). About two-thirds (twenty-nine) of these flakes derive from Talasea. The other one-third (fourteen) come from Lou. Given these low numbers, there is little that one can conclude regarding obsidian use in this region. It also prompted me to look for information that would normally not be considered necessary.

Table 8.1 displays the results of this further investigation. Examination of each flake was made to determine if it was a potentially useable implement or not. Those flakes that I judged large enough to be used while hand-held comprise the third column.

All these flakes plus the ones too small for hand-holding but potentially useful in a hafted mount are included in the second column. Pieces were excluded only if they were clearly too small to be a working tool, or if they had no working edge.

GRAVERS

The gravers of the Reef/Santa Cruz group had the most potential for the application of functional analysis as a category. Gravers are formed on flakes by removing a portion of the edge on either side of the point by opposed retouch. That is, a typically two to three millimetre width and height projection is created by reduction of the adjacent edge; reduction that is ventral on one side and dorsal on the other. When Sheppard (1992) assigned them the name 'graver' he used a functional label. Like a side scraper or other implied function term, the use of gravers for graving is questionable.

A graver is defined in the Oxford English dictionary (Little, Fowler and Coulson 1973: 884) as a tool used to cut, engrave or incise letters (etc.) into a hard surface. This corresponds with the definition of graving in the section above. The fragile nature of a point tool made of obsidian however prevents any sort of graving actions with these so-called gravers.

In the thousands of other flakes in this study that do not come from within the Reef/Santa Cruz group I found only two flakes that could have been gravers. The one from Lasigi village, New Ireland was highly questionable (I discounted several like it in the Reef/Santa Cruz group) and the other from site DGD/2, Nissan was not made by opposed retouch, so both were discarded. The graver style appears to be a Reef/Santa Cruz innovation on this evidence. Peter Sheppard (pers. comm.), however, tells me that he found a few graver points in P. Kirch's Mussau collection when he made a brief examination of that obsidian assemblage. More recently Sheppard noted at least one graver point (probably two) in C. Sand's New Caledonia collection (Sand and Sheppard 2000, Sheppard and Sand 2000). Thus the type is not unique to the Reef/Santa Cruz islands, but not a ubiquitous tool either.

I identified sixty gravers from the Reef/Santa Cruz group. Fifty-one of these were single gravers, and nine were double ended gravers with a graver point on two sides. One of the double gravers might have been a triple graver, as it had a poorly defined third point on the same edge as one of the other points. Were this point by itself, the flake would not have been counted as a graver at all, but it did display opposed retouch and light use-wear on that third point. Twenty-one of these were in good preservation and all appeared to have been used. Five of the twenty-one gravers were double gravers. None of the double ended tools appeared to have both points used (this could be an artefact of the preservation conditions, but seems unlikely).

Much experimentation with replicas was carried out to try to determine the function or functions of the archaeological gravers. While I was in Papua New Guinea as part of the Australian Museum project on Garua Island directed by Robin Torrence, I had an opportunity to make and test several dozen gravers. The typical graver cannot sustain any load bearing, so it is not possible to use it on any medium or hard category materials. The logical use edge is the chisel-like end of the graver's point, as opposed to the side of adjacent edges on the flake itself. I conclude this because the adjacent edges are not consistent from tool to tool.

Rotary motion, such as boring into leather, results in loss of the graver point. Percussive penetration also usually removed the whole point. Piercing motions were more successful, but depended on the morphology of the point; some did not survive this motion either. Slicing and incising in fresh animal flesh did work, but I am unable to see much purpose in this action. The only process I am aware of by which a useful task could be accomplished with these points is by cutting into living flesh for the purposes of body decoration, such as in scarification (or possibly tattooing), since the cut is rarely more than two millimetres deep.

If this is the case, that these gravers are really scarring points, or some related tool, then it is possible that most or all of them were actually utilised. It would be a rare occurrence that cicatrisation would cause edge damage on the point other than a slow dulling of the sharp edge, and obvious use marks on the points were rare indeed. Since seven of the flakes appeared to have a blood-like residue on their surface, I tried a hemastix® reaction test, as described by Loy (Loy and Wood 1989, Loy 1992, Loy and Hardy 1992).

The hemastix® procedure is to deposit a small amount of ultra-purified water on the surface of the residue and 'mix' with a small stick. The water is left for fifteen to twenty minutes to allow time for dissolution to take place. The liquid is then transferred to an Ames hemastix® reaction strip that reacts with the ring structure of haemoglobin (and also the ring structure in chlorophyll and some other organic molecules). Colour change and speed are the criteria for scoring the test.

The result was "trace reaction" for two gravers and "no reaction" on the other five gravers. According to Loy, a random sample of objects produces about 20% trace reactions when there is no blood present; these are the false positives attributed to those other organic ring structured molecules (Culliford 1971). No reaction almost always means that no blood is present because the hemastix® test is not likely to give a false negative. Further pursuit of the residues on the two flakes with the trace reaction, and consultation with Loy, concluded that these were false positive reactions. The conclusion is that no blood is present on these gravers now.

Those flakes with graver points on them were not confined solely to graver duty either. In two cases, a cutting edge was identified on a flake that also had a graver point on it. One of these was an obviously used graver point, i.e., a flake with two functions, while the other was a possibly used graver point. While I stated above that cicatrisation would not produce much, if any, detectable use-wear on the point of a graver, it is clear that some of these tools were used for something that did leave detectable use-wear traces on them. The tip and only the tip of some graver points were covered in extremely tiny fractures indicative of (gentle) contact with some hard substance.

THE CHERT

Uniquely in the Reef/Santa Cruz group, obsidian is replaced by a more locally sourced chert over time with some chert appearing in the earliest levels of the sites and in almost equal proportion as obsidian in the latest levels. There were some non-obsidian lithics at other sites as well. Most of these however consisted of oven stones, non-flaked materials, and some stone with no obvious function; virtually nothing in a capacity that could have competed with obsidian in function.

From Sheppard's investigations into the Reef/Santa Cruz material (1992), I already knew that reduction processes were comparable for the two materials. I had intended a comparison of the functional scopes of the two material types; essentially I wanted to know if obsidian and chert were interchangeable. The chert flakes occur in far fewer numbers than does the obsidian, an interesting observation given its closer source and apparently good flaking potential.

After the failure of functional analyses on the Reef/Santa Cruz obsidian, I hoped something could be accomplished with the chert. Working with European flint tools two orders of magnitude older than the Pacific materials did not prepare me for the poor surface condition of the chert. It was in even poorer overall condition than the obsidian.

Sheppard reports counts of 'used' tools from the Reef/Santa Cruz collection. After examining it, I felt uncomfortable with doing even this much. I counted 'useable' tools, but can add nothing to Sheppard's analysis on the basis of my own.

IMAGE RECORDING

Use-wear in particular, but lithic studies in general, benefit from having recorded images of the tools. The traditional method of recording has been via thirty-five millimetre film, exposed through the microscope, and printed on paper. These photographs can be of value to the researcher, allowing for inspection of the tool with magnification away from the laboratory setting. Unfortunately, the light microscope suffers from the limitations of the nature of light itself and at high magnifications there is very little depth of field to work with. Exactly how high varies with the quality of the optics and the type of microscope, but from one hundred times magnification upwards would be normal.

One of the important parts of training for a future researcher to do use-wear study is to be able to integrate the images the microscope presents at different depths of focus into a single useable image. The camera can only record a single focus position, and even the most painstaking manipulations of the stone tool on the microscope stage cannot alter the fact that most objects have extreme relief at the micro-scale. The typical use-wear report of the 1980s, as well as the typical presentation of this decade, contains a series of shots that are often useless in communicating anything to the audience (e.g., Keeley 1980, Hurcombe 1992; worst of all were the endless slides at the microwear conference in Liège in December 1990). The plates presented in Kamminga's monograph (1982: 180–217) are a noteworthy exception to this trend, avoiding the problem by using many low-power magnification images combined with electron micrographs when necessary to show high magnifications.

Some photos are useful though, and all are presented together in this section. Light photographs are presented first in figures 8.3 through 8.9. Scanning electron micrographs are presented in figures 8.10 through 8.13. Finally some experimental images produced using a confocal laser scanning microscope (CLSM) are presented in figures 8.14, 8.15 and 8.16. Figure 8.3 reveals several flake tools and will be discussed below. Figure 8.4 is of the graver point on tool 775 from the Reef/Santa Cruz group. Visible in this image is the opposed retouch that characterises the graver tool type. The entire bottom left of the photo is out of focus, but it is also presented at the wrong angle to the incident lighting to reveal much detail, and I was interested in displaying the edge. The white tracks running from the point downwards are examples of PDSM—much of this side of the tool is covered in scratches roughly parallel to those in the field of view but neither parallel nor systematic. These are perhaps a result of trampling underfoot and are not related to use.

Figure 8.7 shows the same tool, but the image is at much greater magnification and is of the graver point end on. The upwards direction in the photo is the side of the tool viewed in the previous figure—the ventral side of the flake. At this higher magnification, it is very difficult to get much of the field of view in focus. A portion of the very edge is in focus and the tiny fractures visible there are evidence of use for this tool. These fractures are not present on any other edge of the tool.

A final photograph of tool 775 is in figure 8.8. What first appear to be scratches in the smooth surface of the tool are revealed on closer inspection to be erosion marks. At greater magnification they appear very similar to those in figures 8.10, 8.11, 8.12, & 8.13. This is discussed further in the section on erosion below. It would be easy to confuse certain forms of erosion with use-wear if one was not careful. The random location of the marks and the lack of consistent complementary damage reveal the post-depositional nature of the erosion.

Tool 797 is another graver from the Reef/Santa Cruz group. This one has two point projections on opposing ends of the flake. Figure 8.5 displays one of these points while figure 8.6 contains an image of the other point. These two points together with the one in figure 8.4 cover most of the range in morphology for the Reef/Santa Cruz gravers.

Figure 8.5 is a classic example of the fuzzy photograph. While it is sharp enough to convey the information of rough morphology, it lacks enough clarity to be very useful; not even the flake scars are visible in the image. It is not even a high magnification photograph. Figure 8.6 is more useful, in that it is possible to make out the opposed retouch on the point, the rough surface microtopography, and the residues that 'flare' in dark field illumination. When photographing relatively flat surfaces the images can have excellent sharpness. Figure 8.9 is one such image showing the environmental damage that occurs on the surface of the Reef/Santa Cruz chert. Tool 1000 (pictured here) is an example of the worst of the assemblage. The cracked surface layer is thick enough (several tenths of a millimetre) that you cannot hold the bottom of the cracks in focus at the same time as the top surface.

Bearing in mind the limited successes of the standard microscope and film setup, I sought an alternative method—something that preserved enough of the image that another use-wear analyst might be able to draw their own conclusion regarding a tool's use just on the basis of the photos. The scanning electron microscope has been used to good effect in the past (Yamada and Sawada 1990), but was so labour intensive that few practitioners have pursued this technique. When using an SEM on a non-conductive material, the electrons beamed at the object build up a negative charge and this interferes with the operation of the microscope. Coating the object in a molecular layer of carbon is the main method of removing electrons, as the conductive carbon layer allowed a current to flow around the surface of the object. It is the deposition and subsequent removal of the carbon that is so time consuming.

Like so many procedures I outline in this thesis, the traditional method of scanning electron microscopy was not only grossly inefficient for examining large numbers of flake tools, but also damaging to some residues as a result of the vapour deposition and solvent removal of the conductive layer.

Video turned out to be an excellent method of recording images of the tools, but one that had many limitations. Replacing the thirty-five millimetre camera with a high resolution electronic video capture device, and the film with video tape, allows for viewers to see a segment of the tool's inspection under the microscope. Surfaces of the tool can be panned across and focus can be altered yielding the same information that one achieves first-hand when viewing through the oculars of the Olympus. Displaying, replaying and publishing proved to be difficult obstacles for this medium, however.

I video taped many tool inspections for later re-examination, a method that allowed me to review the tool working edge in a more comfortable manner, watching a monitor rather than squinting through binocular eyepieces. The passive nature of the inspection was a problem. When viewing it myself, or showing it to another for comment, inevitably something just on the edge of the field of view would be of interest, or the panning rate proved inappropriate.

Minor difficulties I encountered were the comparative difficulty in preparing a video for a group presentation compared to using transparencies, and the serial nature of the tape; it is impossible to jump from one view to another several images ahead or behind because the tape needs to be wound on. Storing to a video disc (such as CD or the newer DVD) would overcome both of these problems at once.

The final problem is publishing. It is impossible to put the moving image on a page, and requires computer equipment and techniques beyond the reach of the average archaeologist to make a composite image from several video frames—which would be necessary to overcome the problems already mentioned for film. Again, a digital system of publication could remove most of the obstacles preventing the publishing of video images.[2]

The best imaging success by far was with the advanced SEM located in the Electron Microscopy Centre in the Research School of Biological Sciences at the Australian National University. It is possible to photograph obsidian flakes without cleaning off all the residues, and without carbon coating them first. Normally, as discussed earlier, an electron microscope operates in a high vacuum atmosphere as the air molecules deflect the electron stream. It is possible with modern electron microscopes to operate in a low vacuum environment, and the presence of low pressure air in the imaging chamber helps to evacuate the build up of electrons from the surface of the tool. The presence of air in the system prevents magnifications above approximately 2500 times, but this is much more than the magnification routinely used in lithic analyses.

Figure 8.12 displays semicircular erosion marks similar to those also seen in figure 8.15. Sharp, detailed and with good depth of field, this photo is excellent except for the upper left portion where the microscope scrambled some of the pixels in making the image. Figure 8.11 is an SEM of the same pitted tool surface displayed in figure 8.14. While not identically framed, the SEM is much better able to convey the problems of attempting to do use-wear analysis on this tool than is the CLSM photo.

A promising alternative technology that I was able to experiment with was the confocal laser scanning microscope.[3] Like the SEM, it promises a variety of image magnifications all possessing excellent depth of field. The image is produced in the processor of a computer and manipulated as necessary to display on a CRT. In fact, it can layer together scans of different depth in a transparent object and build up a rotating three dimensional image from them.

Figure 8.14 displays the pitted surface of a tool in false orange colour (the operator could choose any hue desired). The image reproduced here is not as detailed as that displayed on the CRT and suffers from excessive contrast, but the information

[2] Given the recent rate of decrease in the prices of digital video equipment during my tenure as a student it is safe to predict that my statements concerning the unavailability of such resources to the typical archaeologist are either already no longer true, or this will be the case shortly. Certainly these restrictions applied when I began my research.

[3] I was offered one session on the CLSM courtesy of Eric Hines at the CSIRO Department of Entomology in Acton, ACT. Ordinarily they have to charge for external time on the equipment, but apparently they had never used the microscope to view anything but tissue samples. We were both curious if it would produce successful images with opaque materials. I did not pursue this avenue further because of the excessive contrast and poor detail in the final images.

presented (and lack thereof) is preserved. The edges of contour changes are clearly displayed—the erosion pits are the only thing visible in the image—but the flat surface of the stone is not visible between the pits and the edge of the flake (at the top of the photograph) is not visible.

The next two CLSM photos (figs 8.15 & 8.16) are examples of the different kinds of image manipulation possible with this system. The false orange figure 8.15 suffers from the same excess of contrast and invisibility of the tool surface as figure 8.14. Figure 8.16 has reduced contrast and a different method of presenting low surfaces. The detail of the two does not differ, the edges of the erosion trails are similarly visible in both, but the second image better presents the whole object.

A problem that virtually all the images presented in this thesis share is the loss of sharpness from repeated digital to analogue conversion. The SEM photos are digital assemblies of the data from the computer attached to the microscope. They are scanned onto high resolution analogue negative film,[4] which was processed, printed, and then the prints were scanned into a computer at 2400 dpi resolution. The CLSM photos again are constructs of the computer in the microscope controller. The laser light is imaged by the system (the light would be damaging to human eyes to view directly) and turned into digital data. Those images that were deemed acceptable were written to disc and I later received a photograph from the equipment operator (apparently produced by photographing the image as it appears on the monitor). This photo was then scanned into a computer. The video system suffers as well. The camera uses a CCD imaging system, which produces digital image data, it is recorded on an analogue tape system, and were it transferred to computer the image would have to be converted back to digital. All three systems would benefit from remaining in their original digital state until the final printing process.

Recently released digital video camera units may provide the ultimate solution, improving even on the outlook provided by Reilly and Rahtz (1992). With an image resolution approximately two times that of the best analogue tape systems, they bring "broadcast quality" video down to the consumer price range. It is then relatively inexpensive and comparatively easy to transfer the video to a desktop computer and either edit the video or sample out single frames, either of which can then be saved on any digital storage medium for subsequent use. The entire process remains in the

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[4] Many of the electron micrographs suffer from a scanning problem that is obvious when the final print is viewed. The horizontal alignment of adjacent rows of pixels is off, resulting in loss of clarity in the image. None of these other photos have been included in this thesis.

digital realm, avoiding the problems mentioned above. Similar success with even better resolution is possible using digital still cameras, however currently the cost of cameras with this higher resolution is greater than for digital video.

GLASS HYDRATION AND DISSOLUTION—GOOD AND BAD

Once obsidian is knapped into a tool, the freshly exposed surfaces start to absorb water. After tens of years, the layer is thick enough that it can be measured. This is the basis of obsidian hydration dating (OHD), and allows the production date of a specific flake to be estimated (Friedman and Smith 1960). After much initial enthusiasm towards OHD came the realisation that the dates did not agree with radiocarbon dates from the same site; they were internally consistent but 'out' and they did not agree from site to site either (Ericson 1988). The research into this phenomenon discovered that the elemental makeup of the obsidian and the temperature of the ground at the site were essential factors determining the hydration rate (Ambrose 1976b).

Ericson's work (1977) stressed the importance of "knowing your obsidian" before accurate dating was possible via measurement of the hydration layer thickness. The hydration rate varies with the source; therefore one needs to know the source before attempting to date it. Ambrose was a pioneer in both obsidian sourcing techniques and in OHD procedure. The first published study of hydration rate constants applicable to different sources of obsidian in Melanesia was made in 1976 (Ambrose 1976b).

Ambrose (1994, 1996, 1998) introduced a new technique that allowed the dating of formerly undatable obsidian. The hydration layer increases in thickness while a competing dissolution reaction removes surface material (White 1988). By looking along the periphery of small cracks (often a result of the flaking process) an accurate measurement of the hydration layer, and hence the age of the flake, was completed. There are important implications to the hydrating process. Not only is surface material lost over time, but it has also been demonstrated that the major elemental composition of stone can be affected by the weathering process (Sheppard and Pavlish 1992). The former process will affect the potential for use-wear studies on the flakes. The latter process could affect source attributions done via superficial elemental analysis systems (such as PIXE-PIGME) in cases of extreme weathering.

The poor condition of the obsidian flakes from these sites leads to the question of "what happened?" The earliest uses of OHD in archaeological sites often used a radiocarbon date to calibrate the hydration rate curve. Once the factors affecting hydration rate were understood (in the 1970s) the OHD process became independent of other dating systems. Research on glass dissolution advanced further through investigations by material scientists interested in the containment of nuclear waste materials inside of glass castings. Abrajano Jr., Bates and Bohlke (1988) found that the specific proportions of component elements in a glass could impart dramatically different susceptibility to surface dissolution rates; agreeing with the OHD research that treats each specific source of obsidian separately in determining hydration rates.

White (1988) reports that the obsidian weathering process is comprised of a hydration reaction and a dissolution reaction. The hydration process is responsible for the layer that is measured as part of the dating technique. The dissolution causes loss of the surface layer. While obsidian is amongst the most resistant of glasses to the dissolution process, the long time that artefacts remain in the ground makes them susceptible to even very slow reactions. Bates et al. (1988) experimented with accelerated dissolution regimes and discovered that the amount of water present in either vapour or liquid form was the key factor in determining the rate of reaction.

Knauss et al. (1989) looked specifically at the reaction kinetics. They found that pH, temperature, water flow rate, and chemical composition of the glass determined the reaction rates in each case. As pH varies away from neutral the reactions proceed faster, but acidic and alkaline environments proceed to different endpoints and will leave different traces on the glass. Increased temperature drives the reactions more quickly in an exponential manner. Water flowing past the glass surface removes the ion solution that both the hydration and dissolution reactions produce. This solution impedes the reaction, so more flow equals faster reactions.

Ericson (1988) and Schulz, Clark and Folz (1992) took this material research data back into the specifically archaeological realm by looking at artefacts in terms of dissolution kinetics. The implications of all this research are exemplified spectacularly in the Melanesian obsidian flakes examined here. On one site several flakes from one excavation unit are found in perfect condition while others from the same unit are highly eroded. At another site the flakes in one unit are unaffected while those in neighbouring units are damaged. Even on occasion one side of a flake can be quite differently affected than the other side, and the line of sectioning occurs on apparently random axes.

I conclude on the basis of observations of these obsidian flakes that dissolution reaction kinetics can be affected by localised conditions in a single site. While I saw only a few examples of difference on a single flake, these differences do exist and require explanation. For one half of an obsidian flake to look like it was chipped only recently, while the other half is noticeably eroded suggests to me that at least one of the factors affecting reaction kinetics was different for the two portions of the flake. It is extremely unlikely to be the glass chemistry itself or the temperature that varies across the flake. Yet neither pH nor water flow rate seem like particularly good candidates for variation on such a small scale. Temperature variations across an archaeological site are documented (Ambrose 1976: 102–3) while variations in pH and humidity really need no documentation. It appears as though these can occur on extremely small scales as well.

An examination of the archaeological material reveals three general 'types' of dissolution marks: a series of aligned arcs etched in the surface (figs 8.12, 8.15 & 8.16), one or more quite linear depressions across the flake face (figs 8.8 & 8.13), and the most frequently observed—a multi-layered collection of pock-marks that recall impact craters on the Moon (figs 8.10, 8.11 & 8.14). The papers on glass erosion referenced above reveal nearly identical photographs of the same phenomena. What look like the results of some organised process are actually the results of chemical reactions attacking vulnerable areas of the glass surface. An interesting observation is that much of this erosional process is fractal, or self-similar, in nature; the image hardly changes despite great changes in the magnification (figs 8.8 & 8.13; cf. Rees et al. 1991, Buchanan 1997).

While this explains why so many of the flakes I examined were in poor condition, with Melanesian conditions being hot, humid, subject to high rainfall, and extremes of soil pH occurring frequently, it does not explain why this condition has gone virtually unreported in the use-wear literature to date. An informal survey I conducted of several obsidian use-wear researchers revealed that all were aware of the potential for this damage but only one suggested it had been a problem and that was on a single site (Fullagar 1992, R. Fullagar pers. comm.). A logical place to find this kind of hydration damage is in Mesoamerica where climatic conditions are much the same as Melanesia. No use-wear studies in the region (e.g., Widmer 1993, Doonan 1996, Aoyama 1995) report the analysis being prevented by surface dissolution of the obsidian flakes. Other areas of the globe where obsidian was an important raw material, such as the Pacific coast of North America and in the Mediterranean, perhaps lack the conditions (pH, humidity, temperature) leading to significant surface dissolution reactions occurring. It is therefore something of a combination of bad luck and coincidence that I tried to analyse so many damaged assemblages.

METRICAL ANALYSIS

Comparison of size for each flake, layer, assemblage, and island in this study allows averages to be calculated and both inter- and intra-site values to be considered. Typically, archaeologists record one or more variables from a long list of possible measurements (e.g., Irwin and Holdaway 1996); including longest axis length, length as produced, perpendicular width, width as produced, maximum thickness, spinal thickness, surface area, plan area and perimeter.

Most of these assist in achieving very specific goals. To formulate such specific questions for the data requires a high level of knowledge about the tools themselves and the culture that made them; a level of knowledge that does not exist for Pacific obsidian. The current investigation was therefore more general in approach and exploratory in nature.

Using automated, electronic data-gathering tools, specifically Mitutoyu digital callipers with PC interface and *Entrer Trois* software (co-written and supplied to me by Simon Holdaway) running on an IBM PC compatible computer, it would have been very straightforward and quite simple to measure one hundred per cent of the obsidian flakes for any number of linear dimensions. Once the software is programmed to record the desired dimensions it takes just a few seconds to make each measurement and push a button to save it. On the basis of a brief preliminary examination, I concluded they were all similar 'flakes' without any stylistic variation.

With no tools (save the gravers) evident at all, the flakes all seemed to have been produced by a similar technique with similar results. I felt I could reduce the number of measurements made on each flake without reducing the value of comparisons made between groups of flakes.

I already knew from the sourcing data that the range in the density of the obsidian from different sources was less than three per cent. The measurements of length, width and thickness create a volume measure—albeit one indicating the minimum size box that the flake would fit into rather than an exact flake volume. I believed that if the flakes tended towards an average such that thickness varied as an inverse function of the length or width that mass would relate to the theoretical box volume by a constant. This proposed relationship for flake morphology is observed in free-hand percussion flaked obsidian. Figure 8.3 displays several obsidian flakes that I produced which display this variation, these experimental flakes are typical of the archaeological ones. The flakes that are of roughly equal length and width also tend to be comparatively thick. Those flakes that are blade-like in being long and narrow are also thinner. I believe this is a linear relation with a small random error. In other words, I believed if I knew actual volume, I could calculate the product of L*W*T. This "theoretical density" would allow me to characterise the flake size of an assemblage without actually measuring even one linear dimension of the flakes. All I needed to know was the actual volume—which is easily calculated from the mass.

All of the flakes were weighed (the raw data from this process have been made available through the *Archaeology World* web server, URL: http://artalpha.anu.edu.au/web/arc/resources/pacific/expedient/expedient.htm). I did this using a Mettler AT261 electronic balance and recorded the output using a serial connection cable with a DOS computer running *Entrer Trois* software. With a research assistant's help it took only a few seconds to weigh each flake with almost no potential for error (as many as one hundred and seventy-five weights per hour with assistance, and eighty per hour alone). Only after completing most of this procedure did some of the potential problems with this approach occur to me. Problems such as: the existence of a non-linear relationship between shape and size, finding the distribution of the test assemblage and the archaeological assemblage to be unrelated, and not having an inverse relationship between length/width and thickness. As I hope to demonstrate below, these pitfalls can be avoided.

As this was an untested theory with the potential not to work at all, it needed to be substantiated. It had worked on a small test sample of flakes, but it needed to prove itself on a larger assemblage. Wal Ambrose had provided a collection of obsidian flakes from Ahus island (see chapter five), and as these flakes were not included in the regional collection, they could serve as an independent test case. Each flake was weighed and then measured for length, width and thickness (again using *Entrer Trois* to record the results).

The resulting regression equation related theoretical volume to mass with a correlation coefficient of $r^2 = 0.98$. Figure 8.17 is a plot of the data from the Ahus assemblage—mass plotted against the product of length by width by thickness (three measurements in centimetres multiplied yields cubic centimetres which is the same as millilitres). Both the plot and the regression reveal that there is a very strong linear nature to the Ahus assemblage; almost perfectly linear in fact.

This result allayed doubts concerning the assumptions that went into devising this scheme. By this time, however, I was looking at the article by Schick (1991, see "reduction strategy" above). To incorporate these data I needed to record yet another variable for the obsidian; the size of sieve that each flake remained in after a nested sieving procedure. The most accurate way to do this would be to actually sieve the collections. Dry sieving of fragile obsidian, however, is not a practical task, especially when use-wear is being considered. Even at the best of times this procedure is likely to increase the flake count by a large proportion and ruin any information the individual flakes held.

Many geologists who investigate the behaviour of small rocks in sieves (to better relate their experimental data to real world actions) use mean diameter to predict corresponding screen size (J. Chappell pers. comm.). Using mass to calculate LWT is a poor way to determine maximum dimension of the flake, but a very good route to mean diameter. Similarly, the linear dimensions on their own are a poor predictor of mean diameter. Mass therefore is a particularly useful way to arrive at the outcome of a nested sieve procedure without either doing the sieving or making multiple measurements of each flake.

Again I wanted to know for certain how accurate my predictor would be. The Ambitle materials excavated by Ambrose were used to test the relationship. First, weight and measurements were taken for each flake. Then each was 'sieved' through a series of paper squares to determine which size mesh would trap the flake while preventing damage to the obsidians. A non-damaging screening procedure would replace the mathematical modelling approach and provide greater accuracy to the results but the process I used was completely impractical for large numbers of flakes. A regression analysis of weight versus size produced an equation almost identical to the one calculated for Ahus materials, with a similar high correlation coefficient of $r^2 = 0.97$ (see fig 8.18).

Once I had established that the procedure worked to relate size and weight, I could look at how well it predicted the outcome of the nested sieving procedure. Using the regression equation I was able to predict the size class for each flake in the Ambitle assemblage. I then ran the Ambitle material carefully through a series of hand-made paper sieves to determine the actual results I was trying to predict. In order to prevent damaging this assemblage, I hand-cut appropriately sized squares out of some stiff paper with a knife, leaving as little border (the wire of the sieve) between the holes as possible. I then processed the flakes one at a time, starting at the largest size holes and proceeding until it did not drop through, recording the hole size and continuing with the next flake.

