CHAPTER 1

Defining the Problem

> Pattern recognition is a basic methodological approach ... Without quantification, however, there can be no explicit pattern recognition. Without pattern recognition, there can be no archaeological science. Without archaeological science, our ideas about man's past cannot be predictably tested, which is the basic goal of archaeology. Without predictability, man's ideas about the past amount to antiquarianism. Therefore *pattern recognition and quantification are basic to the archaeological process*. These are, however, merely the first steps in that process, but archaeologists must take them before they can ever hope to contribute through their work to a science of archaeology.
>
> (South 1977: xiv; emphasis mine)

The aim of this thesis is to examine one aspect of pattern recognition and quantification. This is pattern recognition and quantification of butchery practice in faunal remains from Australian historical archaeological sites. It is the intention of this thesis to question existing methodology in faunal analysis and provide an alternative analysis of butchery practices as evident by historical archaeological faunal remains. This is perceived as being necessary in order to enable more accurate and meaningful statements to be made concerning the utilisation of domestic animals by past human groups and to enable inter- and intra-site comparisons. It will be demonstrated that the analysis of fauna from historical sites in Australia has so far been crude, and that the results from those analyses which have taken place do not lend themselves to inter-site studies. Inter-site studies are seen as essential if the potential of faunal remains to contribute to Australian history and archaeology are to be realised.

The potential usefulness of faunal remains as evidence of socioeconomic and ethnic conditions in historical archaeology is just beginning to be investigated. The impetus in the development of techniques used in the faunal analysis of historical assemblages has come primarily from Britain and North America, where faunal studies have become an integral part of site reports. In North America a variety of approaches have been used in answering the questions posed by the investigator. Variation in method has revolved around the question of how best to quantify the results of identification. This has resulted in a degree of confusion amongst archaeologists using the results of these analyses, especially in the area of inter-site comparisons. Until an appropriate methodology for analysing results of faunal analyses is developed, so that inter-site comparisons can be made, there will be little advancement in faunal studies relating to both historical and prehistoric sites. To date, most methods concerned with analysing faunal remains have involved methods using number or weight of fragments, or else calculation of animal minimum numbers.

In 1979, Lee Lyman wrote a paper in *American Antiquity* concerned with estimating consumed meat from an historical faunal assemblage dated to 1903. The faunal remains he analysed were excavated from Fort Walla Walla in southeastern Washington. The paper had the intention of pointing out to archaeologists and faunal analysts that it is illogical to assume that entire animals are consumed if entire animals are not represented in the sample. As Lyman (1979: 539) put it 'four left cow femurs represent four individual cows but not necessarily 2,000 lb of meat.' Lyman proposed a new technique which involved the explicit delineation of butchering techniques as evidenced by the bones in the faunal assemblage under analysis. This exercise was seen as providing an insight into those parts of the animal body which were used and those parts which were discarded, in an attempt to define 'butchering units'. A butchering unit was defined as that 'piece of the animal body that results from the act of butchering' (Lyman 1979: 539). Lyman viewed this unit as being more useful for study than that of animal minimum numbers because consumption is of butchering units, and not of complete animals. Butchering units were seen as providing 'more accurate estimates of consumed meat and thus more accurate calculations of food energy' and as different units command higher or lower socio-economic status, butchering units were also seen as socio-economic indicators. Different units are sold for different prices and have higher or lower associated prestige. For example, in the contemporary Australian situation, a scotch-fillet is priced higher, and has higher prestige associated with it than does chuck steak. Butchering units are therefore seen as an indicator of the socio-economic level of the people who deposited the remains under analysis.

Lyman applied his butchering unit method to the cattle, sheep and pig remains from the Fort Walla Walla site. The first step in this type of analysis is to define the butchering units one is going to use. The units Lyman developed for sheep can be seen in Table 1·1. The next step is to calculate a consumable meat weight figure for each of the butchering units, in order to quantify the faunal remains. Lyman achieved his meat weight figures by consulting historical documents. The final step in this approach is to calculate the number of examples of each butchering unit represented by the faunal remains and to multiple this number by the consumable meat weight figure for the specific butchering unit in each case. By totalling up the figures for each butchering unit, a total consumable meat weight for each species can then be calculated. There are, however, problems involved with the calculation of meat weights, which will be discussed in Chapter 4. The results Lyman arrived at were based on the theoretical assumption that you can equate a specific bone or specific set of bones with specific butchering units. It is primarily this assumption which is the concern of this thesis.