A comparison of each specific flake reveals that this model is not perfect. Around the transition size for each sieve (i.e., the flake mean diameter that is close to the mesh size of a given sieve) there were some misclassifications. It is the case that the screening procedure itself has an element of random error in it (if you shake the screens longer, more bits go through) and that multiple runs of the sieving would have produced slightly different results for each run. The average should not vary much from run to run, but the predicting equation does not vary at all. I did not do these multiple runs because I was dealing with an archaeological assemblage for my test case and I did not want to damage it.

While the agreement was not perfect on the basis of individual pieces, overall the residual sum from miscategorisation was nearly zero. The total number of obsidians one size class up was balanced out by a near equal number incorrectly placed one size class down. A similar near-zero residual could be produced by several other erroneous processes including categorising all the pieces in the mean size class, or by miscalculating the slope and intercept of the regression equation. Such errors are easy to identify and not present in this case. This result allows me to be confident that the use of this method on archaeological materials to produce the size profile graphs discussed earlier for each assemblage is sound.

I now know that at about the same time that I was working on the above relationships amongst size, volume, mass and mean diameter some related work with very different aims was being carried out by Harold Dibble and Andrew Pelcin (Dibble 1995, 1998, Dibble and Pelcin 1995, Pelcin 1997a, 1997b, 1997c, 1998). Their research sought to determine the volume of a flake from a number of measurements (such as platform dimensions and detachment angle from the core) that could be made on a damaged or retouched piece. The implications and utility of this relationship are not relevant to my research. The discovery of a solid linear relationship between shape and volume of glass flakes (they used man-made glass quite similar to obsidian in fracture mechanics) reinforces my work in this area.

SUMMARY

Several exciting facets of archaeological research were covered in this chapter: functional analysis and related topics, the chemical weathering of glasses, and size analysis of the assemblages. The history of use-wear in archaeology indicates that the best results are produced by systematic approaches refined by blind testing.

The development of my own procedure was concurrent with the blind test. Not strictly the ideal sequence, but the good results suggest an acceptable alternative.

Application of the method to the archaeological pieces proved a frustrating and time consuming exercise. Just less than one per cent of the total collection was assigned a function; too few to make inferences about behaviour with. The graver tools of the Reef/Santa Cruz group proved somewhat enigmatic as a group. Experimentation with replica tools and examination of the archaeological ones were inconclusive but suggested that cicatrisation is the most likely purpose of the graver point. Inspection of the chert pieces, also from the Reef/Santa Cruz group, revealed even more environmental degradation of the stone surface than that observed on the obsidian. No new information was derived from the chert pieces.

Use-wear procedures are based on the appearance of the stone flake. Photography of various sorts is therefore an integral part of the researcher's activity. Several different methods of obtaining an image were presented, with those made in the low vacuum SEM chamber offering the best resolution. The difficulties of working in various digital and analogue media were also considered.

That the use-wear study was less successful than hoped for was due to the weathering of the obsidian pieces. This led to an investigation of the causes of weathering. The valuable information produced by hydration dating relies on part of the same chemical process that begins whenever a fresh surface of obsidian is exposed by fracture. Surface dissolution and the development of a hydration rind are closely related reactions whose progress is controlled by the same factors. It is inevitable that in locations, such as Melanesia, where the hydration layer forms quickly the expectation is to find a portion of the flakes suffering from the effects of surface erosion.

The final method presented concerned the characterisation of flake shape in the assemblages. The profiles used in chapter six to group sites by reduction sequence really require that the nested sieve procedure be used as a sorting method. This is a destructive process for obsidian. The alternative was to sort the obsidians by some other method. Using mass and arithmetic proved to be possible. The metrical study presented here explains why this worked.

Table 8.1.
Summary of the functional examination. The 'n' values are drawn from all time periods of a single site. 'Useable' is based on morphology only.

Site	n	% Useable hafted	% Useable hand-held
SAC	2919	42	20
SAD	527	48	25
SDI	41	85	59
ELS	86	44	7
ELT	56	25	16
EAB1	81	22	8
DES	9	100	89
DFF	27	11	4
DFV	59	11	0
DGD2	41	44	22
DIZ	17	100	94
DGW	4	29	15
DAF	330	89	69
RF2	619	77	50
RF6	16	54	18
SZ8	294	84	57

Figure 8.1.
Example of a completed initial use-wear inspection recording form.

	Site name: Balof Cave		
#	**Ventral observations**	**Dorsal observations**	**Used?**
>9300	some soil present - tiny chip on blank edge	covered in sediment	N
>9301	white 'furry' crystalline structures		N
>9302		brilliant blue residue	N
>9303			N
>9305	many fractures concentrated on one edge	same	Y
>9307			N
>9308	surface covered in tiny pits	both sides	N
>9309	surface eroded - rounded edges - almost water rolled	flaky deposits on both sides, more on this one	N
>9310	residues possibly from use - one corner used		Y
>9311			N
>9312	white crystalline structures like frost	no concentrations of edge damage	N
>9314			N
>9316	small plant remains; they grew here	apparent use on one edge	Y
>2373	consistent flaking on one edge	same edge	Y
>2374	surface in good shape - one edge damaged	same	Y
>2376	myriad snap fractures	same	Y
>2377			N
>2378	edges intact - one side damaged		Y
>2379	some fractures present - bad physical shape to use	not consistent with ventral fractures	N
>			
>			
>			
>			

Figure 8.2.

Example of a completed final stage use-wear observation recording sheet.

Specimen # :	345-3	Site name:	Balof 1 (E6-3)
Source:	Talasea		
Known treatments:	None		
Photos taken:	Roll 2-26/27/28		

Tool type:	flake	core	other:
Edge angle:	<30°	30°-60°	>60°
Platform prep:	plain	prepared	absent
Retouch:	present	absent	
Retouch comments:			

Macro Evidence

Ventral	Dorsal
	Edge angle 50 degrees Edge has many tiny but visible steps on it Scraper mode !?

Micro Evidence

Fracture #	absent/<5/>5
Fracture type	flake/step/snap/fl&st/fl&sn/other
Fracture size	small/medium/large/marco
Edge rounding	absent/light/heavy
Striations	absent/parallel/perpendicular/oblique/random
Invasiveness	absent/edge only/≤1/2D/≥1/2D
Distribution	absent/intermittent/continuous/asymmetric/away from edge
Polish	absent/light/heavy

On back side of page, place notes and observations.
Include residues: colour, structure, distribution, arrangement, and invasiveness.

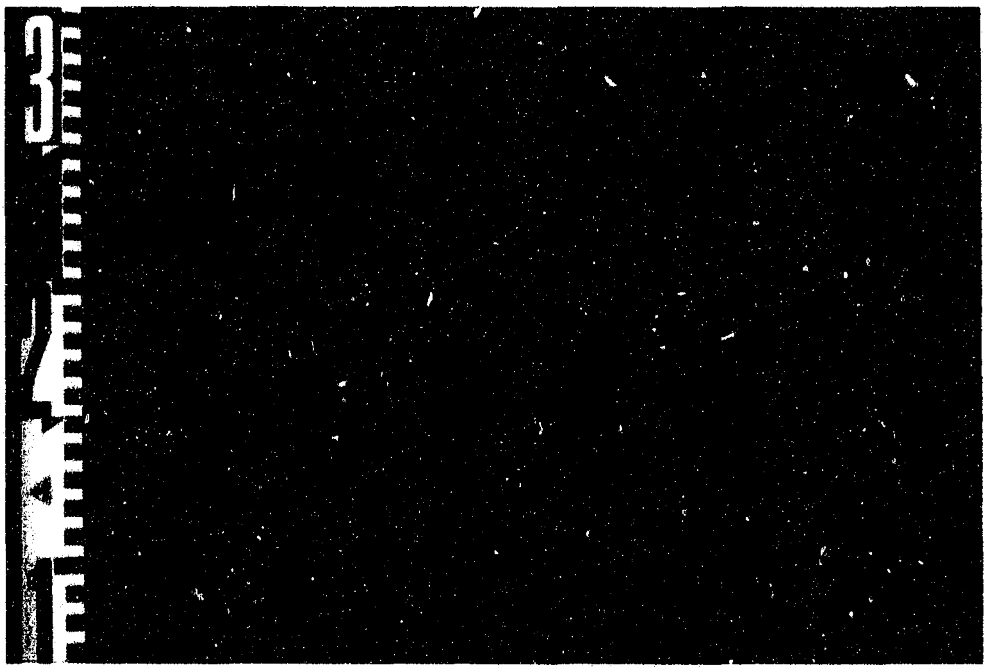
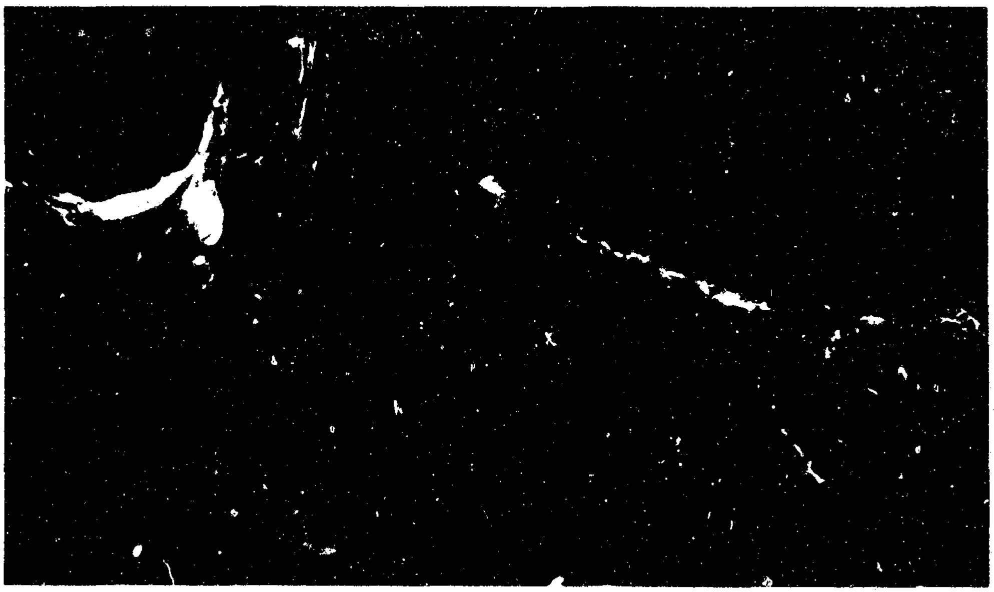


Figure 8.3.
Experimental obsidian tools produced to be similar in size and shape to the archaeological flakes.



1 mm

Figure 8.4.
The opposed retouch that characterizes the graver tool style is clearly visible in this dark field illumination micrograph of a graver (#775).

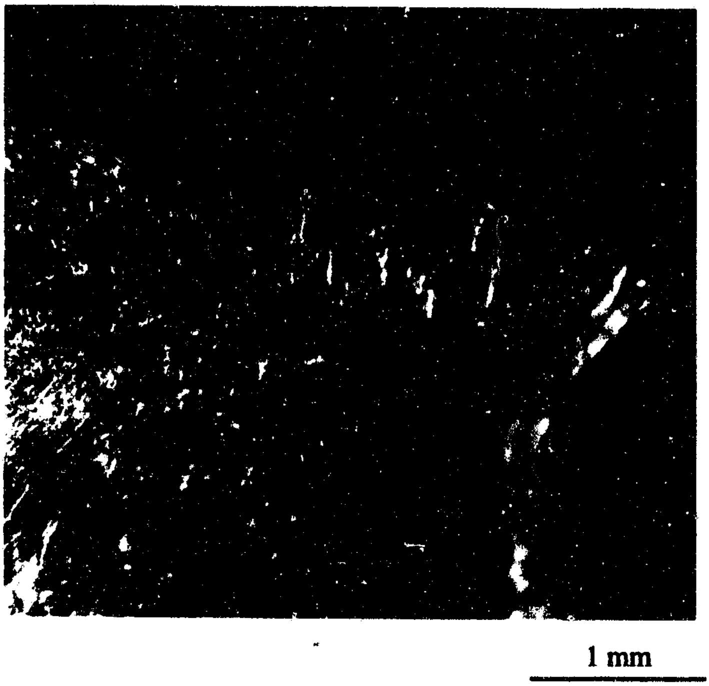
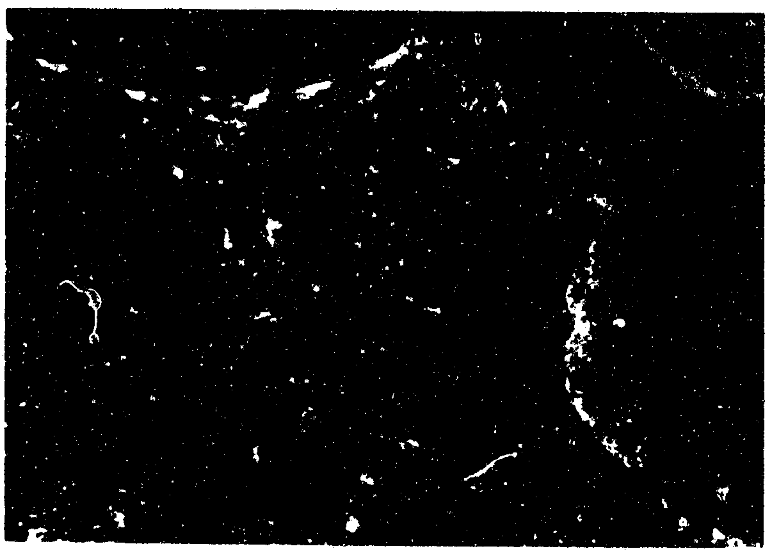


Figure 8.5.
Point #1 of a twin-point graver tool (#797) as seen under dark field illumination.



1 mm

Figure 8.6.
Point #2 of a twin-point graver tool (#797) as seen under dark field illumination.

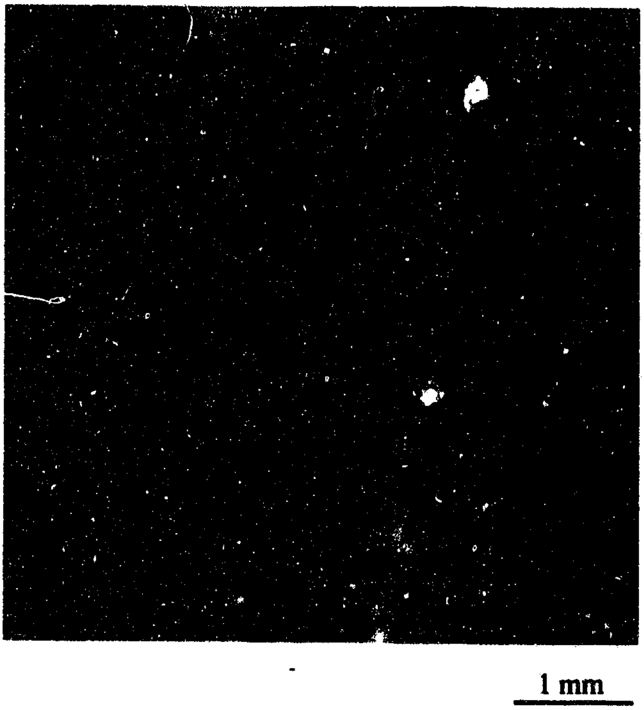
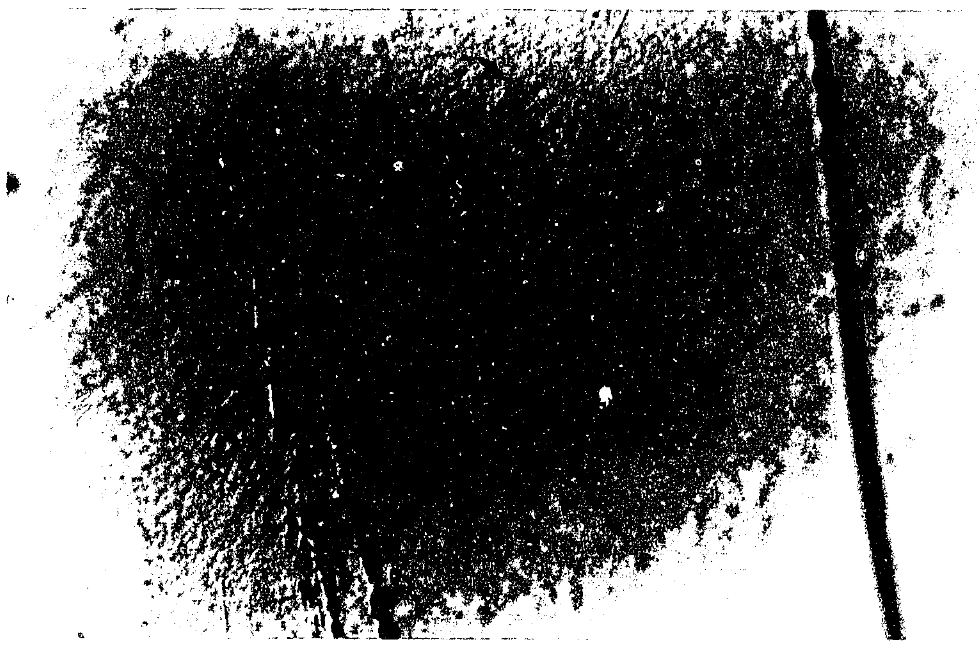
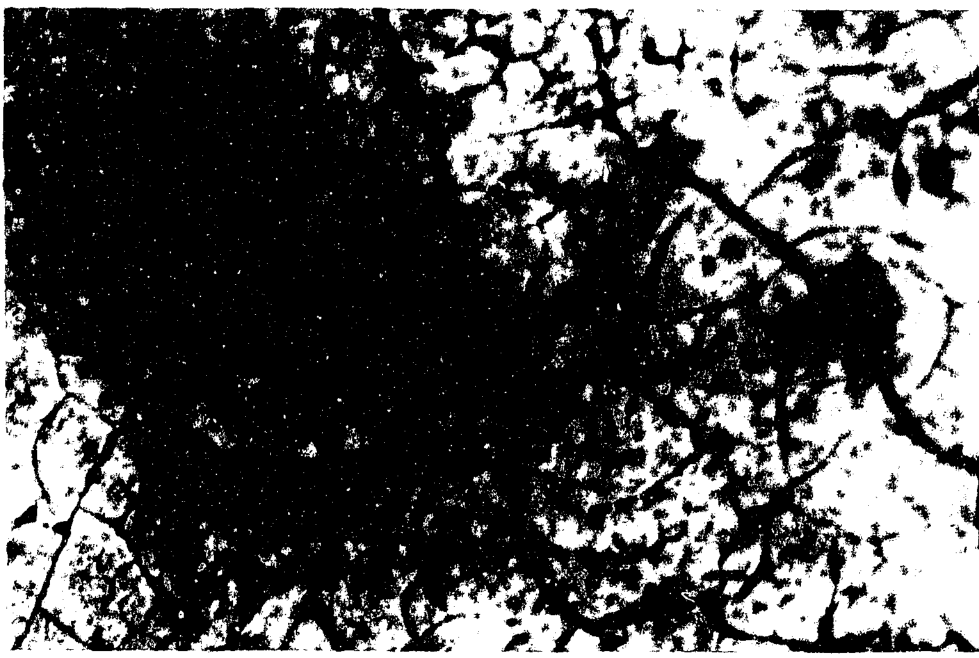


Figure 8.7.
Tiny flake scars cover the ventral edge of this graver tool (#775) as viewed under bright field illumination.



1 mm

Figure 8.8.
Linear erosion marks clearly visible under bright field illumination on this graver tool (#775).



1 mm

Figure 8.9.
This view of the surface of a chert flake (#1000) reveals the extent of surface damage due to the weathering process.

Figure 8.10.
Scanning electron micrograph of the edge of a graver tool. Small pits caused by erosion can be seen on and near the edge.

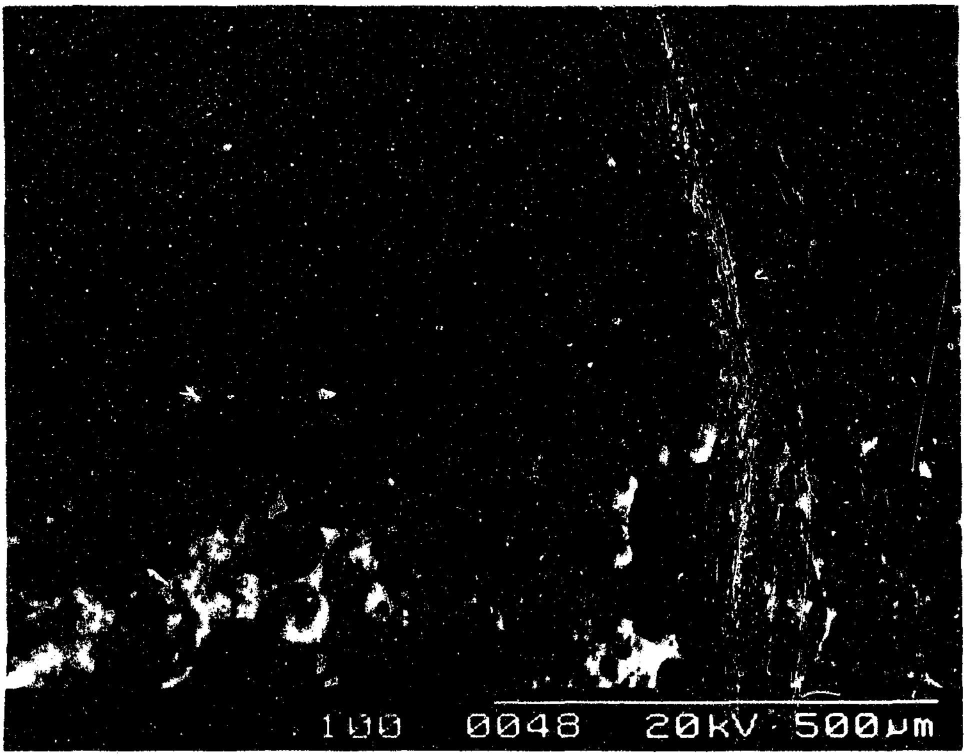


Figure 8.11.
Scanning electron micrograph displaying typical erosional pits on the surface of an obsidian flake.

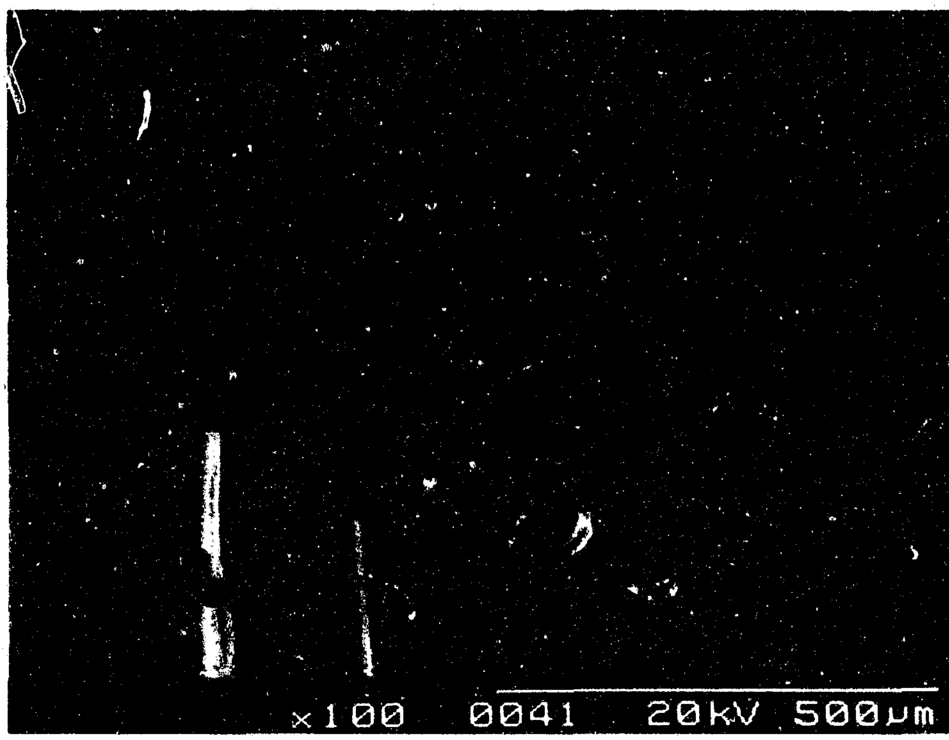


Figure 8.12.
Scanning electron micrograph displaying typical arc-shaped erosion marks on the surface of an obsidian flake.

Figure 8.13.
Scanning electron micrograph inside a linear erosion mark (see fig 8.8) reveals traits of all forms of surface erosion.

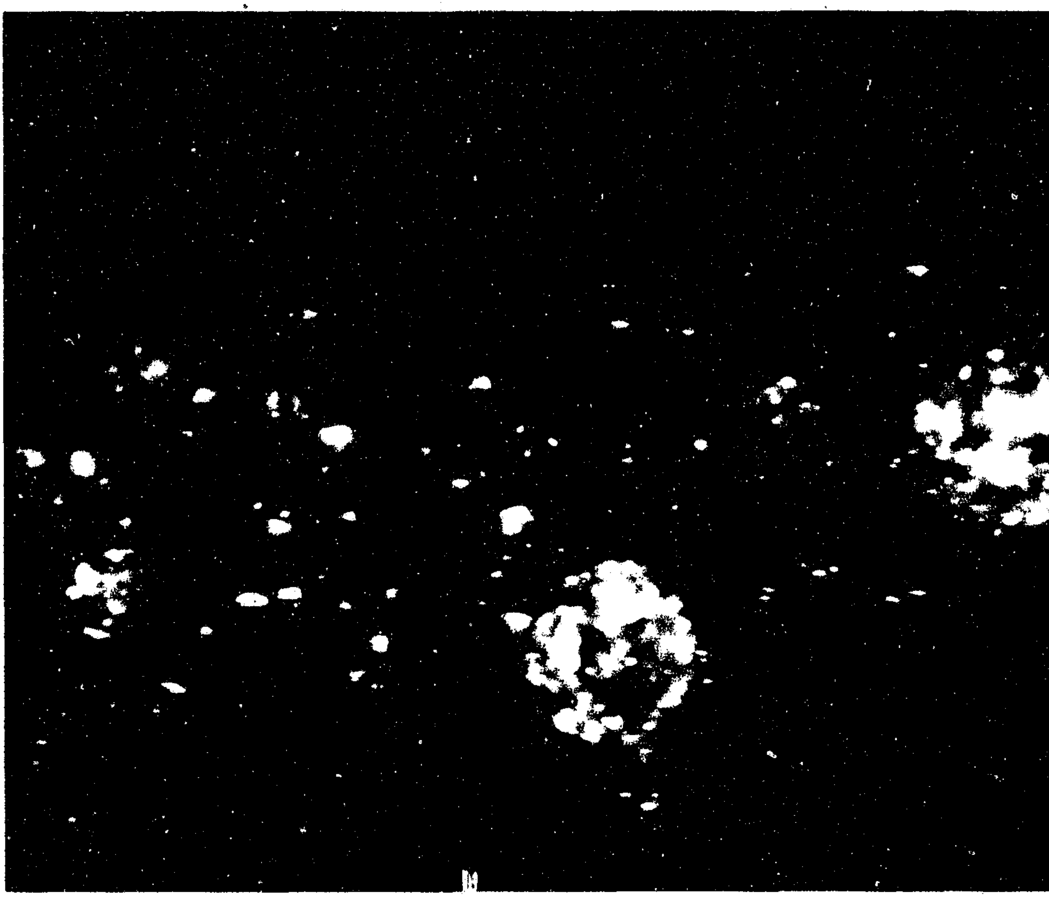
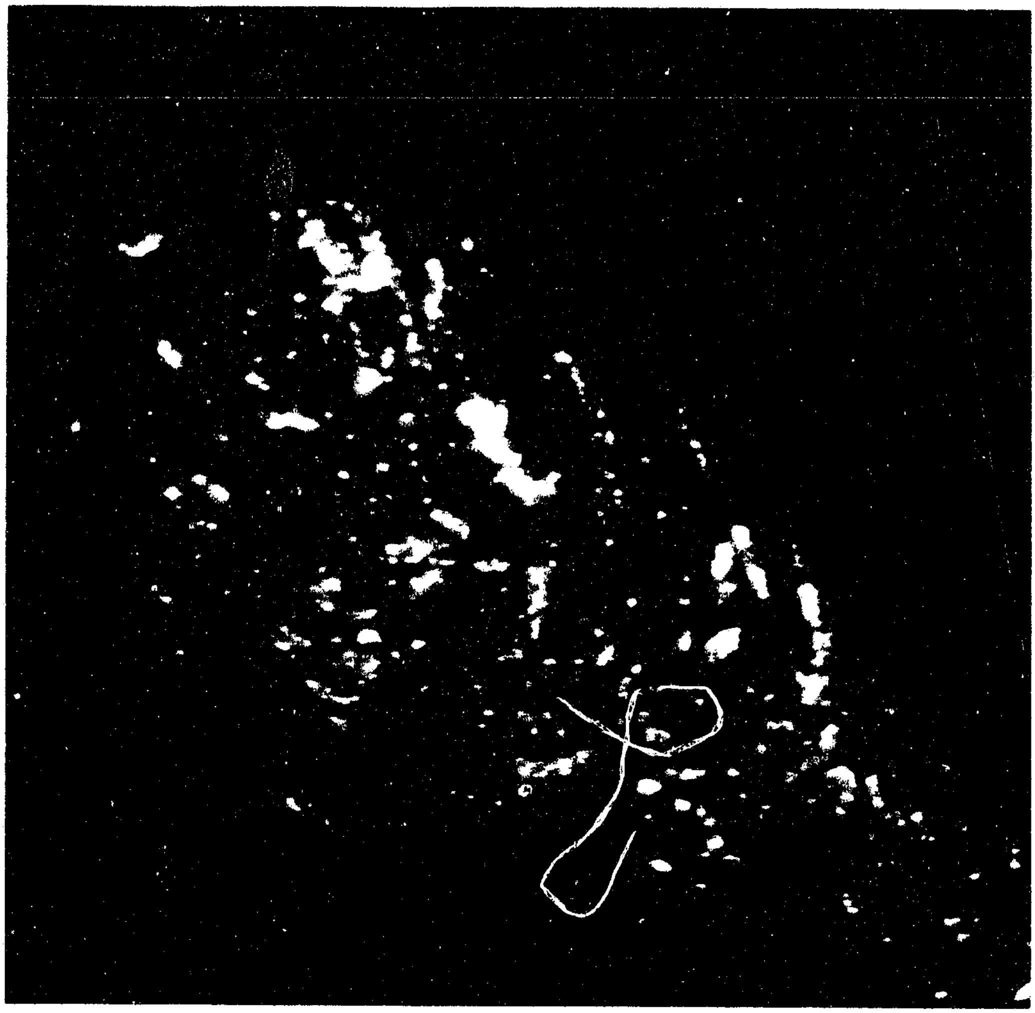


Figure 8.14.
Confocal laser scanning micrograph displaying typical erosional pits on the surface of an obsidian flake.



20 µm

Figure 8.15.
Confocal laser scanning micrograph displaying typical arc-shaped erosion marks on the surface of an obsidian flake.

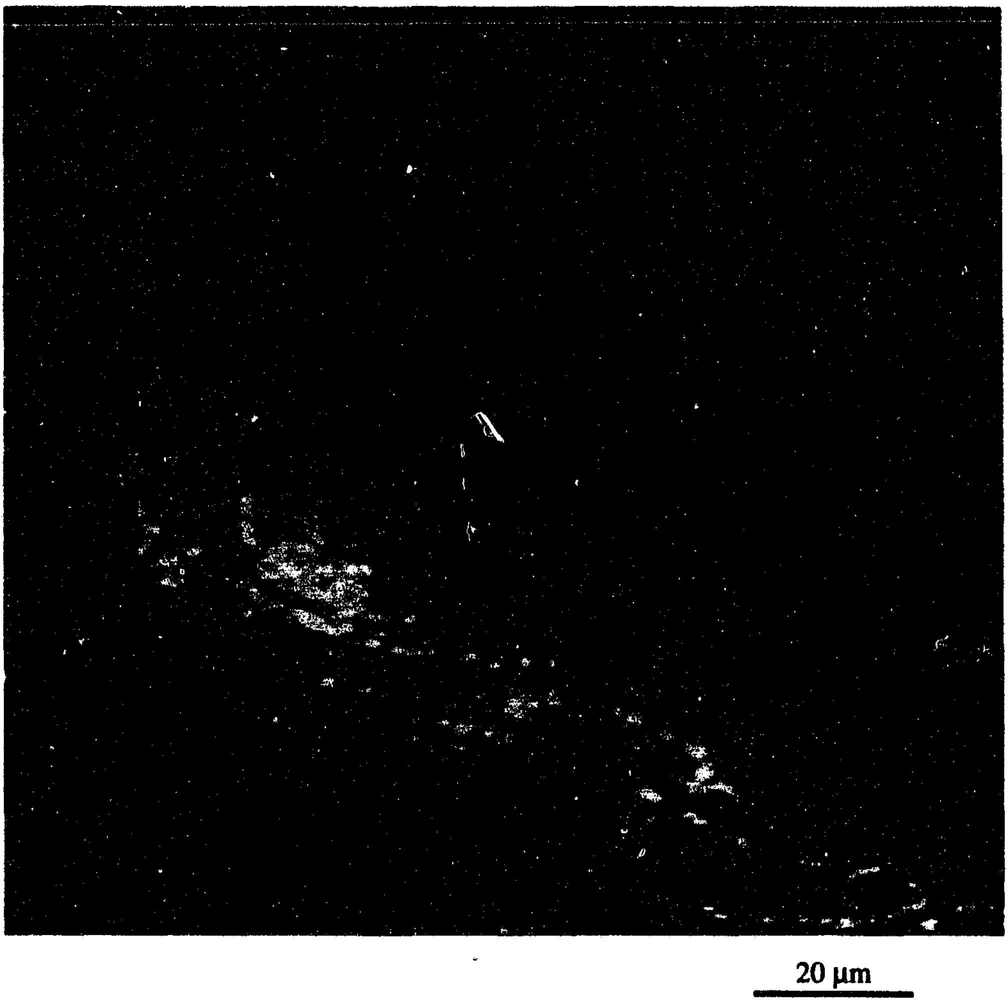


Figure 8.16.
Confocal laser scanning micrograph displaying typical arc-shaped erosion marks on the surface of an obsidian flake. This micrograph is the same as that in figure 8.15 with the imaging computer set to a different mode of presentation.

Figure 8.17.
Flake mass plotted against flake volume with regression equation superimposed - Ahus flakes.

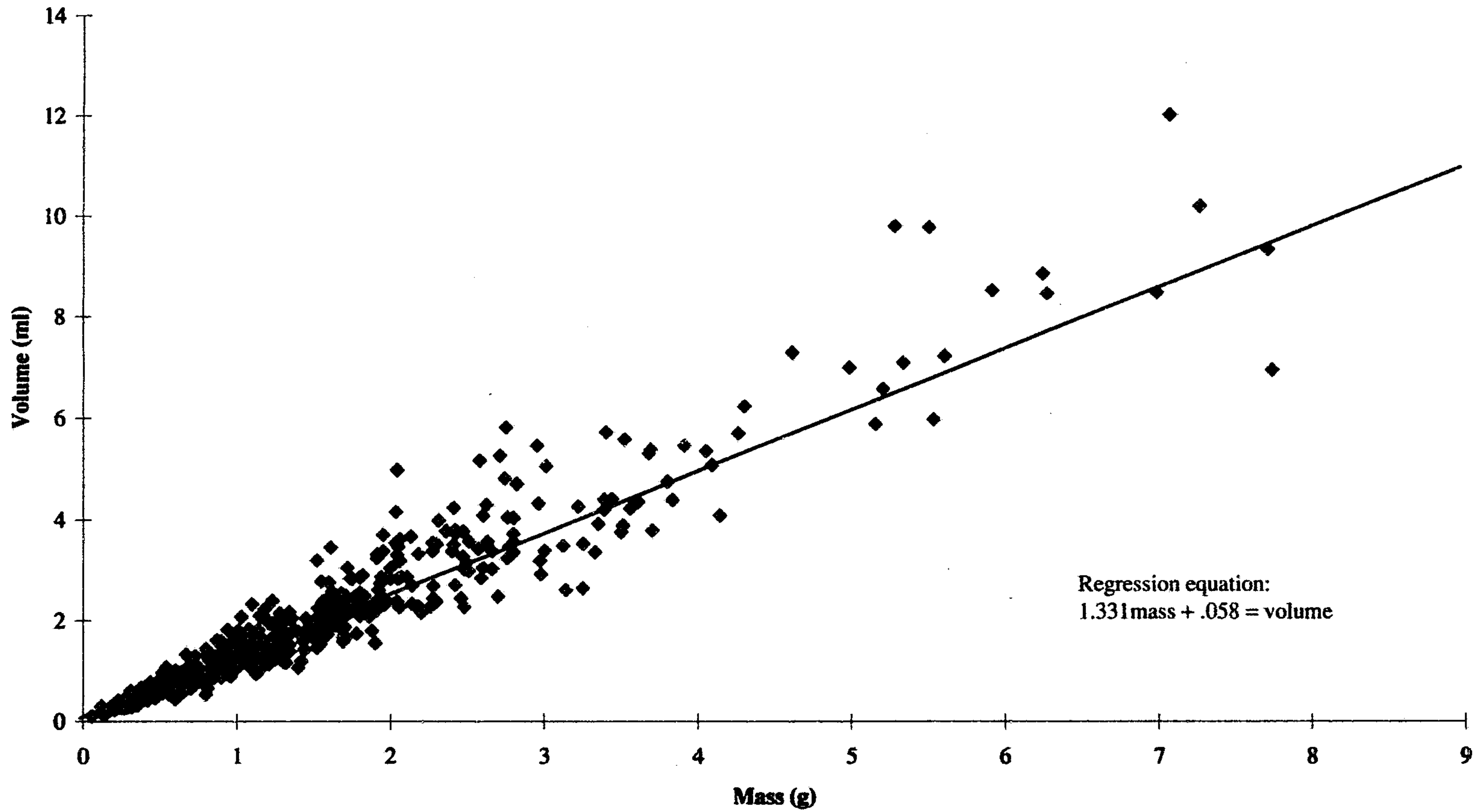
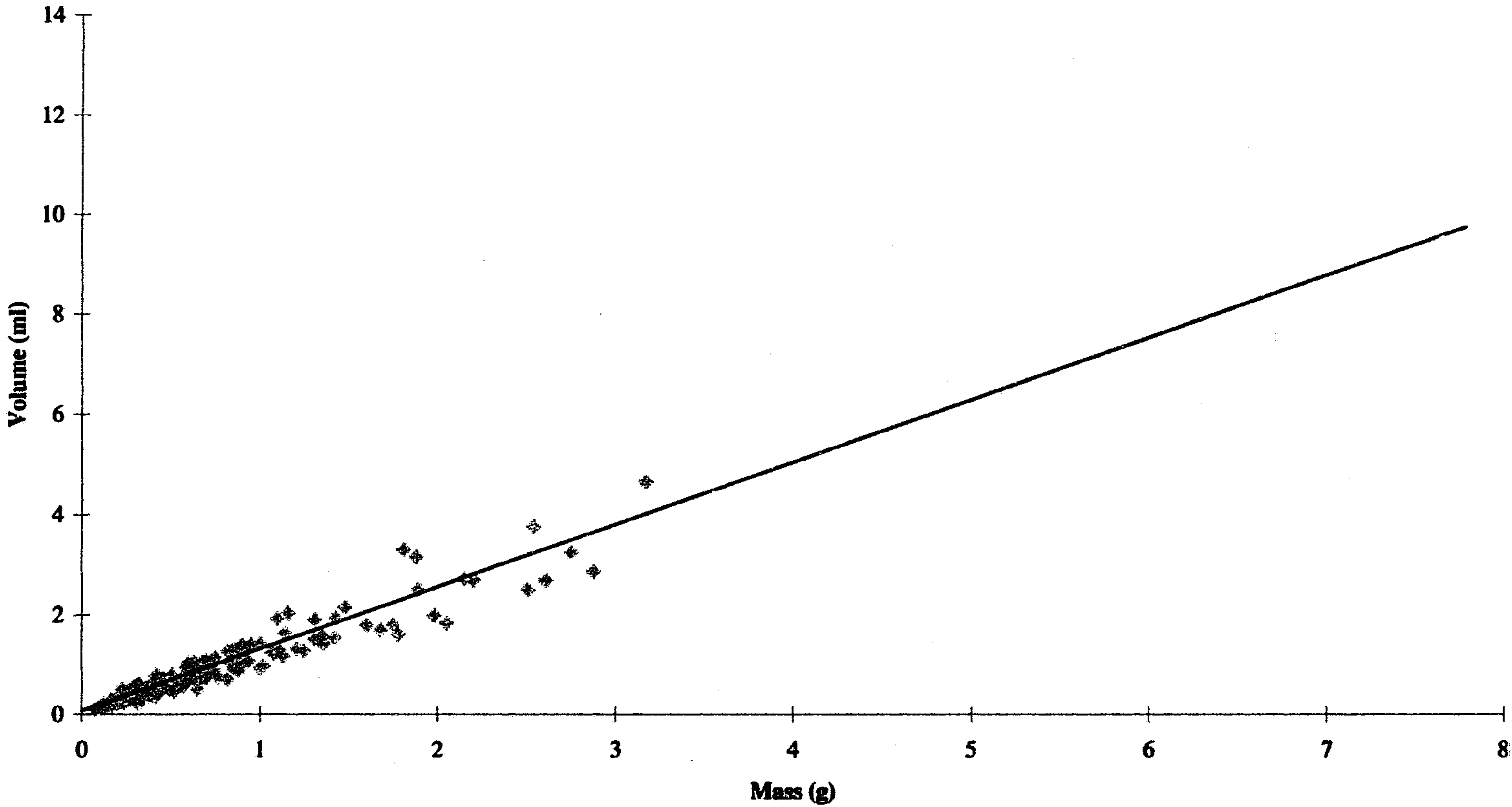


Figure 8.18.
Flake mass plotted against flake volume for Ambitle flakes with Ahus regression equation superimposed.



§9

Discussion of results

Where the preceding three chapters covered the methods and their ensuing results, this chapter discusses these results. The order in which topics are covered is the same as for the discussion of the methods. The discussion of sourcing is first, followed by obsidian abundance, reduction strategy (with bipolar reduction a separate consideration), the functional analysis, the photography, the glass erosion problem, and finally the metrical study.

SOURCING

Chapter three introduced the potential held by exchange studies that integrate source attribution data. While a contribution to the sum total of sourced obsidians in Melanesia was achieved (see Appendix), in the end not as much use was made of sourcing data in this research as was anticipated. No data showed up to contradict expectations regarding the exploitation of the two main source islands, but that is mostly due to the fact that a vast portion of this material was sourced prior to this project's initiation. With no real industry of reduction to examine, it is impossible to link specific modes of production to one source in preference to the other. The main goal with source data was to examine functional attribution in terms of source—but the lack of use-wear data prevents any meaningful interpretation of this. Sourcing data did allow further investigations under the topic "Reduction strategy" below.

The Admiralties source obsidian functions as a date marker. With no Admiralties obsidian outside that island group prior to Lapita times the discovery of this source in a site dictates a date after 3500 bp, but unlike Lapita pottery it does not necessarily indicate a Lapita site. Mopir obsidian is absent from the record through virtually the whole of the Lapita era and would function almost as a negative marker of age. That is, Mopir obsidian is highly unlikely to be recovered from deposits derived from c. 3500 to 2700 years ago. Mopir is an alternate source to Talasea obsidian in the New Britain area, both before and after period two. It is not a competing source to either Admiralties obsidian or to Talasea obsidian in the more remote islands. Mopir obsidian did not get spread around Island Melanesia in quantity like the other two sources.

Each of the four described methods of sourcing has its own advantages and disadvantages of cost, convenience, accuracy and destructive nature. From the perspective of use-wear and residue analysis of flaked tools, the electron microprobe method is a clear loser. Requiring a polished section mounted on a metal plate clearly damages the tool with regards to future functional analyses. The relative density method and the PIXE-PIGME method score a tie in this regard. The salt solution used to float the obsidian requires most residues to be actively removed, and the solvent action of a salt water solution is liable to dissolve many of the remaining ones. It should not harm the flakes from a use-wear standpoint however. Similarly, PIXE-PIGME requires a clean surface to bombard with high energy protons, removing residues that might be diagnostic of tool use, but leaving use-wear traces intact.

The method of choice (strictly from the use-wear viewpoint), for the limited aim of distinguishing between the Admiralties and New Britain sources, should be the absolute density method. The heavy liquid solution is a poor solvent that should leave most residues intact. True it does require visible residues such as soil or plant matter to be removed to ensure the accuracy of the density reading, but it is the least destructive method towards residues. It should also be the preferred method for its low cost, ease of use, and proven accuracy at the major-source level of identification.

Measures of density only function in the limited realm of separating Admiralty Islands, Talasea, and Banks Islands obsidian, however. If a wider scope summation of sourcing procedures is to be made, PIXE-PIGME is the method of choice for its excellent discrimination and minimisation of damage despite the relatively higher cost and limited access.

ABUNDANCE

When an object of restricted availability is transported across a wide region, as is the case with obsidian, its abundance in a site is often found to decay with distance from the source in a predictable manner. Exactly how this fall-off in abundance of the object occurs is dependent upon resource scarcity, ease of transport and cultural factors. In the case of obsidian this decay might be observed in terms of sheer numbers of flakes found in a cubic metre of excavation, by grams of obsidian per cubic metre, or perhaps in flake size.

The sites of this region display fall-off (fig 7.14) with distance in terms of flake numbers per cubic metre of excavation. The graph of obsidian mass recovered per unit excavation is of very similar shape to the flake numbers plot, but the most distant site shows an increase over some of the closer sites. Both graphs have only four data points, and sampling error could easily account for the resultant curve shape. Flake size remains roughly constant across the region. The curve shape is not one which might be predicted for down-the-line distribution; perhaps some other mechanism is involved?

It is possible that rather than a fall-off 'curve', it is a discrete distribution (see fig 9.1). By this I mean that very close to the source one would expect to find a very high abundance of obsidian. At sites close to, but not right at, the source the abundance values would be expected to be quite a lot less than those at the source. At some critical distance further away from the source, the abundance value would drop further. We do know that at the extreme end of the Lapita range, in Fiji and New Caledonia, recovery of obsidian is fewer than ten flakes per site. Each step in this discrete distribution would produce, for example, roughly a tenfold decrease in abundance. Without more data it is impossible to eliminate the many possible models of obsidian distribution mechanisms that would reveal one that accurately describes reality.

R. Torrence (pers. comm.) provided me with a few data points from sites on Garua island for comparison purposes. Garua is a very small island within sight of most of the obsidian sources at Talasea and Mopir, approximately four kilometres from the important Kutau/Bao source, and has one good quality obsidian source itself.[5] The abundance data for sites FAP, FAO and FRL reveal that at the source obsidian is found at least an order of magnitude more densely than at Watom. Site SAC on Watom is the extreme high outlier in my data at seventy-five grams of obsidian per cubic metre of excavation. There is, however, a great variation amongst the three sites as well. The lowest values from a Garua site come from FAO in the Lapita to post-Lapita periods with 550 grams of obsidian per cubic metre. The highest value is for the pre-Lapita site of FAP with more than 12000 grams of obsidian per cubic metre recovered. Regardless, this evidence does lend support to the discrete model.

What cultural activity would account for a discrete distribution? Sites near the source have no "cost of access" and therefore can have a great amount of obsidian deposited in the site. Once you move more than a few tens of kilometres away from the source, there is an energy cost involved in acquiring the obsidian, however small that might be. With water borne acquisition, once a group goes to the trouble of sailing to the source to

[5] According to Torrence's research (Torrence and Victor 1995), the vast majority of 'Talasea' obsidian that finds its way outside the immediate Talasea region is in fact from a single volcanic source known as Kutau/Bao. Within the Talasea area, there are tens of volcanoes present and many of them produce excellent quality obsidian, including one on Garua island itself.

obtain obsidian, there is a range of distances over which the cost is very similar and therefore the observed abundance in these sites will be similar.

I propose that the first 'step' in this distribution would equal the distance over which a small group would be willing to go regularly (perhaps monthly or quarterly) in person to gather the stone. This is shown as fifty to 250 kilometres in figure 9.1 but these numbers are only to demonstrate the concept. In my experience, sailing fifty kilometres is a more 'casual' undertaking than sailing five times that distance. With reasonable sailing technology, an average velocity of five knots is not unrealistic (nearly ten kilometres per hour) and would cover that 250 km distance in about one twenty-four hour day. Not necessarily a time investment that precludes regular repeat voyages. Travel from further away than this and obsidian acquisition becomes yet more expensive. Either these people have to exchange for it with people who are closer, or they have to mount periodic long distance trips to obtain it. Whichever, the end result is another drop in obsidian abundance in the sites for a range of distances from the source. This chain of events continues across to the farthest eastern end of obsidian distribution in Fiji. Here the cost of acquisition is incredibly high, and the resulting abundance measure is in milligrams of stone per site.

The exact number of steps in this proposed distribution scheme remains a question, but approximately eight fit in with the data available. It must be mentioned that if one were to plot the available abundance data on figure 9.1, there is a discrepancy for the Reef/Santa Cruz group. Garua, Watom, Nissan, New Ireland and Fiji Islands' data all lay on the plotted line. Green (1987) suggests that the import of quantities of obsidian from New Britain to the Reef/Santa Cruz Islands maintains cultural links with the islands to the west. This may well be the case, and explain why Banks Islands obsidian is not exploited in greater quantities, but it does not address why this should be different for the Reef/Santa Cruz peoples compared to the others.

The fall-off curve of the archaeological data (fig 7.14) with its major decrease in the low values and a plateau-like levelling off with greater distance is something like a high order inverse power curve. A formula that would take a form such as: $1/x^n + c$ where x is the distance from the obsidian source, n is a number of approximate value ten, and c is a constant value. As the value of n increases, the resulting plot looks more and more like a right angle drawn parallel to the axes of the graph. At a value of between ten and fifteen the plot is most like the observed fall-off.

On reviewing this and comparing it with Renfrew's theories regarding fall-off (Renfrew 1984: 138), it occurred to me that my proposed formula above is essentially the same as his exponential decay model which he presented in alternate form: $I = mx^{-k}$. The difference is that I include a constant and have no multiplying factor. To create figure 9.2, I needed to include both the constant and the multiplier: $y = mx^{-n} + c$. For each exponent n I calculated a multiplier m and a constant c such that the end points of the plot were the same as the archaeological data plotted in that graph. The multiplier is needed because of the huge influence of the different exponents. The constant is needed in the equation because without it the plot would flatten out at zero, where in fact it levels off at about six flakes per cubic metre excavated.

While much of the data plot lies between the n=10 and n=15 plots, notice that the point which corresponds to New Ireland is well away from all the exponential curves. Further evidence that these curves are completely unrelated is obtained via the log - log plot in figure 9.3.

It would be very interesting to see what the curve would look like with the inclusion of some sites closer to the sources than Watom, but inconveniently there are currently no available sites closer than Watom that are separated by at least one hundred kilometres water crossing from the source. With the Admiralties so far from their nearest island neighbours, the best location to seek such a site would be along the north coast of eastern New Britain. Sites such as those in the Arawe Islands are inappropriate in their relationship to both the Admiralties and the Talasea sources. These sites close to, but not at, the source are the ones required to best determine the shape of the fall-off curve. As Renfrew's model explains, the exponential relationship does not hold for distances of less than approximately one hundred kilometres, so the Garua data would be of no assistance here.

Sometimes outlying data points, whether they represent valid or erroneous measurements, can overwhelm or obscure a trend present in the body of the data set (Hartwig and Dearing 1979: 27-9). It is quite interesting to eliminate the outliers in this data to determine what relationship exists among the remainder of the sites. Taking SAC abundance figures out of the Watom mean abundance value, and the ELT abundance out of the New Ireland mean produces an almost straight line (correlation coefficient is greater than 0.98) from Watom out to the Reef/Santa Cruz group (fig 9.4). This contradicts both the discrete theory and the exponential fall-off theory. This fall-off, however, is linear and will not accommodate the furthest regions of obsidian distribution (i.e., Fiji).

Again, without more data it is impossible to be certain, but if figure 9.4 is an accurate representation of abundance over distances from two hundred to two thousand kilometres, then it is likely that a complex, multi-modal explanation is required. One that allows for extremely high abundance close to the sources, a gradual linear fall-off over some two thousand kilometres, and then a marked paucity at the extreme range of distribution. The implication of a linear (rather than exponential) decay model is that the cost of acquisition has little effect on the presence of obsidian in a given settlement over a broad expanse of ocean.

The final method of transformation that I consider is able to produce a linear regression model without excluding any sites. Using the abundance data displayed in figure 7.6 (numerical abundance), the reciprocal of distance ($1/distance$) plotted against the reciprocal of the square of the abundance ($1/abundance^2$) produces figure 9.5. It is not necessarily obvious what this graph displays. Where the plotted line intercepts the Y axis represents an infinite distance from the sources and an abundance figure of approximately 6.5 flakes per cubic metre. On the opposite end, where it intercepts the X axis sits an infinite abundance at a distance of approximately three hundred and fifty kilometres from the sources. This regression only applies to distances from 370 up to about three thousand kilometres. No data are available from other excavations to check this model because the measure of numbers of flakes per cubic metre excavated is not one usually generated. With only four graph points to rely upon, little emphasis can be placed on this model.

However well the discrete distribution theory is supported by the evidence, a fall-off curve model is better supported. There is an historical precedent for such behaviour in other settings, a logical explanation for why it occurs, and only the presence of what are possibly outlier data in a small data set prevents secure conclusions. Site SAC is at least partly redeposited and may be affected by obsidian enrichment through the redeposition process. Site ELT is an obsidian-rich test-pit that may grossly over represent the abundance of obsidian in that site. Even if I discard both of these sites, note that New Ireland does not sit particularly close to the regression line in figure 9.4. New Ireland is the problematic island in the exponential decay models presented in figures 9.2 and 9.3 also.

Abundance varies quite a lot through time both between and among sites. I find it quite unexpected that the distribution should be so symmetrical and that the peak should fall in period three. The first two and last two time periods have abundance measures of roughly the same value. Only in the middle, period three, does the value increase dramatically, by more than double. This is true whether one refers to abundance in grams or numbers of flakes (figs 7.4 & 7.8).

Despite the similarity between these two graphs, there is a lot more variation visible in the abundance measure based on mass (fig 7.3) than there is in the abundance measure based on counts (fig 7.7). It is obvious that the mean is drawn upwards by the presence of two sites in period three. These are the same two sites that I removed as outliers above to create figure 9.4. Experimentally, I removed ELT and SAC from the data again, producing two new figures (9.6 & 9.7). With the abundance measure based on mass, the resulting plot (fig 9.6) retains its symmetry with more of a plateau across periods two to four, rather than a peak in period three. I see both figures 7.4 and 9.6 reflecting the increased long-distance mobility in Melanesia that accompanies the arrival of the Lapita culture in period two. This increase in transport technology does not change much over a long period of time. The drop of abundance in period five may result from the discontinuation of long distance social contact that accompanied the regionalisation of Melanesia post-Lapita and the development of the ethnographically known trading schemes (Ambrose 1978, Macintyre and Allen 1990).

Turning to figure 9.7, the abundance measure based on numbers, the removal of the two outlier sites results in a radical reshaping of the plot. A gradual build-up of obsidian exploitation intensity is evident through time, with a sharp drop-off in recent times. This could be interpreted as casting a different emphasis on the importance of the Lapita culture in obsidian transport. It is quite unlike figure 7.8 which places period three at the height of obsidian utilisation and the other time periods at roughly half this level. As with so many of the analyses in this thesis, until more data is available no clear conclusion can be drawn.

Given that the arrival of Lapita culture in Melanesia coincides with the initial transport of Admiralties obsidian beyond the source archipelago, that some sort of exchange network necessarily existed to move this obsidian over thousands of kilometres of open ocean, and that these long-range links between islands disappear more or less slowly after the Lapita culture ends it seems logical to conclude that abundance should peak in mid- to late- Lapita times. This proves true by mass and counts, with the exception noted.

Flake size forms the third side of the triangle in the relationship of mass, counts and size. If counts go up while mass remains constant, size necessarily decreases. It does not work out quite so simply however, as sample size is quite different between periods. As noted in chapter seven, there is an odd variation in flake size across the region. The furthest out island group displays the largest flakes while the three closer islands show a non-linear fall-off in size.

How can this variation be accounted for? Other research reveals that resource scarcity typically results in smaller end products in a chipped stone industry. Newman's work in the United States (Newman 1994), as an example, found direct linear correlation between distance from source and size of flakes. This is where assistance comes from the other procedures which I conducted—reduction strategy in particular.

The result of knapping at a site is a lot of small flakes of obsidian; therefore the profiles for the Reef/Santa Cruz sites do not indicate on-site reduction. Importation of completed flakes would explain both behaviours at once. The large average flake size is a result not of having so many large flakes but of lacking the small pieces of debitage, while the profile shape of these sites more than likely corresponds to one of final use. This line of reasoning also works for the sites near the obsidian source. Extremely high obsidian abundance figures are noted because so much debitage was created, but the mean flake size need not necessarily be lower due to the presence of large cores.

The final characteristic of abundance remaining to be discussed is the flake size variation across time. While there is little variation exhibited in the time periods there is a small peak in period two. This consistency in flake size through several thousand years I attribute directly to the lack of formalised industry in the area; that is, to a paradigm of expedient technology. What variation there is between time periods can be attributed to the nature of the sample and to randomness. The nature of the "sampling error" inherent in the material is exemplified by the fact that the Reef/Santa Cruz material is of larger than expected size and falls in periods two and three, thereby increasing the mean for those two periods. The two sites from period one contain only flakes (few in total number) that are of small size. A different selection of sites would rearrange the distribution of larger flakes over time.

REDUCTION STRATEGY

Expedient technology accurately describes most of the flake production throughout the history of Island Melanesia, but not all of it. From the earliest stemmed tools of New Britain through the graver tools of the Reef/Santa Cruz group to the historically produced triangular section spear points of Manus, there are many cases of lithic industries with a specific product. They are the exception however, and even then tend to occur near the sources rather than away from them. Despite the rarity of obsidian and its potential to yield both high quantity and high quality sharp edges, little care was taken in producing useable edges. Even in those locations (and time periods) where planned formal tool production existed, the majority of obsidian recovered was in the form of casually produced flakes.