Table 1·1: Lyman's butchering units for sheep defined skeletally

BUTCHERY UNIT	SKELETAL DEFINITION
CHUCK	CERVICAL VERTEBRAE, SCAPULA, THORACIC VERTEBRAE 1-5, RIB 1-5, PROXIMAL HUMERUS AND SHAFT
FORESHANK	DISTAL HUMERUS, RADIUS-ULNA, METACARPAL
BRISKET	STERNUM, VENTRAL RIB 1-5
BREAST	RIB CARTILAGE, VENTRAL RIB 6-12
SHORT/HOTEL RACK	THORACIC VERTEBRAE 6-12, DORSAL RIB 6-12, LUMBAR VERTEBRAE
LOIN	LUMBAR VERTEBRAE
FLANK	NO BONES (ASSUME SAME NUMBER AS LOIN)
LEG	PELVIS, SACRUM, FEMUR, TIBIA, METATARSAL, TARSAL, PATELLA

During the mid 1980s work was being conducted in Central Otago, New Zealand, on attempting to derive butchering units for the three main domesticates, cattle, sheep and pigs, for Chinese and European historical faunal assemblages which had been excavated (Piper 1984; 1988). This work proved to be unsuccessful. First attempts at deriving butchering units involved modifying Lyman's units to the New Zealand situation. This was done by consulting nineteenth century British and New Zealand cook books which had diagrams and text on how to butcher various domesticates, and also from discussions with a number of elderly butchers. However, it soon became evident from the butchery marks exhibited on the archaeological bones, that they did not fit the hypothetical units. The location of butchery marks on the archaeological specimens was used in the next attempt to devise butchering units. After the examination of the assemblages from a number of sites, some in excess of 5000 individual bones or bone fragments, no pattern to the location of butchery marks (which according to Lyman indicate the butchering units) could be defined. There simply was no statistically significant repetition of butchery marks in the same location for the same bone, which would enable the definition of butchering units for the assemblage under consideration. As a result of these studies in New Zealand, it was concluded that it was very dubious to assign cattle, pig and sheep bones to specific butchering units which could equate to meat weight, food energy and socioeconomic status.

It is therefore proposed that Lyman's method produces extremely inaccurate results for two reasons. These two reasons relate to variation in butchery practice which is sufficient to invalidate Lyman's method. Firstly, variation is perceived to result from functional reasons: that is, the reason why an animal is butchered will determine the butchery pattern and the resultant butchering units. For example, in the case of sheep, the animal may be butchered for breakfast and lunchtime eating, in which case it is likely to be cut almost entirely into chops. Alternatively it may be butchered for main meals, in which case it is likely to be cut into roasting joints. In the case of pigs, the animal may be butchered for bacon or for hams or for roasting joints. Each of these different reasons will result in different butchering units, which may be represented in the same site. The second reason for inaccuracy results from idiosyncratic behaviour on the part of the butcher. Even highly experienced butchers put cuts in slightly different places on different occasions, due to a number of factors such as variation in muscle control, size of carcass, and how hurried the butcher is. It is therefore difficult to decide when variation in cut placement or orientation results from such idiosyncratic behaviour and when such variation is functionally significant. This brings subjectivity into an area of analysis which, if it is to produce consistently reliable results, requires the rigour of objectivity.

The butchering unit method only works if the butchery practice is standardised. Ethnoarchaeological, documentary and archaeological data will be used to test whether or not this is the case. If butchery practices are shown not to be standardised, then the data will be used to propose an alternative method for quantifying faunal remains from Australian historical archaeological sites, which could be applicable not only to those sites but to archaeological sites in general.