I return to the theory proposed by White (1997) that there was more profligate use of the more readily accessible source material in comparison with the rarer sources at any given site, as first mentioned in chapter six. Combine the facts that one-third of the examined sites contradict this trend with the conclusion that casual reduction processes prevailed everywhere, and it seems most unlikely that there are real behaviours involved. It was quite rare to find a core in most of these sites. In fact, large pieces of any variety are notable exceptions to the norm. An alternative to White's theory is that the chance of finding a large piece of obsidian in a site is directly related to the number of pieces recovered. In sites with very few flakes, just one larger piece would lift the average weight.

Examining the size distribution characteristics of the assemblages in chapter seven (reduction strategy section) I observed that the larger pieces were distributed by a Poisson function. They appear much more unpredictably than if they were normally distributed like the smaller pieces. Hiscock (in press) discusses the importance of randomly appearing tool types in small Australian assemblages, with the relevance to this topic being that archaeologists have to accommodate the usual absence (and rare presence) of these pieces without using them in a diagnostic manner. With small total numbers and high standard deviations (in many cases larger than the average itself) the mean weight numbers in table 6.5 are often the result of, and highly susceptible to, presence or absence of just a single large flake. It is virtually impossible to control for these effects with such small sample sizes, and comparatively few assemblages.

The use of flake size distribution profiles for comparing different sites proved to be a simple procedure with great efficacy that was immune to the effects of the larger flakes appearing randomly. Sites with similar (or different) profiles were obvious. The largest grouping of sites consisted of those with a very rapid decrease to zero observed and are listed in table 9.1 as group #1. Another grouping consisted of those sites with a peak of size class two centimetres and a rapid decrease to zero observed after that. These four sites are group #2 in table 9.1. Three more sites group together with a similar profile to the previous one, but it has two peaks, one at size class two and the other at size class four centimetres. These are group #3 in the table. Finally there are the few sites for which the profiles were unique. Sites SAD-4 and SAC-3 fall in between the above profile groupings and are unlike each other. Site EAB1-1, as noted in the previous chapter, is the only site with a profile like the expected curve.

Those sites grouped together on the basis of the profile can be said to have shared a reduction strategy at the time of deposition. With the exception of site EAB1-1, it is impossible to determine exactly what this behaviour was. It remains for someone to complete an experimental series of core reductions producing a range of different activity profiles. Only then can a label be placed on each group of sites. For particularly large sites, either those of great area or long time duration, it is quite likely that no single activity profile will ever match up completely with that of the site because it is to be expected that more than one reduction strategy would occur in a large site. Few, if any, of the sites in this study would have this problem however, as each has a restriction of some sort on the free choice of reduction strategy.

The Reef/Santa Cruz sites appear to be importing already knapped material, so the choices available to these people are greatly lessened; use (i.e., no reduction on site at all) and resharpening are the only two. For an island close to the source where resource scarcity is not an issue, of which Watom island is the best example in this group, the reuse strategies are unlikely ever to occur. On Nissan and New Ireland, all sites are physically quite small, and the time depth is well-divided into smaller portions. The profile approach should work for every site I have considered, and by careful manipulation of the provenience data it should apply to most other sites as well.

The flexibility of the profile test is shown by the use of reduced profiles. The chance of a false positive result is increased, but the chance of a false negative is reduced. For the reef scatters, this is the only opportunity to incorporate them into the scheme. An examination of the groupings on the basis of the short profiles reveals a few changes from the long profile groups, but basically the outcomes were consistent. The first grouping consists of five sites (group #4 in table 9.1). A second group has nearly the same short profile as the first, but a more even proportion distribution; listed as group #8 in table 9.1. Another group similar in profile to the first, but with more uneven proportion distribution, consists of three sites in group #7. Site RF6-3 sits alone with fifty per cent of its flakes in each of the two size classes. The last two groupings consist of those sites with more flakes in the larger size class than in the smaller class. Three sites with only slightly more in the larger class are in group #6. With a much larger proportion in the larger size class are the final four sites in group #5.

Neither of the reef-top sites group with any of the above sites, or with each other. In my opinion this is evidence that the removal of obsidian by the wave action has obscured too much information for any conclusion to be made for wave-sorted assemblages. Remember that were the 'expected' profiles for a variety of reduction strategies available, each grouping in table 9.1 could be assigned to an activity. This would make the exercise much more valuable as a tool for understanding obsidian usage across Melanesia.

The use of the Chi Squared test has already been covered in chapter seven. It is a very powerful and useful statistical procedure for the natural sciences, but it depends on a number of qualities which one can never be certain that these obsidian data possess. Further, if multiple expected curves for the different modes of lithic reduction were introduced, the resulting tables of numbers would be quite unwieldy. Interpreting the results across multiple tables would be much more difficult than using graphical means. It is the case that it provided a good corollary for the profile tests, and this very fact is indicative of the likelihood that in this particular circumstance, the assumptions of the Chi Squared test were met.

BIPOLAR FLAKING

Between the research of others (Patterson and Sollberger 1976, Jeske and Lurie 1993) and the observations made here, I think enough evidence has been presented to seriously question the existence of bipolar flaking as a universal approach across Melanesia, although not discounting the use of it altogether. Bipolar reduction is contraindicated by the pervasive use of expedient approaches to obsidian knapping. With the exception of the formal tools, there is no suggestion of any sort of 'strategy' in the regional approach to reduction. The general production of sharp edges by direct percussion seems ruled by serendipity. To expect to find bipolar flaking as a regularly used technique is in contradiction of this evidence.

FUNCTIONAL ANALYSIS

The use-wear portion of this research project presented the greatest possibilities for new information, and therefore the biggest disappointment in its outcome. The ability of the presented system of analysis to determine the use of an obsidian flake was convincingly demonstrated by the blind test undertaken. Refinements in the use-wear procedure which were developed during the analysis, such as the use of electron microscopy, promised greater speed and accuracy for the interpretations.

Several theses of the past decade (such as Grace 1989, Hurcombe 1992) have developed a functional analytic procedure and applied it to a test assemblage of tools, and this has been sufficient in itself. The time has certainly come, I believe, to take use-wear studies out of the developmental phase and integrate them into the technique of all archaeologists. This step will result in more use-wear analysis in everyday archaeology. Critically it needs to be accompanied by a paradigm shift which moves use-wear into the realm of 'prehistory' in response to the criticisms of Torrence (1989a, see p. 6 this thesis) and Fullagar (1994, see p. 29 this thesis). To this end, the creation of a new methodology was not the goal of this thesis, but a means of furthering our understanding of the people that used these flake tools. That a successful procedure exists would be of benefit to analysis of suitable assemblages in future, but has proved of minimal impact to date.

The few positive outcomes of the use-wear study suggest that obsidian edges were used not to their best advantage, but for whatever task was at hand. In light of the facts that flaked obsidian yields such incredibly sharp edges, that obsidian is such a delicate and brittle material, and that it is a relatively scarce resource away from the source; it is quite surprising to see the range of tasks to which this material was put. Obsidian was a very small component of the lithic material from many of the sites, less than one per cent in some cases. It makes wonderful cutting edges but works poorly on most other tasks, including scraping, incising and chopping. Yet it is these other tasks for which the obsidian was apparently often utilised. In the Reef/Santa Cruz group, it was used interchangeably with the chert flakes despite the great differences in their fracture mechanics and basic physical properties.

Also in the Reef/Santa Cruz group occurs the only formal tool type of the region seen in these assemblages in any quantity, and one of only a few known to exist. Produced only in obsidian (none were found, or indeed possible, in chert) the graver tool was found by experimentation to have few potential functions. The small, sharp and brittle projection on the end of the flake cannot be used for graving, cutting or drilling. Many of the points have a morphology adequate to the task of creating a scar or applying a tattoo, but perhaps surprisingly no blood was found on any of the flakes.

Despite the lack of blood found on the graver points, there is reason to believe that scarification was the most likely purpose of these tools. A survey of some Pacific Island ethnographies reveals many cultures that used obsidian tools and many that decorate the human body with tattoos and scars, but scant attention is paid to the procedures themselves in most texts (G. Summerhayes pers. comm., Cayley-Webster 1898, Radcliffe-Brown 1922, Powdermaker 1933, Mead 1950, Malinowski 1961, Lawrence and Meggitt 1965, Freeman 1970, Du Toit 1975, Tickle 1977, Brown 1978, Specht 1981, Parkinson 1999). Most tattooing implements are multiple point needles, not unlike the tools used to create the Lapita pottery decoration, and therefore unlike the graver point. The remaining task is cicatrisation and this is the most likely use to which the gravers were put. Many scars can obviously be created using only a sharp knife edge, but others would require the fine point edge of the graver tool.

That there may have been no utilitarian function for some of the graver points needs also to be considered. If, in certain settings, the tools had a cultural value independent of their utility then it would explain the lack of use-wear traces on the majority of graver points. That a few graver points have clear evidence of use does not contradict this suggestion. Odell's work (1994) in the Illinois Valley revealed that chert blades were used in a variety of tasks in most locations, but had specific functions in ceremonial centres. An analogous situation could exist in the Reef/Santa Cruz group. Unfortunately, this cannot be determined from looking at the information provided by the graver tools in this assemblage.

I have presented no discussion of the *chaîne opératoire* for these obsidian tools, to take an example of an advanced analytic method presented in chapter four. This is because we still do not know the whole life cycle of the graver tools, or the plain flakes either. In one sense, this thesis is locked into the era that many of the artefacts were excavated in due to the combination of mistreatment of the obsidian and still incomplete time-space coverage in the excavation record.

The treatment of stone artefacts in archaeology has undergone a dramatic change during the past decades, as demonstrated in chapter four. Much of this change is witnessed in these assemblages excavated over a thirty year period. The stone artefacts have gone from being an ignored item in a deposit, through a series of steps of ever more careful treatment until they became the valuable source of information that they are today. Both the increase in variety of methods and the refinement of existing methods, only some of which were discussed in chapter four, have made this possible.

The excavations in this study that were completed in the 1960s placed all the stone from a single spit into one container, usually a bag. By the 1970s, multiple bags were being used, but still more than one flake per bag was the norm. Finally by the 1980s, the potential of use-wear was realised by many researchers and at least a portion of flakes from each site were reserved specifically for functional analysis. It is the use-wear study more than any other method that has demanded this change in handling. All procedures, however, benefit from reduced damage to the assemblage.

Post-excavation treatments were also seen to vary greatly in these collections. Flakes assigned to one analytic regimen are often lost to any subsequent ones, even if the procedure is not by definition destructive. While I had access to the Tikopia flakes excavated by Kirch and Yen (1982), the fact that each flake was cut into at least two sections precluded even the simplest morphological measurements or use-wear investigations. Flakes from other sites were traced onto paper with a pen, resulting in large deposits of blue ink around the edge of each flake and tiny pen induced chips in the fragile edges. Obsidian hydration dating requires that a thin section of the flake be removed, some sourcing techniques require sectioned or ground up preparations of the obsidian, and use-wear and residue analyses are often at odds with each other in their preparation procedures.

This explicitly brings to attention the manner in which decisions taken even before excavation has begun can permanently affect the data content of an assemblage and reinforces the suggestions made in chapter four about careful research design for stone tools. Lithic specialists doing their own excavation now plan sub-sampling strategies with specific purposes in mind, and reserve still other portions for no current purpose at all. All archaeologists need to integrate this strategy into their future handling of flake tools to ensure maximum return of information. One cannot subject a one hundred per cent sample to one procedure today and expect another procedure to work tomorrow.

Table 8.1 listed the useable flakes in each assemblage. A simple extrapolation of the data to the unexcavated portion of each site permits the creation of table 9.2 which projects the density of excavated obsidian pieces into the remainder of the sites. While it is bound to be inaccurate, it does allow for preliminary assessment of the potential information remaining at each site. Fewer than ten flakes are likely to remain at site DGW, while nearly sixty thousand potentially remain buried in site SZ8.

Some of the sites are basically exhausted, or were never richly abundant to begin with. The reef-top sites, which were surface collections, and sites such as DGD3 and DGW are likely to produce small returns for a lot of effort. Whether due to large area or rich abundance, many sites hold vast quantities of obsidian still. The sites on Watom and the Reef/Santa Cruz islands are expected to yield much obsidian to further excavation. Sites ELT and DFV are the most rewarding prospects on New Ireland and Nissan respectively.

There is more to the gaps in knowledge than just getting the most out of discovered sites on explored islands. As much as I would like to have incorporated the Mussau materials into this study, there exists a void across the span of the Solomon Islands from which virtually no assemblage currently exists. Between Buka and the Reef/Santa Cruz group there are many islands but few excavations and even fewer finds. Mussau on the other hand is of roughly similar average distance from the two sources (i.e., Talasea and Admiralties) as New Ireland and therefore does not offer as much potential for filling in the gap.

Some time periods contain far fewer data points than others. Periods one and four especially are lacking data points in comparison with the other three. Ideally, one would examine the changes across the region for each time period. This would produce five fall-off curves, in the way that there are five profile figures (7.20 through 7.24). To achieve this would require a great deal more excavation as many new sites would be needed to cover the range of space and time. Attempts to include the tertiary sites were unhelpful as abundance figures are extremely difficult to derive from most site reports (Fredericksen 1997b: 383). Specifically, in addition to the usually listed count and total mass of obsidian in a site, one needs count and mass data per excavational unit and individual flake weights to produce comparable numbers to those presented here. In theory these data are available and less difficult to obtain than new excavations would be. In practice they are difficult to calculate even with the assistance of the excavating archaeologist.

IMAGE RECORDING

While the future of the thirty-five millimetre colour positive and black-and-white negative are secure for now, the new direction in photography is clearly digital imaging. Video photography presents enormous benefits with few drawbacks, notably presentation. Use of alternative magnification tools such as the scanning electron microscope and the confocal laser scanning microscope make the capture of quality images easier than ever before and offer possibilities to the lithic analyst for less time-intensive procedures.

Manipulation of the stone flake in the chamber of the SEM is one of the greatest benefits of this system. Mounting a flake with known areas of interest accessible, one can scan around the surface at low power quickly and easily, watching the image on a large screen instead of a pair of ocular lenses. Areas of importance can be identified, magnified up to several thousand times, recorded to film, and analysed all in one brief sequence. The pictures are of sufficient quality that analysis can be reserved for later if desired.

In contrast, using a petrological incident light microscope is very labour intensive. Mounting the flake often allows only one portion of the flake to be viewed at a given time, requiring multiple remounting attempts per flake. Complex shaped flakes sometimes cannot be viewed at all because the objective lens cannot be positioned near the edge. The depth of field is so shallow as to require long training periods before the analyst can actually see what is in the field of view. Identification, examination at differing magnifications, recording to film, and analyses are often reserved for separate sessions to avoid fatigue and the resultant pictures are often quite poor. Certainly they are rarely of a quality required to use in the analysis itself.

Perhaps the digital information revolution will catch up with the dissemination of information at its most basic level. Distribution of this thesis on CD-ROM or from a website on the World Wide Web would allow me to include many extra photographs, including video footage, and the huge tables of data that I produced, or simplify the writing of the document by making each section a stand-alone piece of hypertext. The potential for digital publishing in general I think is becoming quite evident in that inclusion of just a few extra pages can increase the cost of traditional publication whereas there is virtually no extra cost involved in adding extra materials to an electronically published book.

GLASS EROSION

With the advantage of hindsight and an examination of the hydration dating and nuclear waste literature it is difficult to understand how the problem of obsidian erosion in archaeological sites has avoided attention by use-wear researchers for so long. It is convenient for dating purposes that obsidian hydrates at a predictable rate, without this fact OHD would not exist. It is problematic for use-wear analysis however. While the factors accelerating weathering are very specific—high temperature, extremes of pH, and high water vapour pressure—these conditions are not hard to find.

The tropics are an ideal setting for obsidian weathering. The hydration and dissolution reactions proceed at a quicker rate here than in non-tropical settings because of the climate. Use-wear studies in locations such as Melanesia and Mesoamerica are likely to face greater frequency of weathered artefacts as a result.

While we can learn of erosion from the hydration dating experience, perhaps also the OHD researchers can learn from the experience of the erosion observed here. It has long been realised that the conditions of hydrating obsidian are specific enough to require measurements for each site and for each obsidian source to be dated in this way. I have observed flakes where the conditions appear to vary quite markedly from one side of the flake to the other. There were a few flakes in the assemblages that had noticeable erosion on one half but not on the other, with the halves either of plan or sectional view. It is impossible to determine from the information available if the flakes were laying flat on a former soil surface and the sectioning effects were variable, or if the flakes were arranged in variable orientations and the environmental effects affect specific soil layers.

Whether these effects of erosion on dating are now completely under control or not, they will continue to play havoc with use-wear analysis with no obvious solution. Varying from easily observable with the naked eye, to barely noticeable at hundreds of times magnification, the damage needs to be assessed prior to undertaking any functional study, and the potential for this damage needs to be kept in mind whenever a new flake from a tropical site is examined. This erosion occurs on a continuum like other forms of PDSM. Certain lesser levels of damage can be dealt with, while others hopelessly obscure any evidence of use.

METRICAL ANALYSIS

The purpose to the method of characterising the size of each obsidian flake by measuring its mass was to replace a vast number of tedious measurements with considerably fewer, in this case one quarter as many. There are two ways in which I can assess the success of this procedure. The first method is in examining how well it fits the data to which it was applied. The relationship between mass and volume was developed by linear regression of the Ahus flake measurements.

This proved to be a very solid relationship with a high correlation coefficient. The success is measured by how well the regression equation predicts the volume of each flake based on the mass in the Ambitle assemblage. The regression equation that results from the Ambitle data is almost identical to that from the Ahus data, and using the Ahus equation on the Ambitle data produces excellent estimates with low residual figures.

This result strongly reinforces the belief that these two assemblages were produced via identical reduction strategies; that is, that there was no design intended with the knapping beyond the production of sharp edges. It also confirms that flake morphology in such systems varies along predictable lines with longer pieces turning out proportionally thinner than shorter pieces.

The second test of success in this characterisation is how well the predictor data worked for replicating the nested sieving procedure that was necessary to produce the numerous profile graphs that were discussed above. The only physical test I could perform on this formula was to sieve the Ambitle and Ahus data and compare the predicted outcomes with the actual outcomes. The results were very similar for both assemblages in that the prediction produced net residuals of approximately zero.

For each assemblage, the masses were input into a spreadsheet from the balance, the formula was applied to each mass, the result was a prediction of the screen size that would trap each flake. Then the actual screen size that trapped each flake was measured and entered into the spreadsheet. Predicted size is subtracted from actual size yielding the difference, or residual value. The sum of these residual values is the net residual for the data set, and is indicative of how well, on average, the formula predicted the outcome. A few predictions were one size class too big, others were one size class too small, and most were exactly correct. The number of over and under estimations for both the Ambitle and the Ahus collections were roughly equal, and therefore cancel out.

It is impossible to know how accurate the estimates are for each archaeological assemblage without resorting to actually running each one through a nested sieve tower. On the basis of the Ambitle test assemblage, however, it is possible to say that it should be quite accurate. The only problem that is noticeable is the absence of the small flakes from each assemblage. It will be recalled from chapter seven that there were no flakes assigned to the sub-one centimetre size class. Yet for many sites there were quite a number of flakes produced by screening the deposits with a small screen (either one-quarter inch or five millimetre size). The re-examination of some assemblages specifically for flakes in this smallest size class reveals that they do exist. This examination simply consisted of browsing through an assemblage seeking a few examples of the smallest flakes present. These small flakes were then compared to a one centimetre screen for the potential of the flakes to pass through the mesh.

It would appear that the formula accurately predicts the screen size classes for the Ambitle assemblage, a collection of flakes that are all small in size (below three grams), but systematically overestimates the size classifications of the other archaeological materials. I believe the methodology has merit, but it needs further work with additional assemblages to determine exactly where the problem lies. Specifically, one would need to run additional assemblages of obsidian flakes through both the nested sieve stack and through the estimation procedure at the same time and compare the results.

SUMMARY

I have assessed the popular methods of obsidian source determination with specific regard to the implications for further research with the obsidian, particularly use-wear studies. Absolute density determination is the preferred approach in terms of destructiveness, PIXE-PIGME is preferred in terms of source discrimination ability. Looking at abundance of obsidian within the sites, I proposed three alternative modes of distribution and determined that none can be completely discarded or favoured on current evidence. Since I deduced Renfrew's exponential decay model (1977) directly from the data I believe this mode to be most acceptable.

Reduction strategy was demonstrated to be no real strategy at all—expediency ruling the process. I presented an alternative theory to that of White (1997) showing how simple statistical behaviour in excavations that produce few obsidian flakes can account for the observations without resorting to any cultural input. That he found the more common source material to also be larger in size can be a result of chance. The graphical profiles collected the sites into groups representing similar treatment of the obsidian—sites that shared cultural processes in the past. Reef scatter assemblages were not found to be comparable with any archaeological sites, including each other, as it is likely that too much sorting by wave action has occurred. It is improbable that bipolar reduction was an important part of obsidian reduction although the definitive proof of examining the assemblages in the appropriate statistical manner remains to be completed.

Use-wear analysis, despite the scarcity of results, produced an unexpected conclusion. It appears as though the obsidian was used with as little consideration for its potential as when it was knapped. Use-wear also indicates that the most plausible use for the Reef/Santa Cruz graver tool was as a scarification implement. The effects of ill-treatment of an assemblage after excavation were summarised with the correlated need to plan excavation strategy in consideration of both current and future procedures for the lithic material.

Based on all the methods above, it is apparent that the largest geographical gap in site coverage falls in the Solomon Islands, while the chronological gap most in need of filling is the earliest period. The inclusion of published material (the tertiary sites) reinforced the observations of this thesis on sourcing issues, but proved of no utility in terms of abundance measures. Combining observations among many of the methods used reinforced the verity of the outcomes.

The best options for photography of obsidian tools are in the digital media with the SEM as the preferred imaging system. Weathering of obsidian, while worst in the tropics, is a potential problem for research based in any region of the Earth. Observation of these assemblages supports variable intra-site erosion kinetics in contradiction of the hydration dating literature. Finally, the approach to characterising the size of the flakes proved to work well on the test assemblage, but was less secure in its utility on the archaeological materials, suggesting further research is necessary.

Table 9.1.
Site grouping based on short and long profiles. Site names in bold are in the same group by both tests. Note that group #4 and #7 both match with group #1.

	Group #1	Group #2	Group #3
Long	**DGD2-2**	**SZ8-2**	**DGW-4**
	ELT-3	**RF2-2**	**DGD2-5**
	EAB1-4	**SAD-3**	RF6-3
	DFV-5	**SDI-4**	
	ELS-3		
	DFV-1		
	DFF-2		

	Group #4	Group #5	Group #6	Group #7	Group #8
Short	**DGD2-2**	**SZ8-2**	**DGW-4**	**DFV-1**	SAC-3
	ELT-3	**RF2-2**	**DGD2-5**	**DFF-2**	ELS-3
	EAB1-4	**SAD-3**	SAD-4	EAB1-3	DFV-5
	EAB1-1	**SDI-4**			
	EAB1-2				

Table 9.2.
Estimates of remaining obsidian assemblages for several sites. Totals extrapolated from already excavated portions to remaining portions for each site.

Site	Area excavated m^2	Total area m^2	Flakes remaining	Useable remaining
SAC	44	150*	7000	2950
SAD	110*	1400*	6700	3200
SDI	4	150*	1500	1275
ELS	5	200	3350	1475
ELT	2	200*	5500	1375
EAB1	9	35	230	50
EAB2	8.25	275	550	?
DES	5110§	5110§	0	0
DFF	7	200	740	80
DFV	7	180	1400	160
DGD2	6	35	200	90
DGD3	1	26	75	0
DGW	3	8	10	3
DIZ	11250§	11250§	0	0
DAF	39035§	39035§	0	0
RF2	154	1100	3800	2900
RF6	20	10000	8000	4300
SZ8	51	10000*	57000	48000

* estimated minimum § surface collection

Figure 9.1.
A proposed discrete distribution fall-off using hypothetical data. The ordinate values are orders-of-magnitude.

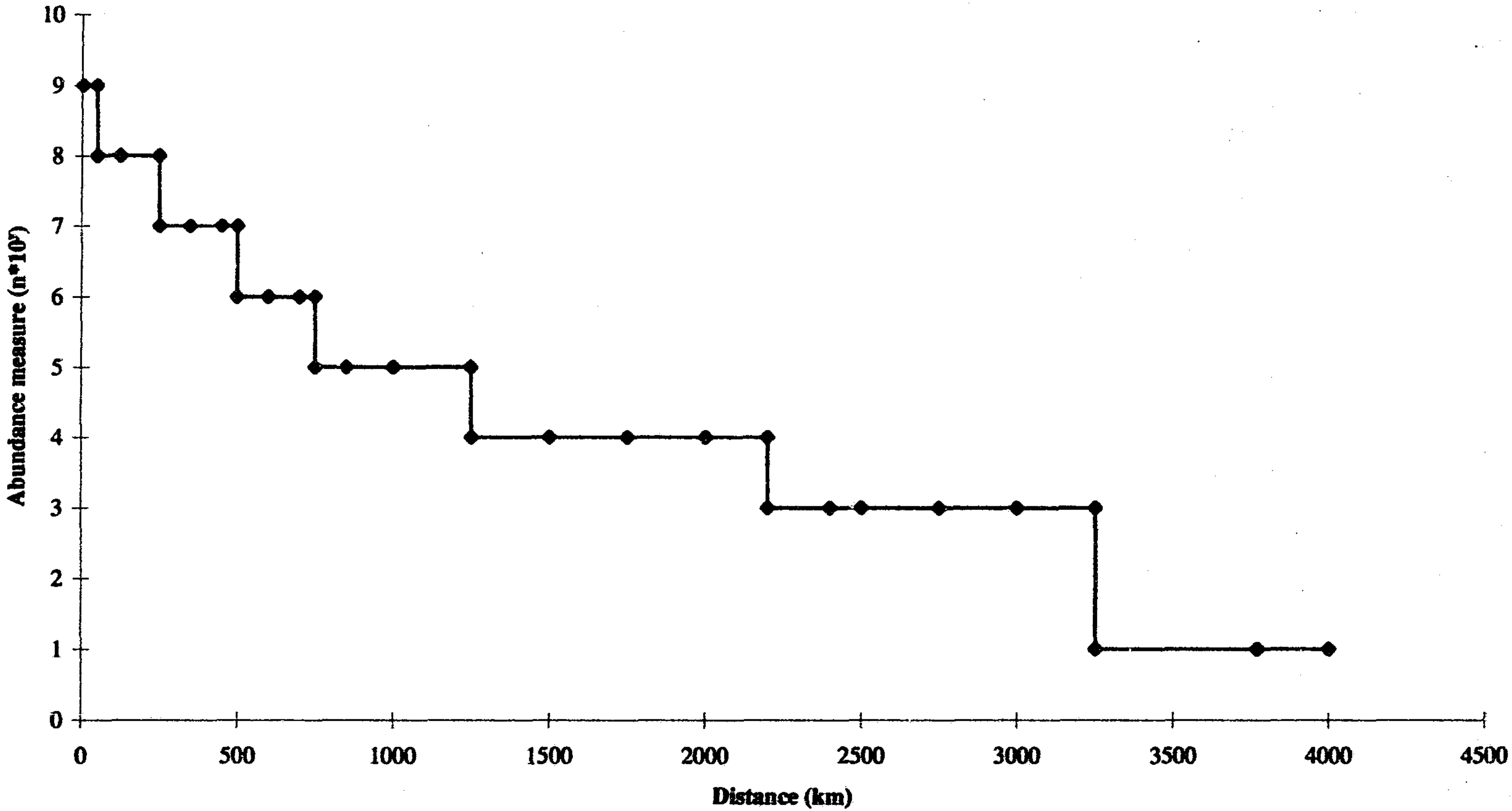


Figure 9.2.
Exponential decay curves for selected exponents compared with the archaeological data set.

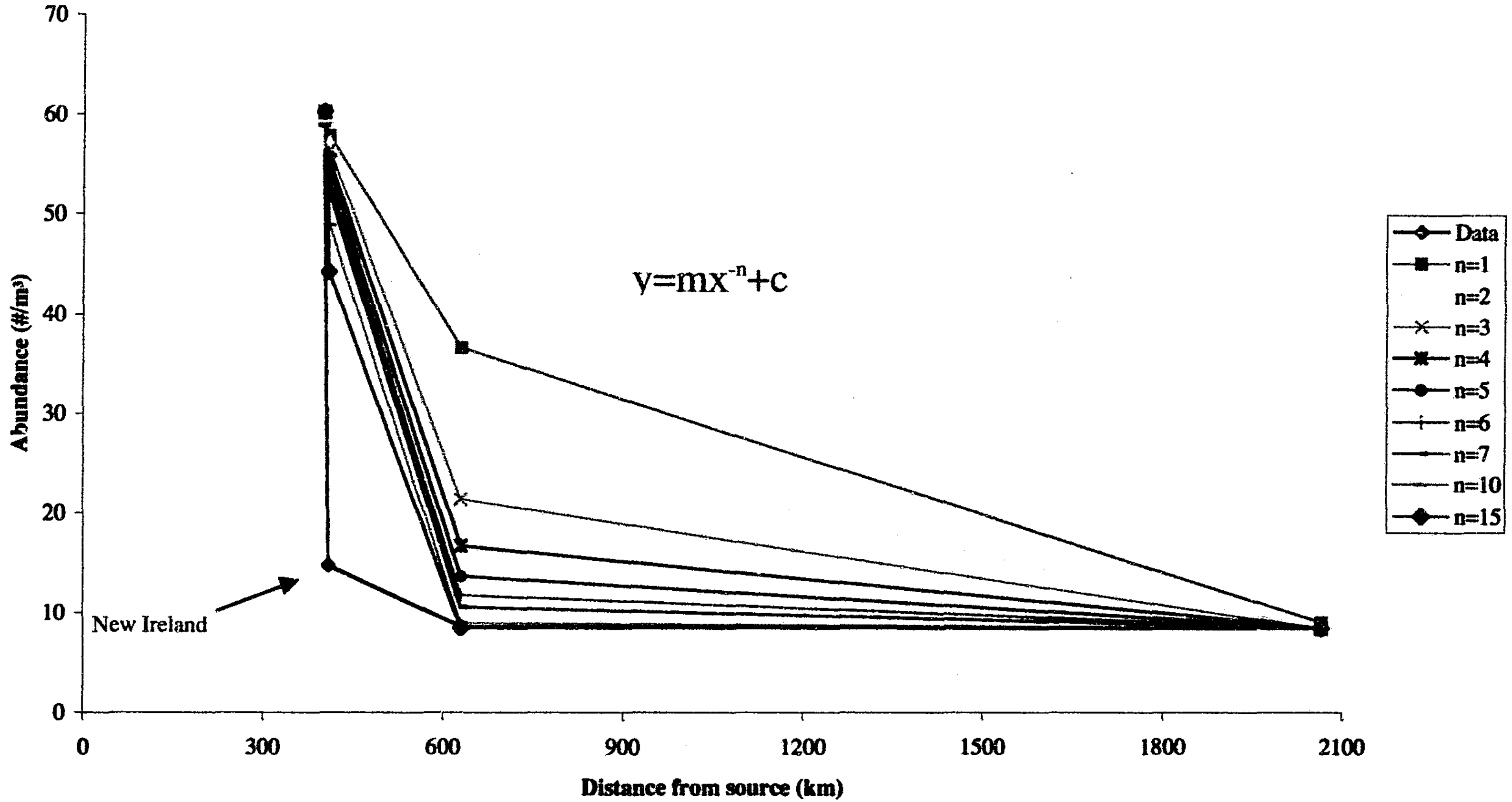


Figure 9.3.
Log-log plot of the curves from figure 9.2.

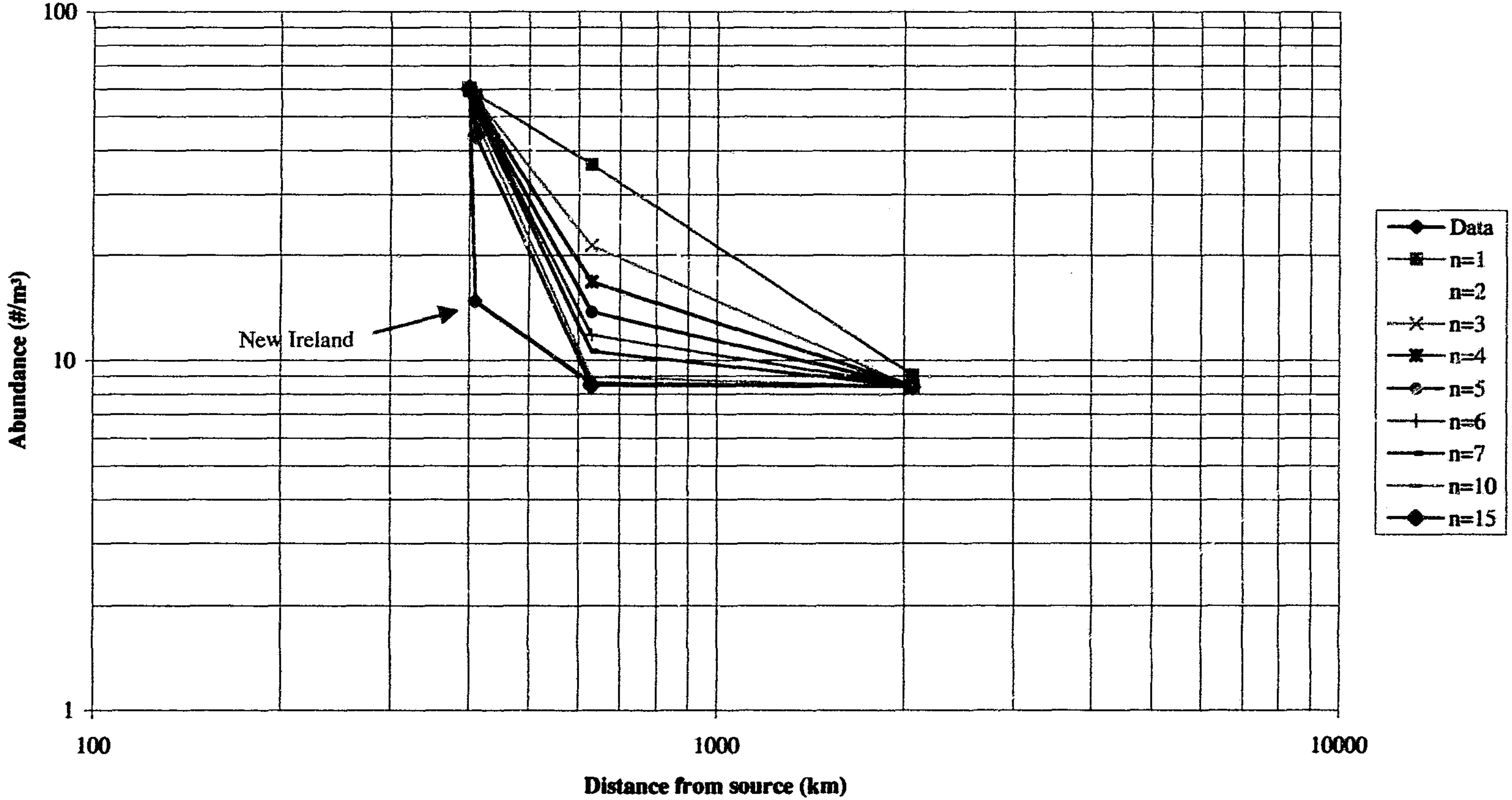


Figure 9.4.
Mean obsidian abundance figures replotted without the statistical-outlier sites.

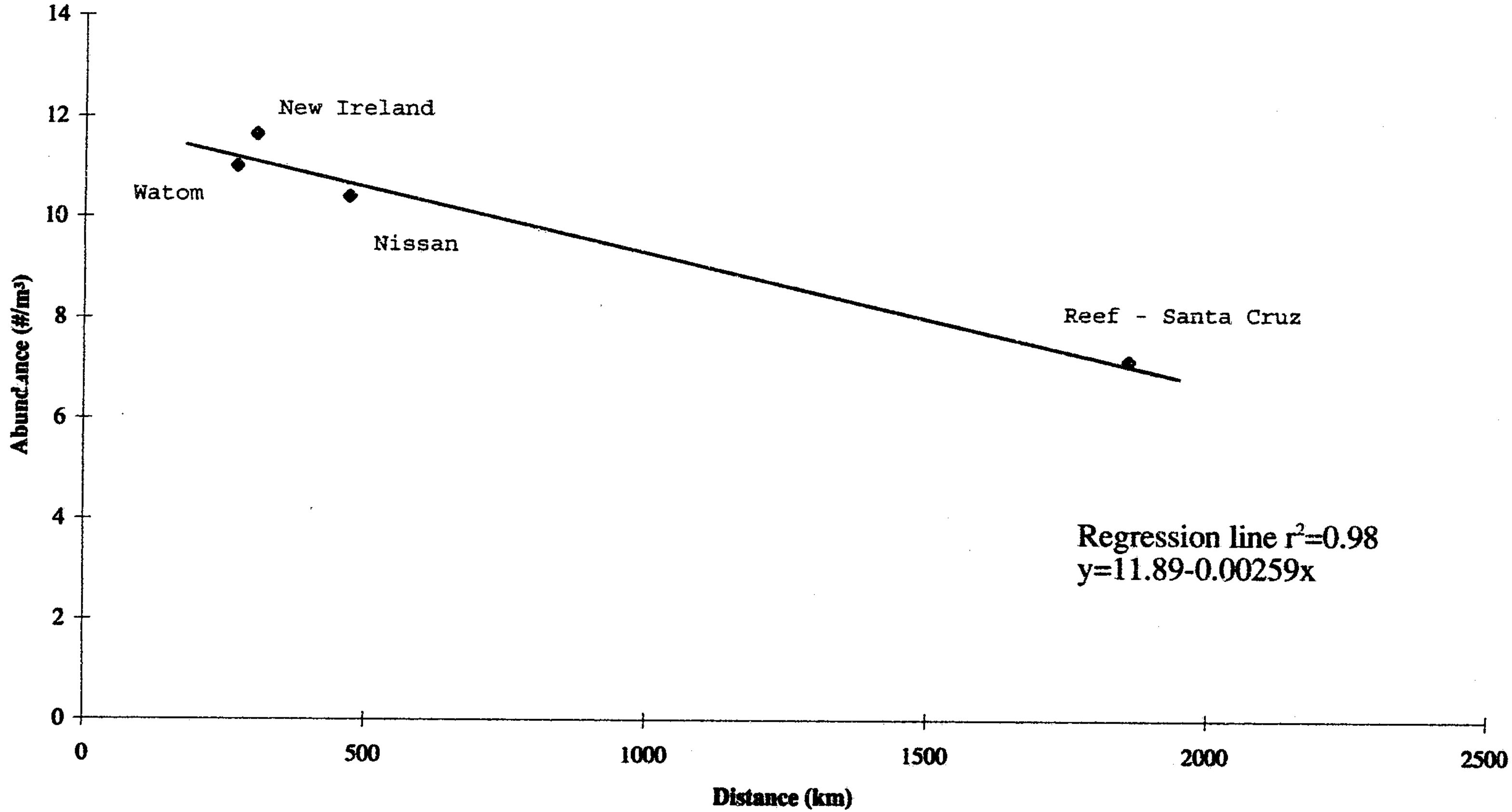


Figure 9.5.
Transformation of the data reveals a 'linear' relationship.

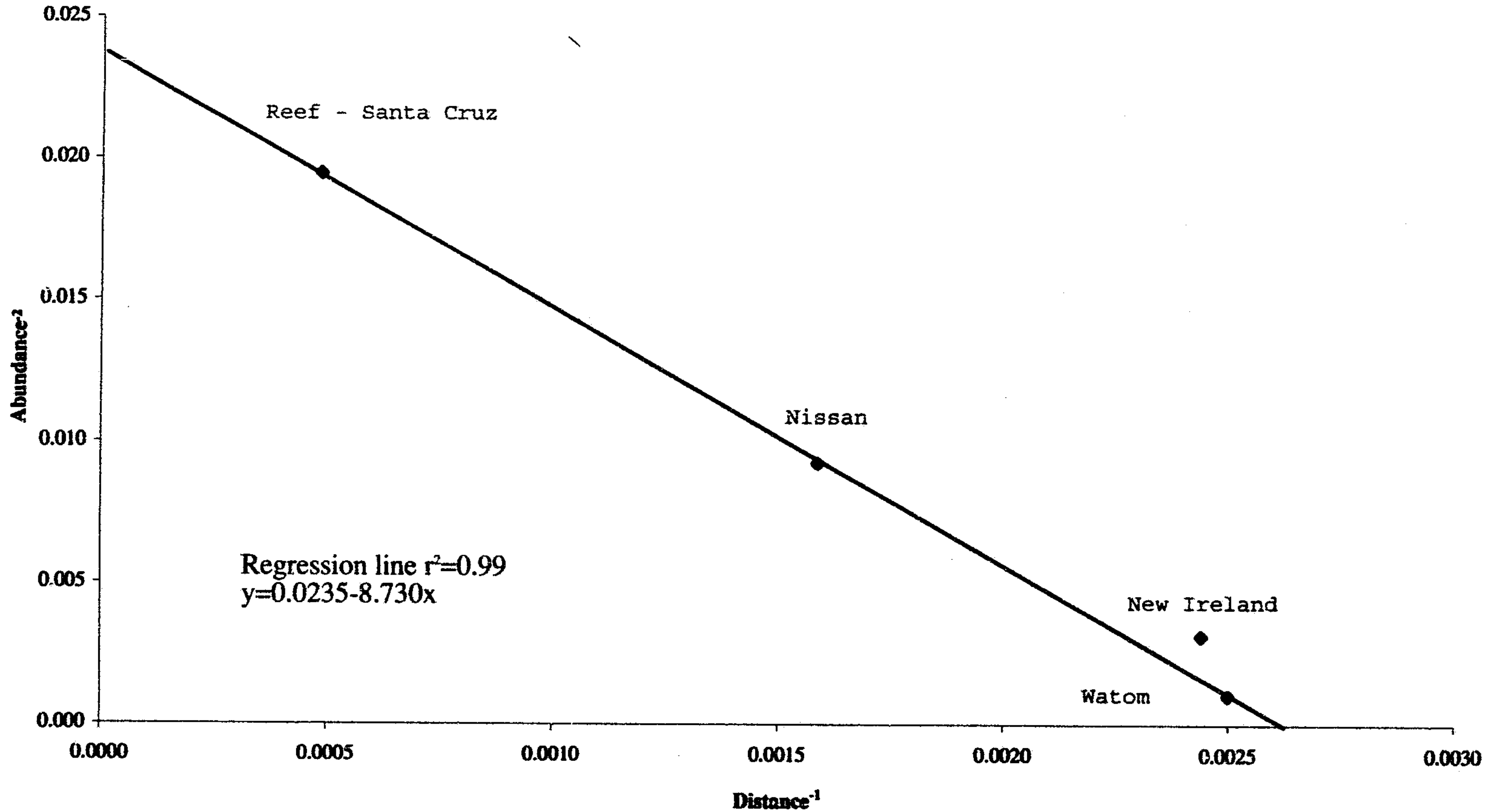


Figure 9.6.
Mean obsidian abundance (grams of obsidian) per time period after removal of statistical-outlier sites.

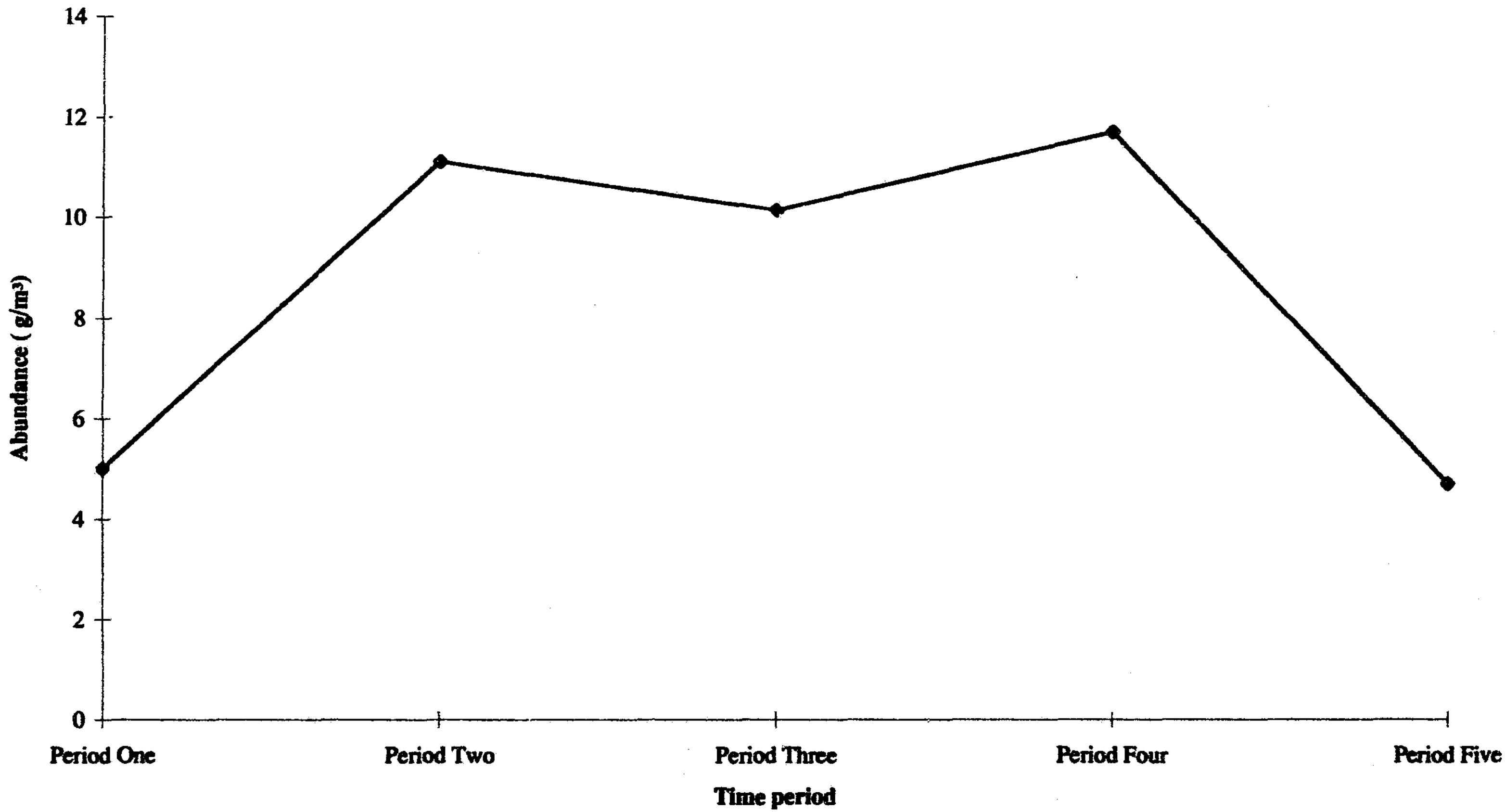
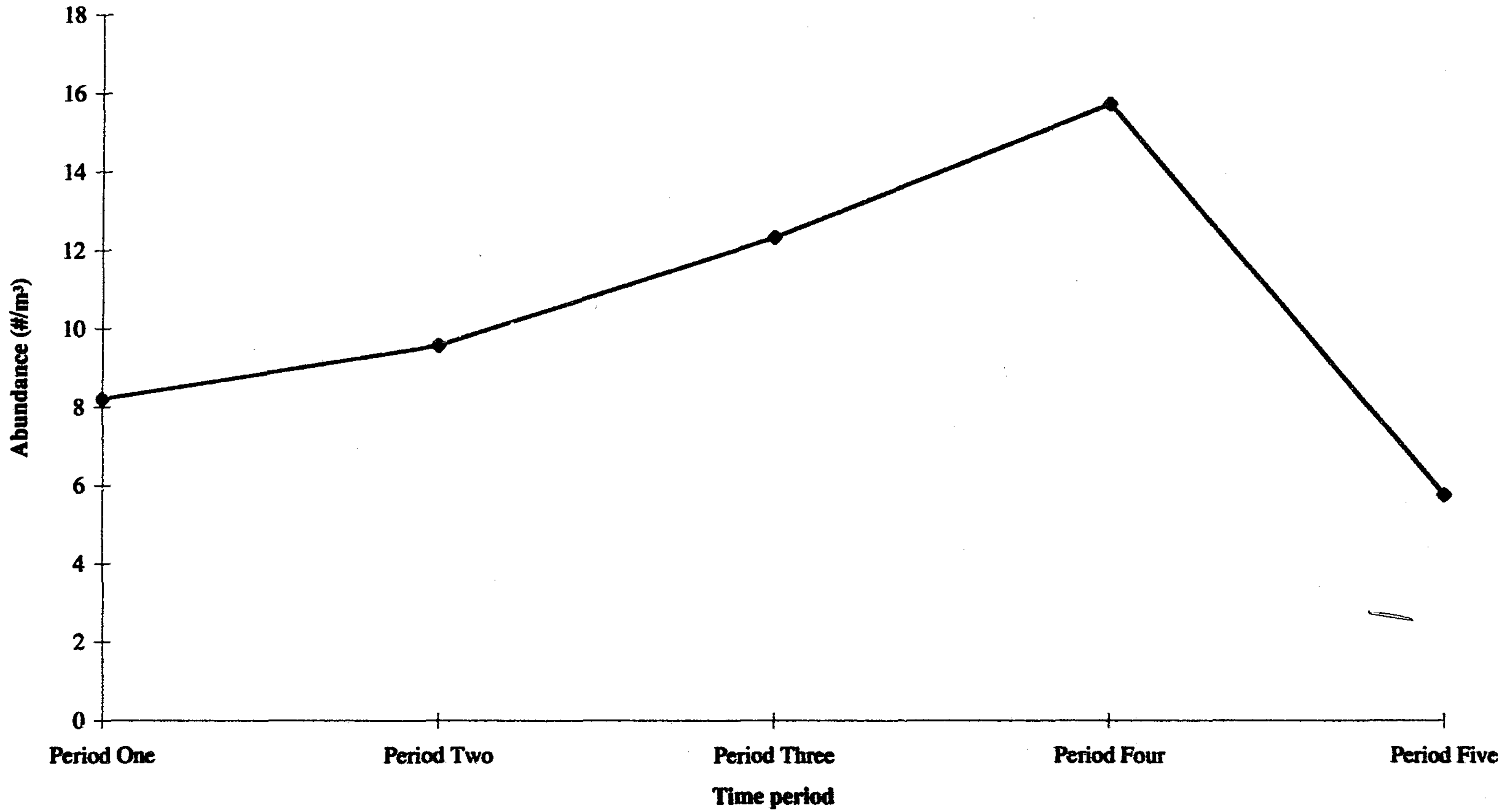


Figure 9.7.
Mean obsidian abundance (number of flakes) per time period after removal of statistical-outlier sites.



§10

Conclusion

As this thesis began, so it concludes. Questions proposed in chapter one are considered here in light of the research undertaken. Not every line of investigation was equally successful in creating answers, and in most cases more questions have been created than put to rest; this is the nature of science.

(1)—What functions were obsidian tools used for?

The question of function was introduced in chapter four, was the primary topic of chapter eight, and was discussed in chapter nine. Despite the low success rate in identifying use-wear on the obsidian tools, the variety of activities identified on the successful ones points away from utilisation of obsidian in the most efficient manner. Rather than saving the resource for its best purpose—cutting—a great variety of tasks were attempted with obsidian flake tools on both plant and animal derived materials. No chronological or spatial functional distinctions were identified that would separate one period or one island from another.

Even on gravers, the only formal tool type seen in the assemblages, no single activity was identified. Rather, all that the gravers have in common is a projecting point produced by opposed retouch on a simple flake. These tools were then used for a few tasks not related to the point itself, and perhaps for two tasks relating to the point. The most likely use for the gravers is cicatrisation, but the use-wear on several of the points suggests there may have been a second, unidentified, use as well. Use-wear on the non-point portion of some of the tools confuses things further. The future in graver research suggests a formal comparison of the Reef/Santa Cruz tools with those from Mussau and New Caledonia that I did not get to view. Even if determining function remains problematic, the form of the tools may prove useful in establishing stylistic links (or divisions) between the three island groups.

Without more functional assignments it is impossible to determine if there was any variation from site to site, or time period to time period. In light of all the data available, however, it seems safe to conclude that differential treatment of obsidian based on place of origin did not exist.

(2)—How were obsidian flake tools produced?

Reduction strategy is the topic of chapter seven, but relies also on the outcome of the metrical study located in chapter eight, and is further addressed in chapter nine. For the vast majority of the obsidian tools examined in this research, and also for those reported in the literature, direct percussion on the hand-held core was the means of reduction used to make flake tools. The knapping of obsidian in Melanesia as based on the flakes in this collection, and on those other assemblages I have had an opportunity to look at, was not completed with a great deal of planning. Neither maximisation of the raw material nor seeking the best edge forms were apparent strategies.

The graver tools are produced by further reduction of an appropriate flake selected during the flaking process. The early stemmed tools that I saw in New Britain were of similar variability to the gravers and could easily have been produced by similar selection processes. Only the triangular cross section spear points of Manus (of all the obsidian tools in Melanesian prehistory), produced up until the recent past (periods four and five), appear to have been shaped with a specific start-to-finish strategy.

In some instances bipolar reduction may have been employed towards the end of the reduction process, when the core was getting too small to hand-hold comfortably. Bipolar flaking, however, is not a ubiquitous strategy employed throughout Melanesia. Nor does it appear to have been used throughout the life of a core.

Production methods at the highest level do not vary across the region. That is, the casual knapping method employed to produce the majority of simple flake products is unchanged from Manus to the Solomon Islands. Localised variations clearly exist at lower levels of production. The graver tools are confined to the Reef/Santa Cruz group among the islands in this study (but also found in the St. Matthias group and New Caldeonia). The earliest stemmed tools are found only in the western end of the region, probably because of their restriction in time. Similarly, the spear points were produced only in the Admiralties and distribution beyond that archipelago is very restricted.

In the fine details, it was shown that some production differences were regional. Size variation exists between islands, but not enough data points were available to make a strong argument about systematic regional trends. It is obvious that some sites were "production centres" whereas others were "utilisation centres" and these differences were reflected in the flake size profiles. Given the degree of internal variation between sites on a single island, the conclusion I draw is that these apparent regional differences are not due to cultural difference. They are a result of sampling effects and mirror alternative activities within a village setting. As with the spatial assessment, the chronological view is that expedient reduction methods are predominant from the earliest tool production known through to the ethnographic period. Again, there were some chronological variations at the fine detail level. Inter- and intra- island variations were both of similar magnitude.

The stemmed tool type is confined by time and by region; they predate the Lapita period settlements on New Britain, but are unknown from contemporary sites on New Ireland. The graver tools are confined to Lapita times, but are more correctly viewed as a local idiosyncrasy (perhaps 'gravers' were produced elsewhere out of biodegradable materials?) than a chronological innovation on current evidence. Only the mass production of spears in the Admiralties reveals an evolution in approach through time.

Obsidian source played no part in the reduction process. It would be surprising to discover otherwise in light of the lack of variation noted above. The casual approach in reduction indicated above almost demands that such discrimination be non-existent.

(3)—How are form and function linked?

Both the form and the function of flake tools are dealt with in chapter seven and nine. In general, I would say that morphology and use are not linked, not culturally at least. The shape of a given flake is apparently a random product of the knapping procedure. Function is determined in part by the shape of the flake, since you cannot cut with a blunt-edged flake for example. If there is no planning involved in the production of the flake shapes, then there is no planning involved in the subsequent function.

It is a given, however, that there must be some link between the shape and the range of tasks to which the flake can be put. Gravers were made out of thin roughly rectangular flakes. Cutting edges occur on different flakes than scraping edges. This question is most appropriate for collections rich in formal tool types. It would also have been interesting if there were large amounts of functional data available. In the current situation, it is impossible to pursue this further.

(4)—What is the nature of the exchange system?

The nature of the exchange system, a concept introduced in chapter three, is dealt with in chapter nine where all of the evidence was available for consideration. Simply on the basis of low quantity of obsidian and long distances of transport, any system of commercial enterprise by specialist traders can be easily discounted. This will remain the case unless it is later demonstrated that large amounts of biodegradable materials made up the bulk of the trader's wares (and just how this might be proven is difficult to see). We know from the treatment of obsidian that it was not considered as a valuable commodity for tool production. That it may have had value in some other manner is a possibility.

Looking at the quantity of obsidian deposited in sites across the region revealed unexpected linkages between the source areas and the rest of the islands. In other locations, obsidian exchange was found to closely match an exponential decay profile, corresponding to known human behaviours. In Melanesia, it looks as though unique factors were at work in determining the abundance of obsidian on an island.

The quantity of obsidian decreases with distance away from the source, but it appears to be a linear decrease and not an exponential one over a two thousand kilometre range. There was some cultural pressure to provide a rare resource in amounts that almost disregard the distance involved. Green (1987) suggested that this pressure comes from the desire to maintain familial ties between islands and that the obsidian is not desired for its specific mechanical properties. I have discovered nothing to contradict this theory.

Differences in the mode of exchange are apparent from the data available at the main source areas in the Admiralties and New Britain. With several similar volcanoes to choose from, exports originate from one particular volcanic source preferentially at each location while local obsidian usage is much less discriminatory. At the extreme end of the range, obsidian is found in minuscule quantities well below what is expected based on the evidence from closer islands and on a linear decay model.

Chronologically there are some obvious differences in obsidian transportation throughout prehistory. Early on, Talasea obsidian was only moved a relatively short distance from the source to the neighbouring islands. Admiralties obsidian was not seen outside that archipelago. The peak era of obsidian exchange was periods two and three; during and immediately after the Lapita cultural period. At that time obsidian was transported thousands of kilometres. After that time, long-range connections became less important. The trend seen in the Reef/Santa Cruz group on the far east of the region was towards increasing reliance on more local sources. To make any further conclusions requires more data than are available.