The ethnoarchaeological data used is that of contemporary butchering practices in Australia. This data was collected between October 1986 and December 1987. Kramer (1979: 4) has stated that 'Like much ethnographic fieldwork, ethnoarchaeological research often involves the use of participant observation and interviewing.' Both of these techniques have been used in obtaining the ethnoarchaeological data used in this thesis. Interviews were primarily used in recording contemporary butchering practices but wherever possible the actual butchering of animals was directly observed and recorded while assisting the butchery in most instances. By butchering is meant the cutting of the animal carcass into units to be used in cooking. The information recorded falls into four categories:

1. The actual placement of cuts, whether made by knives, cleavers, axes, chainsaws, mechanical meat saws or hand saws, and the names given to each of the resulting units.

2. Any reasons why an animal may be butchered in different ways and what these ways would be.

3. The culinary practices (the cooking procedures) performed on each of the butchered units.

4. Any other relevant information.

This data was collected from areas where modern technological innovations (such as mechanical meat saws and refrigeration), which may markedly alter nineteenth-century butchering practices, were either recent innovations, in which case the informants were able to state the changes that these have brought about in butchering practices, or else they were absent. The documentary data consisted of butchery literature, which was collated from a variety of sources. These were technical manuals dealing with butchery practices, cookbooks, ethnographic accounts and archaeological sources. They represented a diversity of cultural backgrounds but dealt chiefly with American, Australian and British butchery practices. These data will be discussed in Chapter 5.

The archaeological data used in this thesis are the faunal remains recovered from excavations carried out between December 1986 and May 1987 of the 'European Rubbish Tip' (ERT) on St Helena Island, Moreton Bay, Queensland. This midden served as the main dumping area for all refuse from the island, including workshop waste, domestic waste and industrial waste from operations such as lime-burning and sugar-refining. The midden contained extensive faunal remains and these are used to test the proposition essential for Lyman's butchering unit method to work, that is that butchery is a standardised process. This proposition will be tested by recording the exact location of butchery marks, such as cut marks, saw marks, cleaver marks, saw shearfaces and cleaver shearfaces. These data will be presented in Chapter 7.

Chapter 2 will review historical archaeology both in Australia and overseas, especially in North America. Historical archaeology will be defined and the reasons for studying it discussed. In particular the relevance of faunal analysis in Australian historical archaeology and developments in overseas faunal analyses from historical sites will be reviewed. This chapter will also examine the published studies of fauna from Australian historical sites. Critical evaluation of the usage of taphonomic theory in these studies will be an important part of the discussion in Chapter 3. Taphonomic understanding will be seen as being the most basic factor in all aspects of faunal analysis and especially in quantification studies. For this reason, Chapter 3 will evaluate how taphonomy can aid in the analysis of historical faunal material.

Chapter 4 will review the sort of information we can expect to get from historical faunal analysis and how we get this information. This will stress the need for reliable quantification methodologies and this, in turn, will lead to a critical evaluation of those methods currently used to quantify faunal remains. The purpose will be to demonstrate the current difficulties involved in such quantification methodology.

Chapter 5 will summarise the data gained from the ethnoarchaeological research and from texts containing description of butchery practices. The basic data retrieved from the ethnoarchaeological studies is contained in Appendix 1 and that retrieved from the literature search is contained in Appendix 2. In this chapter, the data will also be analysed and the results of this analysis produced.

Chapters 6 and 7 will present the archaeological data. This will be treated in the same way that the enthnoarchaeological data and documentary data were treated in Chapter 5. That is to say a summary of the basic data will be given and the analysis of this data and the results of this analysis will be presented. The basic descriptive data itself will be found in Appendix 3.

In Chapter 8 the results of the analysis presented in Chapters 5 and 6 will be discussed. Lyman's butchering unit method will be tested, in the light of the results obtained from the ethnoarchaeological, documentary and archaeological data, as to its applicability to the analysis of fauna from Australian historical sites and fauna from archaeological sites in general. This chapter will also present an alternative approach to butchery analysis as a means not only to define the pattern of butchery present in an assemblage but also as a potential means of quantifying faunal remains. In these discussions the issues raised in Chapter 4 will be taken into consideration.

Finally, Chapter 9 will present the concluding remarks for the investigation as a whole.