(5)—Are there alternative raw materials competing with obsidian for tool production?

The composition of lithic materials in each site is covered in chapter five while comparison between chert and obsidian flakes is located in chapter eight. In the Reef/Santa Cruz assemblages chert increased in proportion through time, successfully substituting for obsidian in the provision of sharp edges. Production techniques were hardly affected by the change in raw material. No chert gravers were found, however. At Balof, the opposite trend occurred. Lots of chert flakes early on gave way almost completely to obsidian from the time obsidian became available there. Many of the small islands in Melanesia have few, or no, local stone resources to compete with obsidian. Any alternate stone would have to be imported like the obsidian was. This did not occur systematically at all.

(6)—How did the alterations to the flake tools occur?

Since the weathering of the obsidian surface affected the use-wear procedure, the two are located together in chapter eight and discussed in chapter nine. The primary factor involved in modification of the tool surface was the same process by which obsidian hydration dating is possible. Obsidian absorbs water by chemical hydration in the warm and humid environs of Melanesia with associated surface losses. These losses obscure most evidence of use-wear and take adhering residues away. The dissolution reaction does not proceed uniformly across the surface of the flake, but attacks locally vulnerable parts of the flake preferentially. The result is often erosion marks that can, at first, be confused with use-wear marks.

This erosion proceeds haphazardly, affecting portions of one flake and sections of a site while leaving others unaffected (or less affected). It is a process that has been virtually unreported in the literature with regards to functional analyses, but one which is generally well covered by the OHD practitioners and physical chemists. I cannot help but wonder if this characteristic of differential erosion on a single flake is a real phenomenon that has implications for OHD accuracy, or if it is an idiosyncrasy of my observations?

The hydration reaction is properly called a PDSM. A secondary factor involved in damaging the use-wear potential of many of these flakes would more correctly be termed a post-excavational surface modification. Archaeologists themselves have inflicted troubling changes on too many flakes. Much of this is through ignorance, and no longer considered acceptable practice (thankfully). Standard practice in the era before use-wear studies was to bag all the flakes from one spit in a single bag, with predictable consequences. Tracing the outline of a flake to record its plan shape makes the process quick, but really is disastrous. Washing the tool for one procedure (usually sourcing) removes the very stuff of residue analysis. Between the environmental and the procedural damages, functional traces disappeared. Consideration of future technical developments necessitates saving some flakes untouched.

(7)—Which methods of obsidian study are successful, and why?

Critical review of the methods employed in this thesis relies on examination of all the procedures employed and their respective outcomes; the general topic of chapters six through nine. While sourcing was an important part of this research, the relative merits of the various methods have been covered in previous chapters. There were no developments to the techniques that would require inclusion here. Since thin sectioning of flakes is vital to OHD procedures, and for EDS sourcing, it is going to continue as part of the archaeologist's regime. I would recommend however that small sample sizes be used, perhaps after the flakes have been examined for use-wear traces. The alternative, as seen with the Tikopia assemblage, is 'destruction' of the whole. The choosing of only debitage pieces for sectioning, while avoiding the problem of destruction of use-wear data, could lead to a skewed outcome in locations where there is differential treatment of materials from different sources.

Measuring the abundance of obsidian deposited in a site revealed some interesting regional information. There is more obsidian on remote islands than would be expected by traditional nearest neighbour exchange practices. Yet there is not enough to suggest commercial activities either. The exact nature of exchange in prehistoric Melanesia, particularly during Lapita times, remains to be determined. I believe this will only be accomplished by inclusion of other materials besides obsidian into the regional study.

Grouping sites on the basis of flake size profile proved a successful exercise. It would have been ideal if it was accompanied by a range of experimental processes to identify the 'cause' of each unique profile, but this was not possible. While in this case it complemented the Chi Squared test, in a study with more data the profile test would have proven just as useful, while the Chi Squared test would have been unwieldy. There are few assumptions involved in the profiles, and these can be controlled for. Assumptions are many in the statistical tests and difficult or impossible to assess much of the time. The profile test is therefore a more secure method of inquiry.

Functional analysis by use-wear and residue scrutiny are now mature procedures. Provided that the analyst checks accuracy with a blind test as I did, the results should be beyond doubt. The use of an expert system can be recommended to maintain consistency, but is perhaps more effort than is necessary with the time investment currently required to develop one. Continued developments in the use of the SEM and new technologies will, I believe, make functional analyses quicker, easier, and more accurate in future. The potential for information gathering of such activities is immense.

The use of mass measures to replace multiple size measures shows potential. The applicable scope of such a procedure is obviously limited to assemblages not only of similar reduction techniques, but to locations characterised by a casual approach to knapping. While the accuracy was demonstrated on one test case, a worrying systematic error was detected in the general archaeological data. It would require further testing to establish if the technique is worth continuing with, but I believe it is worth pursuing just for the damage it prevents to the flakes if not for the manual labour saved. Research pursuing specific questions pertaining to metrical variation would not be amenable to this approach.

(8)—What next?

To propose adequate future directions in research requires a review of the outcome of this project hence this question relies on the preceding seven questions addressed in this chapter. Were it as easy to produce the information as it is to request it, I would have incorporated it all into the research long ago. Holes in the data coverage seem much larger now that I have completed this project than they did before I had begun. More material from early sites is essential to understanding the effects of Lapita populations on those in Melanesia before them. Obviously these will only come as more field work is accomplished. The spread of data from period two through to period five was quite even, but of course could have benefited from more data points in each period.

I wrote in my theoretical section (chapters two through four) of several archaeologist's requests for future lithic research to be innovative and to link to cultural ideas. Or, as I noted in chapter two, to 'do' more prehistory along with the archaeology. Much archaeology was done, but the prehistory requires rather more work to develop than was possible here.

More sites in the eastern Bismarcks are being uncovered with each field season. Not only on the larger islands of New Britain and New Ireland, but on their smaller neighbours. Archaeology is also proceeding at the eastern end of the region, in Vanuatu and New Caledonia for example. It still remains unclear whether many Lapita sites will be found in the Solomon Islands or not. It is not then a matter of calling attention to locations neglected by research, but a case of waiting until another sampling of prehistory at least equal to the one presented here is compiled before anyone attempts another regional study of the obsidian. I suggest that this is a task that will then be worth undertaking.

Appendix

This table presents the results of the source determination procedure I undertook. Density (in grams per millilitre) is calculated from the mass of the obsidian piece in air, the mass in PFMD (both in grams), and the ambient temperature (in Celsius degrees) according to the following formula (Ambrose n.d.):

$$Density = mass_{air} \times \frac{temp - 965.99 \big/ -480.88}{mass_{air} - mass_{PFMD}}$$

Pieces #0 and #187 are Ambrose's standardisation pieces. The first one is a cube of quartz, the other a cube of obsidian that are repeatedly weighed throughout the measurement sessions to accommodate for temperature drift. The next ten pieces (#1131–#2371) were acquired directly from the source locations and therefore act as a test of the procedure. The archaeological pieces come from sites DFF and DGW on Nissan, site DAF on Sohano, and four sites on Tikopia.

As a result of making multiple measurements of the standards it is possible to determine the uncertainty in the calculation of density via this method. The value of the final digit changes by a maximum of three, but the standard deviation is less than one (i.e., less than 0.001 g/ml). Those sources listed as Talasea, Admiralties or Banks are reliable, as the density measured falls within the expected range for one of the sources (fig 6.1). Those listed as "Possibly from" are outside the expected range, but within the maximum observed deviation in the standards of 0.003 g/ml, and therefore could be obsidian from the listed source. The remaining obsidian is either from another source such as Fergusson or more likely from an unidentified sub-source within the three source regions, or it has an inclusion that alters the measured density (Torrence and Victor 1995).

Three forms of inclusions that I have observed are gas bubbles, crystalline formations, and assorted volcanic rock. While good quality obsidian is considered good quality precisely because it is free from included materials, the potential to find a random piece with an inclusion exists for all obsidian. The effects of a small inclusion in a flake obviously vary inversely with the size of the flake, so extremely small flakes are harder to do source determinations with based on density.

Table A.
Raw data from the absolute density measurement sourcing procedure (as described in chapter six) including specimen number, provenience data, specimen label, specimen mass and specimen density. The resulting source assignment is listed in the last column.

#	Provenience*	Label	Mass (g)	Density (g/ml)	Source
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.647	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.647	-
0	-	Quartz	3.36	2.648	-
0	-	Quartz	3.36	2.648	-
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.352	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.354	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea

* A note is necessary regarding the provenience data at site DAF. Wickler divided up this surface collected site into twelve areas (Wickler 1995: fig 3.2). I have listed my interpretation of which area each obsidian piece is from. After comparing notes, Wickler (pers. comm.) informed me that somehow the labelling must have been altered as it was clearly not correct. Just one example is the flakes labelled as being from area eight and south of the wharf. It cannot be both as area eight is at the north end of the site and the wharf roughly divides the site in half along the north-south axis.

187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.352	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.352	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
187	-	Standard	3.27	2.351	Talasea
1131	Source	Umleang	5.92	2.392	Admiralties
1867	Source	Bitokara	0.89	2.346	Talasea
1873	Source	Kumeraki	0.76	2.353	Talasea
1875	Source	Bao	10.02	2.354	Talasea
1880	Source	Voganakai	19.76	2.343	Possibly Talasea
2023	Source	Pam Lin	4.87	2.377	Possibly Admiralties
2043	Source	Lou	0.73	2.397	Admiralties
2048	Source	Lou	1.99	2.379	Admiralties
2365	Source	Schauman	1.37	2.352	Talasea
2371	Source	Mopir	1.54	2.345	Talasea
3117	SQ4 40–50 cm	DFF	0.09	2.349	Talasea
3118	SQ4 40–50 cm	DFF	0.41	2.342	Possibly Talasea
3119	SQ4 40–50 cm	DFF	0.14	2.350	Talasea
3120	SQ4 40–50 cm	DFF	0.35	2.393	Admiralties
3121	SQ4 40–50 cm	DFF	0.26	2.334	Below
3122	SQ4 60–70 cm	DFF	0.07	2.393	Admiralties
3123	SQ4 60–70 cm	DFF	0.29	2.351	Talasea
3125	SQ5 20–30 cm	DFF	0.38	2.349	Talasea
3126	SQ5 20–30 cm	DFF	0.25	2.334	Below
3127	SQ5 20–30 cm	DFF	0.05	2.314	Below
3128	SQ5 20–30 cm	DFF	0.09	2.329	Below
3129	SQ5 20–30 cm	DFF	0.39	2.351	Talasea
3130	SQ5 30–40 cm	DFF	0.35	2.351	Talasea
3131	SQ5 30–40 cm	DFF	0.51	2.353	Talasea
3132	SQ5 30–40 cm	DFF	0.28	2.397	Admiralties
3135	SQ5 40–50 cm	DFF	0.21	2.353	Talasea
3137	SQ6 +26–0 cm	DFF	0.23	2.386	Admiralties
3139	SQ6 40–50 cm	DFF	0.44	2.395	Admiralties
3140	SQ7 30–40 cm	DFF	0.32	2.360	Talasea
3141	SQ7 40–50 cm	DFF	0.24	2.335	Below

3142	SQ7 30–40 cm	DFF	0.30	2.351	Talasea
3143	SQ7 30–40 cm	DFF	0.32	2.390	Admiralties
3146	SQ7 30–40 cm	DFF	0.06	2.352	Talasea
3148	SQ2 0–10 cm	DGW	0.13	2.383	Admiralties
3149	SQ2 0–10 cm	DGW	0.20	2.349	Talasea
3151	SQ2 160–170 cm	DGW	0.33	2.394	Admiralties
3152	SQ2 170–180 cm	DGW	0.81	2.395	Admiralties
3154	SQ2 230–250 cm	DGW	0.19	2.347	Talasea
3155	SQ3 110–120 cm	DGW	0.28	2.395	Admiralties
3156	SQ3 120–130 cm	DGW	0.45	2.302	Below
3157	SQ3 140–150 cm	DGW	1.35	2.388	Admiralties
3159	SQ3 150–160 cm	DGW	0.26	2.397	Admiralties
3161	SQ3 160–170 cm	DGW	0.09	2.395	Admiralties
3162	SQ3 160–170 cm	DGW	0.16	2.395	Admiralties
3163	SQ3 160–170 cm	DGW	0.11	2.442	Anomalous
3164	SQ3 160–170 cm	DGW	0.16	2.393	Admiralties
3165	SQ3 160–170 cm	DGW	0.49	2.397	Admiralties
3166	SQ3 160–170 cm	DGW	0.26	2.396	Admiralties
3173	SQ3 170–180 cm	DGW	0.14	2.385	Admiralties
3174	SQ3 170–180 cm	DGW	0.19	2.375	Possibly Admiralties
3175	SQ3 170–180 cm	DGW	0.34	2.387	Admiralties
3176	SQ3 170–180 cm	DGW	0.48	2.395	Admiralties
3177	SQ3 170–180 cm	DGW	0.20	2.385	Admiralties
3178	SQ3 170–180 cm	DGW	0.34	2.393	Admiralties
3179	SQ3 170–180 cm	DGW	0.77	2.395	Admiralties
3180	SQ3 180–190 cm	DGW	0.16	2.403	Admiralties
3181	SQ3 180–190 cm	DGW	0.35	2.367	Between1
3182	SQ3 180–190 cm	DGW	0.40	2.401	Admiralties
4589	Area 10	DAF	5.52	2.401	Admiralties
4590	Area 10	DAF	2.18	2.408	Between2
4591	Area 10	DAF	2.80	2.399	Admiralties
4592	Area 10	DAF	1.51	2.411	Between2
4593	Area 10	DAF	4.59	2.395	Admiralties
4594	Area 10	DAF	4.13	2.394	Admiralties
4595	Area 10	DAF	0.60	2.392	Admiralties
4596	Area 10	DAF	3.67	2.399	Admiralties
4597	Area 10	DAF	5.35	2.393	Admiralties
4598	Area 10	DAF	5.90	2.400	Admiralties
4599	Area 10	DAF	3.01	2.393	Admiralties
4600	Area 10	DAF	2.90	2.362	Talasea
4601	Area 10	DAF	4.29	2.390	Admiralties
4602	Area 10	DAF	0.44	2.394	Admiralties
4603	Area 10	DAF	2.59	2.390	Admiralties
4604	Area 10	DAF	2.14	2.395	Admiralties
4605	Area 10	DAF	2.58	2.390	Admiralties
4606	Area 10	DAF	2.36	2.390	Admiralties
4607	Area 10	DAF	1.69	2.384	Admiralties
4608	Area 10	DAF	0.62	2.394	Admiralties
4609	Area 10	DAF	2.38	2.393	Admiralties
4610	Area 10	DAF	1.73	2.393	Admiralties
4611	Area 10	DAF	1.34	2.397	Admiralties
4612	Area 10	DAF	1.53	2.391	Admiralties

4613	Area 10	DAF	1.36	2.367	Possibly Talasea
4614	Area 10	DAF	0.81	2.401	Admiralties
4615	Area 10	DAF	2.10	2.395	Admiralties
4616	Area 10	DAF	2.52	2.392	Admiralties
4617	Area 10	DAF	1.22	2.394	Admiralties
4618	Area 10	DAF	2.63	2.395	Admiralties
4619	Area 10	DAF	2.97	2.397	Admiralties
4620	Area 10	DAF	1.48	2.400	Admiralties
4621	Area 10	DAF	1.37	2.399	Admiralties
4622	Area 5	DAF	1.09	2.395	Admiralties
4623	Area 5	DAF	3.28	2.396	Admiralties
4624	Area 5	DAF	2.36	2.393	Admiralties
4625	Area 5	DAF	0.92	2.377	Possibly Admiralties
4626	Area 5	DAF	1.44	2.392	Admiralties
4627	Area 5	DAF	2.30	2.395	Admiralties
4628	Area 5	DAF	1.63	2.392	Admiralties
4629	Area 5	DAF	3.13	2.393	Admiralties
4630	Area 5	DAF	2.54	2.396	Admiralties
4631	Area 5	DAF	2.47	2.394	Admiralties
4632	Area 5	DAF	3.24	2.391	Admiralties
4633	Area 5	DAF	2.50	2.392	Admiralties
4634	Area 5	DAF	2.43	2.396	Admiralties
4635	Area 5	DAF	1.30	2.393	Admiralties
4636	Area 5	DAF	0.38	2.398	Admiralties
4637	Area 5	DAF	1.26	2.394	Admiralties
4638	Area 5	DAF	2.93	2.396	Admiralties
4639	Area 5	DAF	3.95	2.382	Admiralties
4640	Area 5	DAF	4.93	2.392	Admiralties
4641	Area 5	DAF	5.04	2.397	Admiralties
4642	Area 5	DAF	2.91	2.391	Admiralties
4643	Area 5	DAF	0.61	2.392	Admiralties
4644	Area 5	DAF	3.74	2.392	Admiralties
4645	Area 5	DAF	1.48	2.382	Admiralties
4646	Area 5	DAF	2.07	2.395	Admiralties
4647	Area 5	DAF	1.43	2.391	Admiralties
4648	Area 5	DAF	11.20	2.395	Admiralties
4649	Area 5	DAF	2.65	2.398	Admiralties
4650	Area 5	DAF	2.39	2.393	Admiralties
4651	Area 5	DAF	5.66	2.388	Admiralties
4652	Area 5	DAF	1.16	2.392	Admiralties
4653	Area 5	DAF	7.33	2.395	Admiralties
4654	Area 5	DAF	0.18	2.390	Admiralties
4655	Area 5	DAF	1.09	2.394	Admiralties
4656	Area 5	DAF	0.89	2.382	Admiralties
4657	Area 5	DAF	1.43	2.389	Admiralties
4658	Area 5	DAF	0.37	2.393	Admiralties
4659	Area 5	DAF	1.78	2.392	Admiralties
4660	Area 5	DAF	0.99	2.392	Admiralties
4661	Area 5	DAF	1.54	2.394	Admiralties
4662	Area 5	DAF	0.17	2.398	Admiralties
4663	Area 5	DAF	1.09	2.395	Admiralties
4664	Area 5	DAF	0.45	2.388	Admiralties

4665	Area 12	DAF	1.66	2.392	Admiralties
4666	Inner Area 3	DAF	4.86	2.392	Admiralties
4667	Area 12	DAF	3.54	2.387	Admiralties
4668	Area 12	DAF	7.13	2.394	Admiralties
4669	Area 12	DAF	3.07	2.391	Admiralties
4670	Area 12	DAF	2.44	2.394	Admiralties
4671	Area 12	DAF	2.03	2.396	Admiralties
4672	Area 12	DAF	0.40	2.393	Admiralties
4673	Area 12	DAF	3.33	2.379	Admiralties
4674	Area 12	DAF	1.92	2.395	Admiralties
4675	Area 12	DAF	5.89	2.392	Admiralties
4676	Area 12	DAF	1.08	2.389	Admiralties
4677	Area 4	DAF	0.25	2.394	Admiralties
4678	Area 4	DAF	0.70	2.395	Admiralties
4679	Area 4	DAF	0.87	2.391	Admiralties
4680	Area 4	DAF	2.52	2.389	Admiralties
4681	Area 4	DAF	1.28	2.403	Admiralties
4682	Area 4	DAF	0.72	2.386	Admiralties
4683	Area 4	DAF	0.90	2.395	Admiralties
4684	Area 4	DAF	0.74	2.391	Admiralties
4685	Area 4	DAF	0.83	2.389	Admiralties
4686	Area 4	DAF	1.85	2.391	Admiralties
4687	Area 4	DAF	2.40	2.393	Admiralties
4688	Area 4	DAF	1.06	2.393	Admiralties
4689	Area 4	DAF	2.55	2.401	Admiralties
4690	Area 4	DAF	2.16	2.404	Possibly Admiralties
4691	Area 4	DAF	2.46	2.390	Admiralties
4692	Area 4	DAF	1.74	2.394	Admiralties
4693	Area 4	DAF	2.35	2.385	Admiralties
4694	Area 4	DAF	2.16	2.383	Admiralties
4695	Area 4	DAF	1.24	2.392	Admiralties
4696	Area 4	DAF	0.95	2.394	Admiralties
4697	Area 4	DAF	1.41	2.365	Possibly Talasea
4698	Area 4	DAF	0.97	2.389	Admiralties
4699	Area 4	DAF	2.24	2.385	Admiralties
4700	Area 4	DAF	0.61	2.377	Possibly Admiralties
4701	Area 4	DAF	0.70	2.396	Admiralties
4702	Area 4	DAF	1.37	2.392	Admiralties
4703	Area 4	DAF	2.12	2.397	Admiralties
4704	Area 4	DAF	2.80	2.391	Admiralties
4705	Area 4	DAF	1.00	2.391	Admiralties
4706	Area 4	DAF	1.95	2.385	Admiralties
4707	Area 4	DAF	1.19	2.400	Admiralties
4708	Area 4	DAF	0.74	2.392	Admiralties
4709	Area 4	DAF	0.69	2.392	Admiralties
4710	Area 4	DAF	1.40	2.393	Admiralties
4711	Area 4	DAF	0.52	2.391	Admiralties
4712	Area 4	DAF	1.92	2.393	Admiralties
4713	Area 4	DAF	0.39	2.393	Admiralties
4714	Area 4	DAF	0.57	2.395	Admiralties
4715	Area 4	DAF	1.38	2.395	Admiralties
4716	Area 4	DAF	2.56	2.392	Admiralties

4717	Area 8	DAF	2.68	2.398	Admiralties
4718	Area 8	DAF	7.06	2.393	Admiralties
4719	Area 8	DAF	2.67	2.394	Admiralties
4720	Area 8	DAF	2.17	2.396	Admiralties
4721	Area 8	DAF	1.97	2.365	Possibly Talasea
4722	Area 11	DAF	0.84	2.392	Admiralties
4723	Area 11	DAF	3.98	2.357	Talasea
4724	Area 11	DAF	3.40	2.394	Admiralties
4725	Area 11	DAF	5.77	2.390	Admiralties
4726	Area 11	DAF	0.69	2.392	Admiralties
4727	Area 11	DAF	1.26	2.393	Admiralties
4728	Area 11	DAF	0.85	2.393	Admiralties
4729	Area 11	DAF	1.07	2.394	Admiralties
4730	Area 11	DAF	1.22	2.401	Admiralties
4731	Area 2	DAF	6.72	2.386	Admiralties
4732	Area 2	DAF	8.63	2.390	Admiralties
4733	Area 2	DAF	7.12	2.457	Anomalous
4734	Area 2	DAF	5.86	2.392	Admiralties
4736	Area 2	DAF	4.55	2.379	Admiralties
4737	Area 2	DAF	6.89	2.393	Admiralties
4738	Area 2	DAF	13.53	2.394	Admiralties
4739	Area 2	DAF	20.88	2.399	Admiralties
4740	Area 2	DAF	6.42	2.396	Admiralties
4741	Area 9	DAF	6.18	2.397	Admiralties
4742	Area 9	DAF	7.25	2.395	Admiralties
4743	Area 9	DAF	4.43	2.363	Talasea
4744	Area 9	DAF	5.18	2.394	Admiralties
4745	Area 9	DAF	5.72	2.405	Admiralties
4746	Area 9	DAF	2.49	2.394	Admiralties
4747	Area 9	DAF	0.58	2.395	Admiralties
4748	Area 9	DAF	3.99	2.390	Admiralties
4749	Area 9	DAF	1.94	2.371	Between1
4750	Area 9	DAF	0.93	2.393	Admiralties
4751	Area 9	DAF	1.82	2.366	Possibly Talasea
4752	Area 9	DAF	1.91	2.390	Admiralties
4753	Area 9	DAF	0.80	2.392	Admiralties
4753a	Area 9	DAF	5.67	2.392	Admiralties
4754	Area 9	DAF	0.25	2.366	Possibly Talasea
4755	Area 9	DAF	1.40	2.394	Admiralties
4756	Area 9	DAF	0.49	2.401	Admiralties
4757	Area 9	DAF	2.53	2.391	Admiralties
4758	Area 9	DAF	1.55	2.365	Possibly Talasea
4759	Area 9	DAF	0.80	2.394	Admiralties
4760	Area 9	DAF	1.64	2.392	Admiralties
4761	Outer Area 3	DAF	5.85	2.395	Admiralties
4762	Outer Area 3	DAF	0.61	2.393	Admiralties
4763	Outer Area 3	DAF	2.65	2.392	Admiralties
4764	Outer Area 3	DAF	5.89	2.395	Admiralties
4765	Area 1	DAF	5.36	2.392	Admiralties
4766	Area 1	DAF	0.43	2.389	Admiralties
4767	Area 1	DAF	0.88	2.396	Admiralties
4768	Area 12	DAF	0.23	2.392	Admiralties

4769	Area 12	DAF	1.40	2.376	Possibly Admiralties
4770	Area 12	DAF	0.42	2.389	Admiralties
4771	Area 12	DAF	0.91	2.397	Admiralties
4772	Area 12	DAF	0.25	2.387	Admiralties
4773	Area 12	DAF	0.41	2.392	Admiralties
4774	Area 12	DAF	0.72	2.401	Admiralties
3418	15 cm	TK-4	0.88	2.420	Banks
3419*	"	"	0.45	2.419	Possibly Banks
3420	0–30 cm	TK-4	0.46	2.406	Possibly Admiralties
3421*	"	"	1.04	2.411	Between2
3422	0–30 cm	TK-4	0.89	2.392	Admiralties
3424	65 cm	TK-4	0.22	2.391	Admiralties
3425*	"	"	0.11	2.392	Admiralties
3426	0–20 cm	TK-1	0.91	2.415	Between2
3427*	"	"	0.34	2.415	Between2
3428*	"	"	0.14	2.412	Between2
3429	0–20 cm	TK-1	0.67	2.404	Possibly Admiralties
3430*	"	"	0.83	2.397	Admiralties
3431	20–40 cm	TK-1	0.82	2.413	Between2
3432	0–25 cm	TK-36	0.03	2.391	Admiralties
3434	65 cm	TK-TP	0.65	2.417	Possibly Banks
3436	75–95 cm	TK-1	1.05	2.423	Banks
3437*	"	"	0.81	2.417	Possibly Banks
3438	100 cm	TK-36	0.78	2.437	Banks
3439*	"	"	0.50	2.434	Banks
3440	195 cm	TK-TP	0.13	2.400	Admiralties

* These eight specimens are the corresponding other portions of the specimen listed directly above—the Tikopia flakes were sectioned into two or three pieces during analysis.
"Below" refers to obsidian with measured density below that expected for New Britain sources.
"Between1" refers to obsidian with measured density between that expected for New Britain sources and Admiralties sources.
"Between2" refers to obsidian with measured density between that expected for Admiralties sources and Banks sources.
"Anomalous" refers to obsidian with measured density that falls within the expected range for Banks sources at locations where Banks obsidian is not believed to occur.

References

Abrajano Jr., Teofilo A., John K. Bates and J. K. Bohlke 1988. Linear free energy relationships in glass corrosion. In Edward V. Sayre, Pamela B. Vandiver, James Druzik and Christopher Stevenson (eds.) *Material Issues in Art and Archaeology*, pp. 253–62. Pittsburgh: MRS (Materials Research Society).

Alden, John R. 1982. Marketplace exchange as indirect distribution. In Jonathon E. Ericson and Timothy K. Earle (eds.) *Contexts for Prehistoric Exchange*, pp. 83–101. New York: Academic Press.

Allen, Jim 1982. Pre-contact trade in Papua New Guinea. In Ronald J. May and Hank Nelson (eds.) *Melanesia: Beyond diversity*, pp. 193–205. Canberra: Australian National University Press.

Allen, Jim 1984. In search of the Lapita homeland: Reconstructing the prehistory of the Bismarck Archipelago. *Journal of Pacific History* 19: 186–201.

Allen, Jim and Chris Gosden 1996. Spheres of interaction and integration: Modelling the culture history of the Bismarck Archipelago. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 183–97. Dunedin North: New Zealand Journal of Archaeology.

Allen, Jim, Chris Gosden and J. Peter White 1989. Human Pleistocene adaptations in the tropical Island Pacific: Recent evidence from New Ireland, a Greater Australian outlier. *Antiquity* 63: 548–61.

Allen, Jim, and J. Peter White 1989. The Lapita homeland: Some new data and an interpretation. *Journal of the Polynesian Society* 98: 129–46.

Ambrose, Wallace R. 1973. Obsidian as an indicator of age and contacts at Ambitle Island. Paper presented at "International Quaternary Association" conference in Christchurch, New Zealand.

Ambrose, Wallace R. 1976a. Obsidian and its prehistoric distribution in Melanesia. In Noel Barnard (ed.) *Ancient Chinese Bronzes and Southeast Asian Metal and other Archaeological Artifacts*, pp. 351–78. Melbourne: National Museum of Victoria.

Ambrose, Wallace R. 1976b. Intrinsic hydration rate dating of obsidian. In R. E. Taylor (ed.) *Advances in Obsidian Glass Studies: Archaeological and geochemical perspectives*, pp. 81–105. Park Ridge, New Jersey: Noyes Press.

Ambrose, Wallace R. 1978. The loneliness of the long distance trader in Melanesia. *Mankind* 11: 326–33.

Ambrose, Wallace R. 1991. Lapita or not Lapita: The case of the Manus pots. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 103–12. Canberra: The Australian National University.

Ambrose, Wallace R. 1994. Obsidian hydration dating of a Pleistocene age site from the Manus Islands, Papua New Guinea. *Quaternary Geochronology* 13: 137–42.

Ambrose, Wallace R. 1996. Obsidian hydration dating of the Reef/Santa Cruz Lapita sites. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 245–55. Dunedin North: New Zealand Journal of Archaeology.

Ambrose, Wallace R. 1998. Obsidian hydration dating at a recent age obsidian mining site in Papua New Guinea. In M. Steven Shackley (ed.) *Archaeological Obsidian Studies, Method and Theory*, pp. 205–22. New York: Plenum.

Ambrose, Wallace R. n.d. A Note on Obsidian Density Measurement for Separating Melanesian Obsidian Sources. Unpublished paper.

Ambrose, Wallace R., J. Roger Bird and Peter Duerden 1981. The impermanence of obsidian sources in Melanesia. In B. Foss Leach and Janet Davidson (eds.) *Archaeological Studies of Pacific Stone Resources*, pp. 1–20. Oxford: British Archaeological Reports.

Ambrose, Wallace R. and Chris Gosden 1991. Investigations on Boduna island. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 182–8. Canberra: The Australian National University.

Ambrose, Wallace R. and Peter Duerden 1982. PIXE analysis in the distribution and chronology of obsidian use in the Admiralty Islands. In Wallace R. Ambrose and Peter Duerden (eds.) *Archaeometry: An Australasian perspective*, pp. 83–9. Canberra: The Australian National University.

Ambrose, Wallace R., Peter Duerden and J. Roger Bird 1981. An archaeological application of PIXE-PIGME analysis to Admiralty Islands obsidians. *Nuclear Instruments and Methods* 191: 397–402.

Ambrose, Wallace R. and R. W. Johnson 1986. Unea: An obsidian non-source in Papua New Guinea. *Journal of the Polynesian Society* 95: 491–7.

Ammerman, Albert J. 1979. A study of obsidian exchange networks in Calabria. *World Archaeology* 11: 95–110.

Ammerman, Albert J. and William Andrefsky Jr. 1982. Reduction sequences and the exchange of obsidian in Neolithic Calabria. In Jonathon E. Ericson and Timothy K. Earle (eds.) *Contexts for Prehistoric Exchange*, pp. 149–71. New York: Academic Press.

Anderson, Patricia C. 1980. A testimony of prehistoric tasks: Diagnostic residues on stone tool working edges. *World Archaeology* 12: 181–93.

Anderson-Gerfaud, Patricia C. 1981. Contribution Méthodologique a l'Analyse des Microtrace d'Utilisation sur les Outils Préhistorique. Unpublished PhD thesis: University of Bordeaux.

Andrefsky Jr., William 1998. *Lithics: Macroscopic approaches to analysis.* Cambridge: Cambridge University Press.

Andrews, Anthony P., Frank Asaro, Helen V. Michel, Fred H. Stross and Pura Cervera Rivero 1989. The obsidian trade at Isla Cerritos, Yucatán, Mexico. *Journal of Field Archaeology* 16: 355–63.

Anson, Dimitri 1983. Lapita Pottery of the Bismarck Archipelago and its Affinities. Unpublished PhD thesis: University of Sydney.

Anson, Dimitri 1986. Lapita Pottery of the Bismarck Archipelago and its Affinities. *Archaeology in Oceania* 21: 157–65.

Aoyama, Kazuo 1996. Exchange, Craft Specialization and Ancient Maya State Formation. Unpublished PhD thesis: University of Pittsburgh.

Avias, Jacques 1950. Poteries canaques et poteries préhistoriques en Nouvelle-Calédonie. *Journal de la Société des Océanistes* 6: 111–141.

Ambrose, Wallace R. and Roger C. Green 1972. First millennium BC transport of obsidian from New Britain to the Solomon Islands. *Nature* 77:31.

Ayres, William S. and Rufino Mauricio 1987. Stone adzes from Pohnpei, Micronesia. *Archaeology in Oceania* 22(1): 27–31.

Ballard, Chris 1993. Stimulating minds to fantasy? A critical etymology for Sahul. In Michael A. Smith, Matthew Spriggs and Barry Fankhauser (eds.) *Sahul in Review: Pleistocene archaeology in Australia, New Guinea and Island Melanesia*, pp. 17–23. Canberra: Australian National University.

Bamforth, Douglas B. 1988. Investigating microwear polishes with blind test: The Institute results in context. *Journal of Archaeological Science* 15: 11–23.

Barton, Huw and J. Peter White 1993. Use of stone and shell artifacts at Balof 2, New Ireland, Papua New Guinea. *Asian Perspectives* 32: 169–81.

Barton, Huw, Robin Torrence and Richard Fullagar 1998. Clues to stone tool function re-examined: Comparing starch grain frequencies on used and unused obsidian artefacts. *Journal of Archaeological Science* 25: 1231–8.

Bates, John K., Teofilo A. Abrajano Jr., William L. Ebert, James J. Mazer and Thomas J. Gerding 1988. Experimental hydration studies of natural and synthetic glasses. In Edward V. Sayre, Pamela B. Vandiver, James Druzik and Christopher Stevenson (eds.) *Material Issues in Art and Archaeology*, pp. 237–44. Pittsburgh: MRS (Materials Research Society).

Baugh, Timothy G. and F. W. Nelson 1987. New Mexico obsidian sources and exchange on the Southern Plains. *Journal of Field Archaeology* 14: 313–29.

Bellwood, Peter 1978. *Man's Conquest of the Pacific: The prehistory of Southeast Asia and Oceania*. Auckland: Collins.

Bellwood, Peter 1985. *Prehistory of the Indo-Malaysian Archipelago*. Sydney: Academic Press.

Bellwood, Peter 1992. New discoveries in Southeast Asia relevant for Melanesian (especially Lapita) prehistory. In Jean Christophe Galipaud (ed.) *Poterie Lapita et Peuplement*, pp. 49–66. Nouméa: Orstom Nouméa.

Bellwood, Peter 1993. Crossing the Wallace Line—with style. In Matthew Spriggs, Douglas E. Yen, Wallace R. Ambrose, Rhys Jones, Alan Thorne and Ann Andrews (eds.) *A Community of Culture: The people and prehistory of the Pacific*, pp. 152–63. Canberra: Australian National University.

Bellwood, Peter and Peter Koon 1989. 'Lapita colonists leave boats unburned!' The question of Lapita links with Island Southeast Asia. *Antiquity* 63: 613–22.

Berrin, Kathleen and Esther Pasztory 1993. (eds.) *Teotihuacan: Art from the City of the Gods*. London: Thames and Hudson.

Best, Simon 1987. Long distance obsidian travel and possible implications for the settlement of Fiji. *Archaeology in Oceania* 22: 31–2.

Bettinger, Robert L. 1982. Aboriginal exchange and territoriality in Owens Valley, California. In Jonathon E. Ericson and Timothy K. Earle (eds.) *Contexts for Prehistoric Exchange*, pp. 103–26. New York: Academic Press.

Binford, Lewis R. 1973. Interassemblage variability—the Mousterian and the "functional" argument. In Colin Renfrew (ed.) *The Explanation of Culture Change: Models in prehistory*, pp. 227–54. London: Duckworth.

Binford, Lewis R. and Sally R. Binford 1966. A preliminary analysis of functional variability in the Mousterian of Levallois facies: "Recent Studies in Paleoanthropology" (edited by J. D. Clark and F. C. Howell) *American Anthropologist* 68: 238–95.

Bird, J. Roger, Wallace R. Ambrose, L. H. Russell and M. D. Scott 1981. *The Characterisation of Melanesian Obsidian Sources and Artefacts using the Proton Induced Gamma-Ray Emission (PIGME) Technique*. Lucas Heights, NSW: Australian Atomic Energy Commission.

Bird, J. Roger, Peter Duerden, Wallace R. Ambrose and B. Foss Leach 1981. Pacific obsidian catalogue. In B. Foss Leach and Janet Davidson (eds.) *Archaeological Studies of Pacific Stone Resources*, pp. 31–44. Oxford: British Archaeological Reports.

Bird, J. Roger, C. Kanneneyer, D. Lavan, N. Shahgholi and M. Van der Valk n.d. Lapita Project—Obsidian Analysis: 1986–7. Unpublished report.

Bird, J. Roger, L. H. Russell, M. D. Scott and Wallace R. Ambrose 1978. Obsidian characterization with elemental analysis by proton induced gamma-ray emission. *Analytical Chemistry* 50: 2082–4.

Bird, J. Roger, Robin Torrence, Glenn R. Summerhayes and Grahame Bailey 1997. New Britain obsidian sources. *Archaeology in Oceania* 32: 61–7.

Boksenbaum, Martin W., P. Tolstoy, G. Harbottle, J. Kimberlin and M. Neivens 1987. Obsidian industries and cultural evolution in the Basin of Mexico before 500 AD. *Journal of Field Archaeology* 14: 65–75.

Bordes, François 1961. *Typologie du Palaeolithic Ancien et Moyen*. Bordeaux: Publications de l'Université de Bordeaux.

Bordes, François 1968. *The Old Stone Age*. New York: McGraw-Hill.

Brézillon, Michel N. 1968. *La Dénomination des Objets de Pierre Taillée*. Paris: CNRS.

Brown, Paula 1978. *Highland Peoples of New Guinea*. Cambridge: Cambridge University Press.

Brown, Penelope D. 1988. Residue Analysis of Stone Artefacts from Yambon, West New Britain. Unpublished BA (Honours) thesis: University of Sydney.

Buchanan, Mark 1997. One law to rule them all. *New Scientist* 156: 30–5.

Cann, J. R. and Colin Renfrew 1964. The characterization of obsidian and its application to the Mediterranean region. *Proceedings of the Prehistoric Society* 30: 111–33.

Cattaneo, C., K. Gelsthorpe, P. Phillips and R. J. Sokal 1993. Blood residues on stone tools: Indoor and outdoor experiments. *World Archaeology* 25(1): 29–43.

Cayley-Webster, H. 1898. *Through New Guinea and the Cannibal Countries*. London: T. Fisher Unwin.

Cleveland, William S. and Robert McGill 1984. Graphical perception: Theory, experimentation, and application to the development of graphical methods. *Journal of the American Statistical Association* 79: 531–54.

Crabtree, Don E. 1972. *An Introduction to Flint Working*. Pocatello, Idaho: Idaho State University Museum.

Culliford, Bryan J. 1971. The examination and typing of bloodstains in the crime laboratory. *National Institute of Law Enforcement and Criminal Justice* 71: 47–58.

Custer, Jay F., John Ilgenfritz and Keith R. Doms 1988. A cautionary note on the use of chemstrips for detection of blood residues on prehistoric stone tools. *Journal of Archaeological Science* 15: 343–5.

Dauvois, Michel 1976. *Precis de Dessin Dynamique et Structural des Industries Lithiques Préhistoriques.* Perigueux: Pierre Fanlac.

Davidson, Janet 1978. Western Polynesia and Fiji: The archaeological evidence. *Mankind* 11: 383–90.

Dayton, Leigh 1994. Neural nets unearth secrets in stone. *New Scientist* April 15: 21.

Dibble, Harold L. 1995. Technological aspects of flake variation. *American Archaeology* 5: 236–40.

Dibble, Harold L. and Andrew W. Pelcin 1995. The effect of hammer mass and velocity on flake mass. *Journal of Archaeological Science* 22: 429–39.

Dibble, Harold L. 1998. Comment on "Quantifying lithic curation: An experimental test of Dibble and Pelcin's original flake-tool mass predictor", by Zachary J. Davis and John J. Shea. *Journal of Archaeological Science* 25: 611–13.

Doonan, William 1996. The Artifacts of Group 10L-2, Copan, Honduras. Unpublished PhD thesis: Tulane University.

Downie, J. E. and J. Peter White 1978. Balof shelter, New Ireland—Report on a small excavation. *Records of The Australian Museum* 31: 762–802.

Du Toit, Brian M. 1975. *Akuna—A New Guinea Village Community.* Rotterdam: A. A. Balkena.

Egloff, Brian 1979. *Recent Prehistory in Southeast Papua.* Canberra: The Australian National University.

Earle, Timothy K. and Jonathon E. Ericson 1977. (eds.) *Exchange Systems in Prehistory.* New York: Academic Press.

Ericson, Jonathon E. 1977. Prehistoric Exchange Systems in California: The results of obsidian dating and tracing. Unpublished PhD thesis: University of California, Los Angeles.

Ericson, Jonathon E. 1982. Production for obsidian exchange in California. In Jonathon E. Ericson and Timothy K. Earle (eds.) *Contexts for Prehistoric Exchange*, pp. 129–47. New York: Academic Press.

Ericson, Jonathon E. 1988. Obsidian hydration rate development. In Edward V. Sayre, Pamela B. Vandiver, James Druzik and Christopher Stevenson (eds.) *Material Issues in Art and Archaeology*, pp. 215–24. Pittsburgh: MRS (Materials Research Society).

Ericson, Jonathon E. and Timothy K. Earle 1982. (eds.) *Contexts for Prehistoric Exchange*. New York: Academic Press.

Ericson, Jonathon E. and Barbara A. Purdy 1984. (eds.) *Prehistoric quarries and lithic production*. Cambridge: Cambridge University Press.

Fiedel, Stuart J. 1996. Blood from stones? Some methodological and interpretive problems in blood residue analysis. *Journal of Archaeological Science* 23: 139–47.

Findlow, Frank J. and Marisa Bolognese 1982. Regional modelling of obsidian procurement in the American Southwest. In Jonathon E. Ericson and Timothy K. Earle (eds.) *Contexts for Prehistoric Exchange*, pp. 53–80. New York: Academic Press.

Fredericksen, Clayton 1987. Stone tools and cultural diversity: The analysis of stone tool assemblage variability in New Zealand archaeology. Unpublished MA thesis: University of Auckland.

Fredericksen, Clayton 1994. Patterns in Glass: Obsidian and economic specialisation in the Admiralty Islands. Unpublished PhD thesis: Australian National University.

Fredericksen, Clayton 1997a. Changes in Admiralty Islands obsidian source use: The view from Pamwak. *Archaeology in Oceania* 32(1): 68–73.

Fredericksen, Clayton 1997b. The maritime distribution of Bismarck Archipelago obsidian and Island Melanesian prehistory. *Journal of the Polynesian Society* 106: 375–93.

Freeman, Derek 1970. *Report on the Iban*. London: The Athlone Press.

Freslov, J. 1989. Structure and Form in an 'Amorphous' Lithic Assemblage: A case study from New Ireland, PNG. Unpublished BA (Honours) thesis: La Trobe University.

Friedman, I. and R. L. Smith 1960. A new dating method using obsidian. *American Antiquity* 25(4): 476–93.

Fullagar, Richard 1991. The role of silica in polish formation. *Journal of Archaeological Science* 18: 1–24.

Fullagar, Richard 1992. Lithically Lapita: Functional analysis of flaked stone assemblages from West New Britain Province, Papua New Guinea. In Jean Christophe Galipaud (ed.) *Poterie Lapita et Peuplement*, pp. 135–43. Nouméa: Orstom Nouméa.

Fullagar, Richard 1993. Taphonomy and tool-use: A role for phytoliths in use-wear and residues analysis. In Barry L. Fankhauser and J. Roger Bird (eds.) *Archaeometry: Current Australasian research*, pp. 21–7. Canberra: The Australian National University.

Fullagar, Richard 1994. Traces of times past: Stone artefacts into prehistory. *Australian Archaeology* 39: 63–73.

Fullagar, Richard, Wallace R. Ambrose, J. Roger Bird, Jim Specht, Robin Torrence and Neville Baker 1989. Stocktaking the rocks: Obsidian sources in West New Britain, Papua New Guinea. Paper presented at "Australian Conference on Nuclear Techniques of Analysis" conference in Lucas Heights, NSW, Australia.

Fullagar, Richard and Judith Field 1997. Pleistocene seed-grinding implements from the Australian arid zone. *Antiquity* 71: 300–7.

Fullagar, Richard, Betty Meehan and Rhys Jones 1992. Residue analysis of ethnographic plant-working and other tools from Northern Australia. In Patricia C. Anderson (ed.) *Préhistoire de l'Agriculture: Nouvelles approches*, pp. 39–53. Paris: CNRS.

Fullagar, Richard, Glenn R. Summerhayes, Baiva Ivuyo and Jim Specht 1991. Obsidian sources at Mopir, West New Britain Province, Papua New Guinea. *Archaeology in Oceania* 26: 110–14.

Golson, Jack 1991. Two sites at Lasigi, New Ireland. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 244–59. Canberra: The Australian National University.

Golson, Jack 1992. The pottery from Lasigi, New Ireland. In Jean Christophe Galipaud (ed.) *Poterie Lapita et Peuplement*, pp. 155–67. Nouméa: Orstom Nouméa.

Gorecki, Paul, Sue Bassett and John Head 1991. A Lapita site at Lamau, New Ireland mainland. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 217–21.

Gosden, Chris 1991a. Long term trends in the colonisation of the Pacific: Putting Lapita in its place. *Indo-Pacific Prehistory Association*, Bulletin 11: 333–8.

Gosden, Chris 1991b. Towards an understanding of the regional archaeological record from the Arawe Islands, West New Britain, Papua New Guinea. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 205–16.

Gosden, Chris 1992. Production systems and the colonization of the Western Pacific. *World Archaeology* 24(1): 55–69.

Gosden, Chris, Jim Allen, Wallace R. Ambrose, Dimitri Anson, Jack Golson, Roger C. Green, Patrick V. Kirch, Ian Lilley, Jim Specht and Matthew Spriggs 1989. Lapita sites of the Bismarck Archipelago. *Antiquity* 63: 561–86.

Gosden, Chris and Nola Robertson 1991. Models for Matenkupkum: Interpreting a late Pleistocene site from southern New Ireland, Papua New Guinea. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project,* pp. 20–45. Canberra: The Australian National University.

Goulding, N. E. 1987. Obsidian from the Bismarck Archipelago: An analysis of flaking technologies. Unpublished BA (Honours) thesis: La Trobe University.

Grace, Roger 1988. *Teach Yourself Microwear Analysis: A guide to the interpretation of the function of stone tools*. Santiago de Compostela: Universidade de Santiago de Compostela, Servicio de Publicaci6ns e Intercambio Cientifico.

Grace, Roger 1989. *Interpreting the Function of Stone Tools: The quantification and computerisation of microwear analysis*. Oxford: British Archaeological Reports.

Grace, Roger 1996. Use-wear analysis: The state of the art. *Archaeometry* 38: 209–29.

Grace, Roger n.d. The 'chaîne opératoire' approach to lithic analysis. Unpublished paper.

Green, Roger C. 1976. Lapita sites in the Santa Cruz group. In Roger C. Green and M. M. Cresswell (eds.) *Southeast Solomon Islands Cultural History: A preliminary survey*, pp. 245–65. Wellington: The Royal Society of New Zealand.

Green, Roger C. 1979. Lapita. In Jesse D. Jennings (ed.) *The Prehistory of Polynesia*, pp. 27–60. Cambridge, Massachusetts: Harvard University Press.

Green, Roger C. 1985. An Interpretation of Source Allocations for Obsidian from Three Lapita Sites in the Reef and Santa Cruz Island Group, Southeast Solomons. Unpublished paper.

Green, Roger C. 1986. Lapita fishing: The evidence of site SE-RF-2 from the Main Reef Islands, Santa Cruz Group, Solomons. In Atholl Anderson (ed.) *Traditional Fishing in the Pacific*, pp. 119–35. Honolulu: Bernice Pauahi Bishop Museum.

Green, Roger C. 1987. Obsidian results from the Lapita sites of the Reef/Santa Cruz Islands. In Wallace R. Ambrose and J. M. J. Mummery (eds.) *Archaeometry: Further Australasian studies*, pp. 239–49. Canberra: The Australian National University.

Green, Roger C. 1989. Lapita people: An introductory context for skeletal materials associated with pottery of this cultural complex. *Records of the Australian Museum* 41: 207–13.

Green, Roger C. 1991a. The Lapita cultural complex: Current evidence and proposed models. In Peter Bellwood (ed.) *Indo-Pacific Prehistory*, pp. 295–305. Canberra: Bulletin of the Indo-Pacific Prehistory Association.

Green, Roger C. 1991b. A reappraisal of the dating for some Lapita sites in the Reef/Santa Cruz group of the Southeast Solomons. *Journal of the Polynesian Society* 100: 197–207.

Green, Roger C. 1994. Changes over time: Recent advances in dating human colonization of the Pacific basin area. In Doug G. Sutton (ed.) *The Origins of the First New Zealanders*, pp. 19–51. Auckland: Auckland University Press.

Green, Roger C. 1974. Sites with Lapita pottery: Importing and voyaging. *Mankind* 9(4): 253–8.

Green, Roger C. 1996. Prehistoric transfers of portable items during the Lapita horizon in Remote Oceania: A review. *Indo-Pacific Prehistory Association*, Bulletin 15(2): 119–30.

Green, Roger C. and Dimitri Anson 1987. The Lapita site of Watom: New evidence from excavations in 1985. *Man and Culture in Oceania* Special Issue 3: 121–31.

Green, Roger C. and Dimitri Anson 1991. The Reber-Rakival Lapita site on Watom. Implications of the 1985 excavations at the SAC and SDI localities. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 170–81. Canberra: The Australian National University.

Green, Roger C., Dimitri Anson and Jim Specht 1989. The SAC burial ground, Watom Island, Papua New Guinea. *Records of the Australian Museum* 41: 215–21.

Green, Roger C. and J. Roger Bird 1989. Fergusson Island obsidian from the D'Entrecasteaux Group in a Lapita site of the Reef Santa Cruz Group. *New Zealand Journal of Archaeology* 11: 87–99.

Green, Roger C. and Patrick V. Kirch 1997. Lapita exchange systems and their Polynesian transformations: Seeking explanatory models. In Marshall I. Weisler (ed.) *Prehistoric Long-Distance Interaction in Oceania: An interdisciplinary approach*, pp. 19–37. Auckland: New Zealand Archaeological Association Publications.

Gurfinkle, D. M. and U. M. Franklin 1988. A study of the feasibility of detecting blood residues on artifacts. *Journal of Archaeological Science* 15: 83–97.

Hage, Per and Frank Harary 1991. *Exchange in Oceania: A graph theoretic analysis.* Oxford: Clarendon Press.

Hall, Jay, Su Higgins and Richard Fullagar 1989. Plant residues on stone tools. In Wendy Beck, Anne Clarke and Lesley Head (eds) *Plants in Australian Archaeology*, pp. 136–60.Tempus 1. St. Lucia, Queensland: The University of Queensland.

Halsey, Allyson 1995. Obsidian Resource Maximisation. A Comparison of Two Lapita Assemblages. Unpublished BA (Honours) thesis: La Trobe University.

Hanslip, Michael D. 1991. A Scrape with Time: Investigations into the development of polish on flint. Unpublished MA thesis: University College London.

Hårdh, Birgitta, Lars Larsson, Deborah Olausson and Rolf Petrie 1988. (eds.) *Trade and Exchange in Prehistory*. Lund: Lunds Universitets Historika Museum.

Harris, Mary-Noelle 1994. Relative Density Source Characterisation of Obsidian From the Bismarck Archipelago. Unpublished BA (Honours) thesis: University of Sydney.

Hartwig, Frederick and Brian E. Dearing 1979. *Exploratory Data Analysis*. Newbury Park, California: Sage Publications.

Hayden, Brian 1979. *Palaeolithic Reflections: Lithic technology and ethnographic excavations among Australian Aborigines*. Canberra: Australian Institute of Aboriginal Studies.

Healan, Dan M., Janet M. Kerley and George J. Bey III 1983. Excavation and preliminary analysis of an obsidian workshop in Tula, Hidalgo, Mexico. *Journal of Field Archaeology* 10: 127–45.

Healy, F., M. Heaton, S. J. Lobb with M. J. Allen, I. M. Fenwick, Roger Grace and R. G. Scaife 1992. Excavations of a Mesolithic site at Thatcham, Berkshire. *Proceedings of the Prehistoric Society* 58: 41–76.

Hiscock, Peter 1983. Stone tools as cultural markers? The last two decades of stone artefact analysis in Australian archaeology. *Australian Archaeology* 16: 48–56.

Hiscock, Peter 1994. Technological responses to risk in Holocene Australia. *Journal of World Prehistory* 8: 267–92.

Hiscock, Peter 1998. Revitalising artefact analysis. In Tim Murray (ed.) *Archaeology of Aboriginal Australia: A reader*. St Leonards, NSW: Allen & Unwin.

Hiscock, Peter in press. Sizing up prehistory. Sample size and the composition of Australian artefact assemblages. *Australian Aboriginal Studies*.

Hiscock, Peter and Chris Clarkson 2000. Analysing Australian stone artefacts: An agenda for the twenty first century. *Australian Archaeology* 50: 98–108.

Hodder, Ian and Clive Orton 1976. *Spatial Analysis in Archaeology*. Cambridge: Cambridge University Press.

Hughes, Ian 1977. *New Guinea Stone Age Trade*. Canberra: Australian National University.

Hunt, Terry L. 1988. Graph theoretic network models for Lapita exchange: A trial application. In Patrick V. Kirch and Terry L. Hunt (eds.) *Archaeology of the Lapita Cultural Complex: A critical review*, pp. 135–55. Seattle: Thomas Burke Memorial Washington State Museum.

Hunt, Terry L. and Michael W. Graves 1990. Some methodological issues of exchange in Oceanic prehistory. *Asian Perspectives* 29: 107–16.

Hurcombe, Linda M. 1992. *Use Wear Analysis and Obsidian: Theory, experiments and results*. Sheffield: J.R. Collis Publications.

Hyland, D., J. Tersak, J. Adovasio and M. Siegel 1990. Identification of the species of origin of the residual blood on lithic material. *American Antiquity* 55: 104–12.

Irwin, Geoffrey 1974. The emergence of a central place in coastal Papuan prehistory: A theoretical approach. *Mankind* 9: 268–72.

Irwin, Geoffrey 1978. The development of Mailu as a specialized trading and manufacturing centre in Papuan prehistory: The causes and the implications. *Mankind* 11: 406–15.

Irwin, Geoffrey 1992. *The Prehistoric Exploration and Colonisation of the Pacific*. Cambridge: Cambridge University Press.

Irwin, Geoffrey and Simon Holdaway 1996. Colonisation, trade and exchange: From Papua to Lapita. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 225–35. Dunedin North: New Zealand Journal of Archaeology.

Irwin-Williams, Cynthia 1977. A network model for the analysis of prehistoric trade. In Timothy K. Earle and Jonathon E. Ericson (eds.) *Exchange Systems in Prehistory*, pp. 141–50. New York: Academic Press.

Jeske, Robert J. 1989. Economies in raw material use by prehistoric hunter-gatherers. In Robin Torrence (ed.) *Time, Energy and Stone Tools*, pp. 34–45. Cambridge: Cambridge University Press.

Jeske, Robert J. and Rochelle Lurie 1993. The archaeological visibility of bipolar technology: An example from the Koster site. *Midcontinental Journal of Archaeology* 18: 131–60.

Kamminga, Johan 1982. *Over The Edge: Functional analysis of Australian stone tools*. St. Lucia, Queensland: University of Queensland.

Kealhofer, Lisa, Robin Torrence and Richard Fullagar 1999. Integrating phytoliths within use-wear/residue studies of stone tools. *Journal of Archaeological Science* 26: 527–46.

Keeley, Lawrence H. 1980. *Experimental Determination of Stone Tool Functions: A microwear analysis*. Chicago: University of Chicago Press.

Kendall, Sir Maurice G. and Alan Stuart 1979. *The Advanced Theory of Statistics*. Fourth Edition. Volume 2—Inference and Relationship. London: Charles Griffen and Company.

Kennedy, Jean 1981. Ahus of another colour? (A comparison of two potting centres in the Admiralty Islands.). *Australian Archaeology* 12: 45–60.

Kennedy, Jean 1997. The loneliness of an obsidian source in Southwest Manus. *Archaeology in Oceania* 32: 85–96.

Kennedy, Jean, François Wadra, Urgei Akon, Roselyne Busasa, Josephine Papah and Mathew Piamnok 1991. Site survey of Southwest Manus: A preliminary report. *Archaeology in Oceania* 26: 114–18.

Key, C. A. 1968. Trace element identification of the source of obsidian in an archaeological site in New Guinea. *Nature* 219: 360.

Key, C. A. 1969. The identification of New Guinea obsidians. *Archaeology and Physical Anthropology in Oceania*. 4: 47–55.

Kirch, Patrick V. 1981. Lapitoid settlements of Futuna and Alofi, Western Polynesia. *Archaeology in Oceania* 16: 127–43.

Kirch, Patrick V. 1987. Lapita and Oceanic cultural origins: Excavations in the Mussau Islands, Bismarck Archipelago, 1985. *Journal of Field Archaeology* 14: 163–80.

Kirch, Patrick V. 1988a. Long-Distance exchange and island colonization: The Lapita case. *Norwegian Archaeological Review* 21(2): 103–17.

Kirch, Patrick V. 1988b. The Talepakemalai Lapita site and Oceanic prehistory. *National Geographic Research* 4: 328–42.

Kirch, Patrick V. 1990. Specialization and exchange in the Lapita complex of Oceania (1600–500 BC). *Asian Perspectives* XXIX: 117–33.

Kirch, Patrick V. 1997. *The Lapita Peoples: Ancestors of the Oceanic World.* Oxford: Blackwell Publishers Inc.

Kirch, Patrick V., Terry L. Hunt, Marshall I. Weisler, Virginia Butler and Melinda S. Allen 1991. Mussau Islands prehistory: Results of the 1985–86 excavations. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 144–63. Canberra: The Australian National University.

Kirch, Patrick V. and Douglas E. Yen 1982. *Tikopia: Prehistory and ecology of a Polynesian outlier.* Honolulu: B.P. Bishop Museum.

Knauss, Kevin G., William L. Bourcier, Kevin D. McKeegan, Celia I. Merxbacher, Son N. Nguyen, Frederick J. Ryerson, David K. Smith, Homer C. Weed and Leon Newton 1989. Dissolution kinetics of a simple analogue nuclear waste glass as a function of pH, time and temperature. In Virginia M. Oversby and Paul W. Brown (eds.) *Materials Research Society Symposium: Scientific basis for nuclear waste management XIII*, pp. 371–81. Boston, Massachusetts, USA: Materials Research Society, Pittsburgh, Pennsylvania.

Kooyman, Brian, Margaret E. Newman and Howard Ceri 1992. Verifying the reliability of blood residue analysis on archaeological tools. *Journal of Archaeological Science* 19: 265–9.

Lawrence, P. and M. J. Meggitt 1965. (eds.) *Gods Ghosts and Men in Melanesia: Some religions of Australian New Guinea and the New Hebrides.* Melbourne: Oxford University Press.

Leach, B. Foss 1985. The source of prehistoric obsidian artefacts from the Polynesian outlier of Taumako in the Solomon Islands. *New Zealand Journal of Archaeology* 7: 117–23.

Leach, B. Foss and S. E. Warren 1981. Neutron activation analysis of New Zealand and Oceanic obsidians: Towards a simple screening technique. In B. Foss Leach and Janet Davidson (eds.) *Archaeological Studies of Pacific Stone Resources*, pp. 151–66. Oxford: British Archaeological Reports.

Levi-Sala, Irene 1984–86. Experimental replication of post-depositional surface modification on flint. *Early Man News* 9–11: 103–9.

Levi-Sala, Irene 1986. Use wear and post-depositional surface modification: A word of caution. *Journal of Archaeological Science* 13: 229–44.

Levi-Sala, Irene 1987. Microscopic analysis of some Mousterian pieces from Kebara: The effect of post depositional processes on functional interpretation. *Mitekufat Haeven* 20: 143–50.

Levi-Sala, Irene 1989. Que peuvent vraiment nous reveler les études microscopiques des artefacts lithiques. *Anthropologie* 93: 643–58.

Lilley, Ian 1986. Prehistoric Exchange in the Vitiaz Strait, Papua New Guinea. Unpublished PhD thesis: Australian National University.

Lilley, Ian 1988. Prehistoric exchange across the Vitiaz Strait, Papua New Guinea. *Current Anthropology* 29: 513–16.

Lilley, Ian 1991a. Lapita and post-Lapita developments in the Vitiaz Straits-West New Britain area. *Indo-Pacific Prehistory Association*, Bulletin 11: 313–22.

Lilley, Ian 1991b. Lapita sites in the Duke of York Islands. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 164–9. Canberra: The Australian National University.

Little, William, H. W. Fowler and Jessie Coulson 1973. *The Shorter Oxford English Dictionary*. Oxford: Oxford University Press.

Lock, Gary and Trevor Harris 1992. Visualizing spatial data: The importance of geographic information systems. In Paul Reilly and Sebastian Rahtz (eds.) *Archaeology and the Information Age: A global perspective*, pp. 81–96. London: Routledge.

Loy, Thomas H. 1990. Prehistoric organic residues: Recent advances in identification, dating, and their antiquity. *Archaeometry* 32: 645–56.

Loy, Thomas H. 1992. Biochemical aspects of prehistoric blood residues and their analysis: A reply to 3 'cautionary notes'. *Journal of Archaeological Science* 19: 43.

Loy, Thomas H. 1993. The artifact as site: An example of the biomolecular analysis of organic residues on prehistoric tools. *World Archaeology* 25(1): 44–63.

Loy, Thomas H. 1998. Blood on the axe. *New Scientist* 159(2151): 40-3.

Loy, Thomas H. and B. L. Hardy 1992. Blood residue analysis of 90000-year-old Stone Tools from Tabin Cave, Israel. *Antiquity* 66: 24–35.

Loy, Thomas H., Rhys Jones, D. E. Nelson, Betty Meehan, John Vogel, John Southon and Richard Cosgrove 1990. Accelerator radiocarbon dating of human blood proteins in pigments from late Pleistocene art sites in Australia. *Antiquity* 64: 110–16.

Loy, Thomas H. and Andrée R. Wood 1989. Blood residue analysis at Çayönü Tepesi, Turkey. *Journal of Field Archaeology* 16: 451–60.

Macintyre, Martha and Jim Allen 1990. Trading for subsistence: The case from the Southern Massim. In Douglas E. Yen and J. M. J. Mummery (eds.) *Pacific Production Systems: Approaches to economic prehistory*, pp. 120–36. Canberra: The Australian National University.

Malinowski, Bronislaw 1961. *Argonauts of the Western Pacific*. New York: E. P. Dutton and Co., Inc.

Marshall, Brendon and Jim Allen 1991. Excavations at Panakiwuk Cave, New Ireland. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 59–91. Canberra: The Australian National University.

McCoy, Patrick C. and Paul L. Cleghorn 1988. Archaeological excavations on Santa Cruz (Nendö), Southeast Solomon Islands: Summary report. *Archaeology in Oceania* 23: 104–15.

McEldowney, Holly 1994. Partial Report on the Andra Island Excavations, Manus Province. Unpublished report written at the Australian National University.

McEldowney, Holly and Chris Ballard 1991. The Mouk Island site: Manus as paradox or parable in reconstructions of the Lapita cultural complex? In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 92–102. Canberra: The Australian National University.

Mead, Margaret 1950. *Male and Female: A study of the sexes in a changing world.* London: Victor Gollancz Ltd.

Meehan, Betty and Rhys Jones 1988. (eds.) *Archaeology with Ethnography: An Australian perspective*. Canberra: The Australian National University.

Mendenhall, William and Robert J. Beaver 1991. *Introduction to Probability and Statistics*. Eighth Edition. Boston: PWS-Kent Publishing.

Meyer, O. 1909. Funde Prähistoricher Töpferei und Steinmesser auf Vuatom, Bismarck-Archipel. *Anthropos* 4: 251–2.

Miller, D. 1979. *National Sites Survey Summary Report*. Honiara: National Museum.

Nelson, D. E. 1993. Second thoughts on a rock-art date. *Antiquity* 67: 893–5.

Newcomer, Mark, Roger Grace and Romana Unger-Hamilton 1986. Investigating microwear polishes with blind tests. *Journal of Archaeological Science* 13: 203–17.

Newcomer, Mark, Roger Grace and Romana Unger-Hamilton 1988. Microwear methodology: A reply to Moss, Hurcombe and Bamforth. *Journal of Archaeological Science* 15: 25–33.

Newman, Jay R. 1994. The effects of distance on lithic material reduction technology. *Journal of Field Archaeology* 21: 491–501.

Newman, Margaret E., R. M. Yohe II, Brian Kooyman, and Howard Ceri 1997. "Blood" from stones? Probably: A response to Fiedel. *Journal of Archaeological Science* 24: 1023–7.

Odell, George H. 1994. The role of stone bladelets in Middle Woodland society. *American Antiquity* 59(1): 102–20.

Odell, George H. 1996. Introduction. In George H. Odell (ed.) *Stone Tools: Theoretical insights into human prehistory*, pp. 1–5. New York: Plenum.

Parkinson, Richard 1999. *Thirty Years in the South Seas*. Bathurst, NSW: Crawford House Publishing.

Patterson, L. and J. Sollberger 1976. The myth of bipolar flaking industries. *Lithic Technology* 5: 40–2.

Pelcin, Andrew W. 1997a. The effect of indentor type on flake attributes: Evidence from a controlled experiment. *Journal of Archaeological Science* 24: 613–21.

Pelcin, Andrew W. 1997b. The effect of core surface morphology on flake attributes: Evidence from a controlled experiment. *Journal of Archaeological Science* 24: 749–56.

Pelcin, Andrew W. 1997c. The formation of flakes: The role of platform thickness and exterior platform angle in the production of flake initiations and terminations. *Journal of Archaeological Science* 24: 1107–13.

Pelcin, Andrew W. 1998. The threshold effect of platform width: A reply to Davis and Shea. *Journal of Archaeological Science* 25: 615–20.

Pelegrin, Jacques, Claudine Karlin and Pierre Bodu 1988. "Chaînes opératoires": Un outil pour le préhistorien. In Jacques Tixier (ed.) *Journée d'Études Technologiques en Préhistoire*. Paris: CNRS.

Piel-Desruisseaux, Jean-Luc 1986. *Outils Préhistorique: Forme, fabrication, utilisation*. Paris: Masson.

Piroutet, Maurice 1917. *Étude Stratigraphique sur la Nouvelle-Calédonie*. Maçon: Impremeire Protat Frères.

Plog, Fred 1977. Modeling economic exchange. In Timothy K. Earle and Jonathon E. Ericson (eds.) *Exchange Systems in Prehistory*, pp. 127–39. New York: Academic Press.

Powdermaker, Hortense 1933. *Life in Lesu: The study of a Melanesian society in New Ireland*. New York: W. W. Morton and Co., Inc.

Radcliffe-Brown, A. R. 1922. *The Andaman Islanders*. New York: The Free Press.

Rathje, William L. 1978. Melanesian and Australian exchange systems: A view from Mesoamerica. *Mankind* 11: 165–74.

Rees, D., G. G. Wilkinson, Roger Grace and Clive R. Orton 1991. Investigation of the fractal properties of flint microwear images. *Journal of Archaeological Science* 18: 629–40.

Reeve, R. 1989. Recent work on the prehistory of the Western Solomons, Melanesia. *Indo-Pacific Prehistory Association*, Bulletin 9: 46–67.

Reilly, Paul and Sebastian Rahtz 1992. (eds.) *Archaeology and the information age: A global perspective*. London: Routledge.

Renfrew, Colin, J. E. Dixon and J. R. Cann 1966. Obsidian and early cultural contact in the Near East. *Proceedings of the Prehistoric Society* 32: 30–72.

Renfrew, Colin, J. E. Dixon and J. R. Cann 1968. Further analysis of Near Eastern obsidian. *Proceedings of the Prehistoric Society* 34: 319–31.

Renfrew, Colin 1975. Trade as action at a distance: Questions of integration and communication. In J. Sabloff and C. Lamberg-Karlovsky (eds.) *Ancient Civilization and Trade*, pp. 3–59. Albuquerque: University of New Mexico Press.

Renfrew, Colin 1977. Alternative models for exchange and spatial distribution. In Timothy K. Earle and Jonathon E. Ericson (eds.) *Exchange Systems in Prehistory*, pp. 71–89. New York: Academic Press.

Renfrew, Colin 1984. *Approaches to Social Archaeology*. Cambridge, Massachusetts: Harvard University Press.

Rhoads, James W. and Douglas E. Mackenzie 1991. Stone axe trade in prehistoric Papua: The travels of Python. *Proceedings of the Prehistoric Society* 57: 35–49.

Richards, Thomas H. 1988. *Microwear Patterns of Experimental Basalt Tools*. Oxford: British Archaeological Reports.

Rosenfeld, Andrée 1997. Excavation at Buang Merabak, Central New Ireland. *Indo-Pacific Prehistory Association*, Bulletin 16(3): 213–24.

Sackett, James R. 1982. Approaches to style in lithic archaeology. *Journal of Anthropological Archaeology* 1: 59–112.

Sand, Christophe and Peter J. Sheppard 2000. Long distance prehistoric obsidian imports in New Caledonia: Characteristics and meaning. *Earth and Planetary Sciences* 331: 235–43.

Schick, Kathy D. 1991. On making behavioral inferences from early archaeological sites. In J. Desmond Clark (ed.) *Cultural Beginnings: Approaches to understanding early hominid life-ways in the African savanna*, pp. 79–107. Bonn: R. Habelt.

Schulz, R. L., D. E. Clark and D. C. Folz 1992. Theories of corrosion as applied to preservation of archaeological ceramics. In Pamela B. Vandiver, James R. Druzik, George Segan Wheeler and Ian C. Freestone (eds.) *Material Issues in Art and Archaeology III*, pp. 1015–30. Pittsburgh: MRS (Materials Research Society).

Schurig, M. 1930. *Die Südseetöpferei*. Leipzig: Schindler.

Semenov, S. A. 1964. *Prehistoric Technology*. Bath: Adams and Dart.

Serjeantson, S. W. and A. V. S. Hill 1989. The colonization of the Pacific: The genetic evidence. In A. V. S. Hill and S. W. Serjeantson (eds.) *The Colonization of the Pacific: A genetic trail*, pp. 286–94. Oxford: Clarendon Press.

Shackley, M. Steven 1987. Comment on "Tomato Springs: The identification of a jasper trade and production center in southern California". *American Antiquity* 52: 616–23.

Shackley, M. Steven 1992. The Upper Gila River gravels as an archaeological obsidian source region: Implications for models of exchange and interaction. *Geoarchaeology* 7: 315–26.

Shackley, M. Steven 1995. Sources of archaeological obsidian in the greater American Southwest: An update and quantitative analysis. *American Antiquity* 60: 531–51.

Shackley, M. Steven 1998a. Gamma rays, X-rays and stone tools: Some recent advances in archaeological geochemistry. *Journal of Archaeological Science* 25: 259–70.

Shackley, M. Steven 1998b. Geochemical differentiation and prehistoric procurement of obsidian in the Mount Taylor Volcanic Field, northwest New Mexico. *Journal of Archaeological Science* 25: 1073–82.

Shackley, M. Steven 1998c. Intrasource chemical variability and secondary depositional processes: Lessons from the American Southwest. In M. Steven Shackley (ed.) *Archaeological Obsidian Studies*. New York: Plenum Press.

Shea, John J. 1993. Lithic use-wear evidence for hunting by Neandertals and early Modern Humans from the Levantine Mousterian. In Gail Larsen Peterkin, Harvey M. Bricker and Paul Mellars (eds.) *Hunting and Animal Exploitation in the Later Palaeolithic and Mesolithic of Eurasia*, pp. 189–97. Archaeological Papers of the American Anthropological Association.

Shea, John J. and Joel D. Klenck 1993. An experimental investigation of the effects of trampling on the results of lithic microwear analysis. *Journal of Archaeological Science* 20: 175–94.

Sheppard, Peter J. 1992. A report on the flaked lithic assemblages from three Southeast Solomons Lapita sites (SE-SZ-8, SE-RF-2, SE-RF-6). In Jean Christophe Galipaud (ed.) *Poterie Lapita et Peuplement*, pp. 145–53. Nouméa: Orstom Nouméa.

Sheppard, Peter J. 1993. Lapita lithics: Trade/exchange and technology: A view from the Reefs/Santa Cruz. *Archaeology in Oceania* 28: 121–37.

Sheppard, Peter J. and Roger C. Green 1991. Spatial analysis of the Nenumbo (SE-RF-2) Lapita site, Solomon Islands. *Archaeology in Oceania* 26: 89–101.

Sheppard, Peter J. and Larry A. Pavlish 1992. Weathering of archaeological cherts: A case study from the Solomon Islands. *Geoarchaeology* 7: 41–53.

Sheppard, Peter J. and Christophe Sand 2000. Through distant glass: Lapita obsidian from New Caledonia. Paper presented at The Fourth Lapita Conference: "Prehistory of the western and central Pacific" in Canberra, Australia.

Shott, Michael J. 1989. Diversity, organization, and behavior in the material record. *Current Anthropology* 30(3): 283-315.

Shott, Michael J. 1998. Status and role of formation theory in contemporary archaeological practice. *Journal of Archaeological Research* 6(4): 299-329.

Shutler Jr., Richard and Jeffrey C. Marck 1975. On the dispersal of the Austronesian horticulturalists. *Archaeology and Physical Anthropology in Oceania* 10(2): 81–113.

Sidrys, Raymond 1977. Mass-distance measures for Maya obsidian trade. In Timothy K. Earle and Jonathon E. Ericson (eds.) *Exchange Systems in Prehistory*, pp. 91–105. New York: Academic Press.

Siegal, Andrew F. 1988. *Statistics and Data Analysis: An introduction.* New York: John Wiley and Sons.

Singer, Clay A. and Jonathon E. Ericson 1977. Quarry analysis at Bodie Hills, Mono County, California: A case study. In Timothy K. Earle and Jonathon E. Ericson (eds.) *Exchange Systems in Prehistory*, pp. 171–87. New York: Academic Press.

Smith, E. E. M., G. K. Ward and Wallace R. Ambrose 1977. Geographic distribution and the characterization of volcanic glasses in Oceania. *Archaeology and Physical Anthropology in Oceania* XII: 173–201.

Smith, P. R. and M. T. Wilson 1992. Blood residues on ancient tool surfaces: A cautionary note. *Journal of Archaeological Science* 19: 237–41.

Sobolik, Kristin D. 1996. Lithic organic residue analysis: An example from the southwestern Archaic. *Journal of Field Archaeology* 23: 461–9.

Specht, Jim 1967a. A prehistoric pottery site in coastal New Guinea. *Antiquity* 41: 229–30.

Specht, Jim 1967b. "Lapita-style" pottery and Watom Island. *South Pacific Bulletin* 17(2): 29–31.

Specht, Jim 1968. Preliminary report of excavations on Watom Island. *Journal of the Polynesian Society* 77: 117–34.

Specht, Jim 1981. Obsidian sources at Talasea, West New Britain, Papua New Guinea. *Journal of the Polynesian Society* 90: 337–56.

Specht, Jim 1993. Watom Island. Unpublished report.

Specht, Jim, Richard Fullagar, Robin Torrence and Neville Baker 1988. Prehistoric obsidian exchange in Melanesia: A perspective from the Talasea sources. *Australian Archaeology* 27: 3–16.

Specht, Jim and Julian D. Hollis 1982. A new obsidian source in West New Britain, Papua New Guinea. *Mankind* 13: 424–7.

Specht, Jim and M. Koettig 1981. An obsidian flaking area near Talasea, West New Britain, Papua New Guinea. *Archaeology in Oceania* 16: 168–73.

Spence, Michael W. 1982. The social context of production and exchange. In Jonathon E. Ericson and Timothy K. Earle (eds.) *Contexts for Prehistoric Exchange*, pp. 173–92. New York: Academic Press.

Spriggs, Matthew 1990. Dating Lapita: Another view. In Matthew Spriggs (ed.) *Lapita Design, Form, & Composition*, pp. 6–27. Canberra: The Australian National University.

Spriggs, Matthew 1991. Nissan, the island in the middle. Summary report on excavations at the north end of the Solomons and the south end of the Bismarcks. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 222–43. Canberra: The Australian National University.

Spriggs, Matthew 1996a. What is Southeast Asian about Lapita? In Takeru Akazawa and Emőke J. E. Szathmáry (eds.) *Prehistoric Mongoloid Dispersals*, pp. 324–48. Oxford: Oxford University Press.

Spriggs, Matthew 1996b. Chronology and colonisation in Island Southeast Asia and the Pacific: New data and an evaluation. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 33–50. Dunedin North: New Zealand Journal of Archaeology.

Spriggs, Matthew 1997. *The Island Melanesians*. Oxford: Blackwell Publishers Inc.

Stuart, Alan and J. Keith Ord 1987. *Kendall's Advanced Theory of Statistics*. Fifth Edition. Volume 1—Distribution Theory. New York: Oxford University Press.

Stuiver, Minze and Paula J. Reimer 1993. Extended ^{14}C data base and revised CALIB 3.0 age calibration program. *Radiocarbon* 35: 215–30.

Summerhayes, Glenn R. and Jim Allen 1993. The transport of Mopir obsidian to late Pleistocene New Ireland. *Archaeology in Oceania* 28: 144–8.

Summerhayes, Glenn R., J. Roger Bird, Richard Fullagar, Chris Gosden, Jim Specht and Robin Torrence 1998. Application of PIXE-PIGME to archaeological analysis of changing patterns of obsidian use in West New Britain, Papua New Guinea. In M. Steven Shackley (ed.) *Advances in Archaeological Volcanic Glass Studies*, pp. 129–58. New York: Plenum Press.

Summerhayes, Glenn R., Chris Gosden, Richard Fullagar, Jim Specht, Robin Torrence, J. Roger Bird, N. Shahgholi and A. Katsaros 1993. West New Britain obsidian: Production and consumption patterns. In Barry L. Fankhauser and J. Roger Bird (eds.) *Archaeometry: Current Australasian research*, pp. 57–68. Canberra: The Australian National University.

Summerhayes, Glenn R. and M. Hotchkis 1992. Recent advances in Melanesian obsidian sourcing: Results of the 1990 and 1991 PIXE/PIGME analyses. In Jean Christophe Galipaud (ed.) *Poterie Lapita et Peuplement*, pp. 127–34. Nouméa: Orstom Nouméa.

Sussman, Carole 1988. *A Microscopic Analysis of Use-Wear and Polish Formation on Experimental Quartz Tools*. Oxford: British Archaeological Reports.

Swadling, Pamela 1990. Sepik prehistory. In Nancy Lutkehaus, Christian Kaufmann, William E. Mitchell, Douglas Newton, Lita Osmundsen, Meinard Schuster (eds.) *Sepik Heritage: Tradition and change in Papua New Guinea*, pp. 71–86. Durham, North Carolina: Carolina Academic Press.

Swadling, Pamela 1996. The distribution of Lapita sites and coral reef resources in the Solomon Islands. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 237–9. Dunedin North: New Zealand Journal of Archaeology.

Terrell, John E. 1989. Commentary: What Lapita is and what Lapita isn't. *Antiquity* 63: 623–6.

Terrell, John E. 1996. Lapita as history and culture hero. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 51–66. Dunedin North: New Zealand Journal of Archaeology.

Terrell, John E. and Robert L. Welsch 1997. Lapita and the temporal geography of prehistory. *Antiquity* 71: 548–72.

Tickle, Les 1977. *Taukuka: A tattooing of the people of Bellona Island*. Cultural Association of the Solomon Islands.

Tixier, Jacques 1963. *Typologie de l'Épipaléolithique du Maghreb*. Paris: Arts et Métiers Graphiques.

Tixier, Jacques, M. L. Inizam and H. Roche 1980. *Préhistoire de la Pierre Taillée*. Antibes: Cercle de Recherches et d'Études Préhistoriques.

Torrence, Robin 1984. Monopoly or direct access? Industrial organization at the Melos obsidian quarries. In Jonathon E. Ericson and B. Purdy (eds.) *Prehistoric Quarries and Lithic Production*, pp. 49–64. Cambridge: Cambridge University Press.

Torrence, Robin 1986. *Production and Exchange of Stone Tools: Prehistoric obsidian in the Aegean*. Cambridge: Cambridge University Press.

Torrence, Robin 1989a. (ed.) *Time, Energy and Stone Tools*, Cambridge: Cambridge University Press.

Torrence, Robin 1989b. Retooling: Towards a behavioral theory of stone tools. In Robin Torrence (ed.) *Time, Energy and Stone Tools*, pp. 57–66. Cambridge: Cambridge University Press.

Torrence, Robin 1992. What is Lapita about obsidian? A view from the Talasea sources. In Jean Christophe Galipaud (ed.) *Poterie Lapita et Peuplement*, pp. 111–26. Nouméa: Orstom Nouméa.

Torrence, Robin 1993. Ethnoarchaeology, museum collections and prehistoric exchange: Obsidian-tipped artifacts from the Admiralty Islands. *World Archaeology* 24: 467–81.

Torrence, Robin, Jim Specht, Richard Fullagar and J. Roger Bird 1992. From Pleistocene to present: Obsidian sources in West New Britain, Papua New Guinea. *Records of the Australian Museum* Supplement 15: 83–98.

Torrence, Robin, Jim Specht, Richard Fullagar and Glenn R. Summerhayes 1996. Which obsidian is worth it? A view from the West New Britain sources. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 211–24. Dunedin North: New Zealand Journal of Archaeology.

Torrence, Robin and Glenn R. Summerhayes 1997. Sociality and the short distance trader: Intra-regional obsidian exchange in the Willaumez region, Papua New Guinea. *Archaeology in Oceania* 32: 74–84.

Torrence, Robin and K. Laurie Victor 1995. The relativity of density. *Archaeology in Oceania* 30: 121–31.

Tykot, Robert H. and Albert J. Ammerman 1997. New directions in Central Mediterranean obsidian studies. *Antiquity* 71: 1000–6.

Tykot, Robert H. and S. Chia 1997. Long-Distance obsidian trade in Indonesia. In James R. Druzik, J. Merkel, J. Stewart and Pamela B. Vandiver (eds.) *Material Issues in Art and Archaeology V*. Pittsburgh: MRS (Materials Research Society).

Watson, Viginia D. 1986. *Obsidian as Tool and Trade: A Papua New Guinea Case.* Seattle: Thomas Burke Memorial Washington State Museum.

Weigand, Phil C., Garman Harbottle and Edward V. Sayre 1977. Turquoise sources and source analysis: Mesoamerica and the Southwestern USA. In Timothy K. Earle and Jonathon E. Ericson (eds.) *Exchange Systems in Prehistory*, pp. 15–32. New York: Academic Press.

White, J. Peter 1972. Carbon dates from New Ireland. *Mankind* 8: 309–10.

White, J. Peter 1996. Rocks in the head: Thinking about the distribution of obsidian in Near Oceania. In Janet Davidson, Geoff Irwin, B. Foss Leach, Andrew Pawley and Dorothy Brown (eds.) *Oceanic Culture History*, pp. 199–209. Dunedin North: New Zealand Journal of Archaeology.

White, J. Peter 1997. Archaeology in New Ireland. *Journal de la Société des Océanistes* 105: 141–6.

White, J. Peter, Jim Allen and Jim Specht 1988. Peopling the Pacific: The Lapita homeland project. *Australian Natural History* 22: 410–16.

White, J. Peter and J. E. Downie 1980. Excavations at Lesu, New Ireland. *Asian Perspectives* 23: 194–220.

White, J. Peter, J. E. Downie and Wallace R. Ambrose 1978. Mid-Recent Human Occupation and Resource Exploitation in the Bismarck Archipelago. *Science* 199: 877–9.

White, J. Peter, T. F. Flannery, R. O'Brien, R. V. Hancock and L. Pavlish 1991. The Balof shelters, New Ireland. In Jim Allen and Chris Gosden (eds.) *Report of the Lapita Homeland Project*, pp. 46–58. Canberra: The Australian National University.

White, J. Peter and Mary-Noel Harris 1997. Changing sources: Early Lapita period obsidian in the Bismarck Archipelago. *Archaeology in Oceania* 32: 97–107.

White, William B. 1988. Glass hydration mechanisms with application to obsidian hydration dating. In Edward V. Sayre, Pamela B. Vandiver, James Druzik and Christopher Stevenson (eds.) *Material Issues in Art and Archaeology*, pp. 225–36. Pittsburgh: MRS (Materials Research Society).

Wickler, Stephen 1990. Prehistoric Melanesian exchange and interaction: Recent evidence from the Northern Solomon Islands. *Asian Perspectives* 29: 135–54.

Wickler, Stephen 1995. Twenty Nine thousand years on Buka: Long-Term cultural change in the Northern Solomon Islands. Unpublished PhD thesis: University of Hawaii.

Wickler, Stephen and Matthew Spriggs 1988. Pleistocene human occupation of the Solomon Islands, Melanesia. *Antiquity* 62: 703–6.

Widmer, Randolf 1993. Obsidian Use-Wear Patterns and Craft Activities at the Classic Maya Site of Copan. Paper presented at the 58th Society for American Archaeology meeting in St. Louis, Missouri, USA.

Willoughby, Pamela R. 1987. *Spheroids and Battered Stones in the African Early and Middle Stone Age*. Oxford: British Archaeological Reports.

Wright, Richard V. S. 1977. (ed.) *Stone Tools as Cultural Markers: Change, evolution and complexity*. Canberra: Australian Institute of Aboriginal Studies.

Yamada, Shoh 1992. A basic study of use-wear "Polish" on early rice paddy farming tools. In Pamela B. Vandiver, James R. Druzik, George Segan Wheeler and Ian C. Freestone (eds.) *Material Issues in Art and Archaeology III*, pp. 806–16. Pittsburgh: MRS (Materials Research Society).

Yamada, Shoh and A. Sawada 1990. The method of description for polished surfaces. Paper presented at "Les Gestes Retrouvés: Traces et fonction" conference in Liège, Belgium.

Young, Lisa C. 1994. Lithics and adaptive diversity: An examination of limited-activity sites in northeast Arizona. *Journal of Anthropological Research* 50: 141-54.