CHAPTER 2

Historical Archaeology and Fauna

> ... the crux of the matter ... [is] the ability of archaeological evidence to add a third dimension to historical research which will bring into clearer focus the familiar, everyday life of the past, no matter in what period.
>
> (Cotter 1978: 18; emphasis mine)

2·1 Defining historical archaeology

In Australia it is sometimes necessary to remind ourselves that archaeologists not only study periods of great antiquity or societies without a written history: archaeologists and prehistorians are not synonymous. These days archaeologists are examining material culture from all periods of human history, from the remote past to the immediate present. Over the last thirty years and particularly the last fifteen, this has meant a major reorientation in what we as archaeologists think we do. So what is archaeology? The definition of archaeology used in this thesis is that of Rathje (1981), who defined archaeology as 'a focus on the interaction between material culture and human behavior and ideas, regardless of time or space.' It is this 'regardless of time or space' which more accurately than earlier definitions, describes the field of archaeology as now understood. As Daniel (1963: 252) put it, archaeology 'actually begins today'.

Ragir (1972: 178) has defined an archaeological site as 'an accumulation of materials which are the residues of cultural activity'. As archaeologists it is our job to decode the information inherent in these 'residues of cultural activity' in order to further our understanding of the human experience. Our aim is to define patterns of past and present cultures or other recognisable entities such as ethnic groups, in order to better understand and explain past lifeways, culture history and the culture process.

Formerly history was seen to take over from prehistory with the advent of written records. The boundary known as protohistory still exists but with an addition. Not only does history start with the advent of written records, but so does historical archaeology. What is historical archaeology? There has been much debate over the answer to this question, and indeed as to whether the field should be called historical archaeology (Deagan

1982). This debate, known in the literature as 'the crisis of identity' (Schuyler 1978a), took place in North America, was prevalent in the 1960s when the discipline was finding its feet, and although it has abated, is still not fully resolved.

During the 1960s there was a perceived need by those archaeologists working on historical sites to name their area of study, so as to distinguish it from prehistoric archaeology. This was seen as necessary in order to establish the field as a separate discipline. A range of names was advocated: 'historical archaeology', 'historic archaeology', 'historic site archaeology', 'historic sites archaeology', 'post-medieval archaeology', 'colonial archaeology', 'restoration archaeology', 'tin can archaeology' (Schulyer 1978c: 27). Faults have been found with many of these terms which have meant that they were not acceptable to the majority working in the field. For many, the main criticism was that the name was too restrictive in terms of the subject matter under consideration. For example, 'colonial archaeology' omits the study of precolonial sites which have written records and post-colonial sites. Two terms emerged from the debate as being most popular amongst the practitioners. These were historical archaeology and historic sites archaeology. Criticism of the latter involved two aspects. Firstly, the word 'historic' was seen to imply that the field concentrated on sites significant to history and not on sites significant to understanding cultural development. Secondly, the word 'site' was seen to exclude artefact studies using museum collections and at other off-site locations. Based on definitions put forward by Harrington (1952) and Fontana (1978), Schuyler (1978c: 28) has defined historic sites archaeology as:

> The study of the material manifestation of the expansion of European culture into the non-European world starting in the 15th century and ending with industrialization or the present depending on local conditions.

The restrictiveness of the term 'historic sites archaeology' has meant that it has all but dropped from usage.

Use of the term 'historical archaeology' predates 'historic sites archaeology'. Woodward (1978) used the term 'historic archaeology' in the 1930s and Setzler used the term historical-archaeology in a paper published in 1943. The term has, therefore, been used for some time but its emergence as the accepted name of the discipline has only occurred in the last twenty years. The acceptance of this term is without doubt related to the 1967 conference held at Southern Methodist University, Dallas, Texas, where the question of what to name a new society devoted to the archaeology of historic sites was debated at length. The result of this debate was 'The Society for Historical Archaeology'. It is from the title of this society that the name of the discipline has been borrowed.

Over the past twenty or so years a range of definitions has been advocated for historical archaeology (Deagan 1982: 153; Deetz 1972: 115; Deetz 1977: 5; Jelks 1968: 1; Larrabee, Cotter and Noël Hume cited in Cleland and Fitting 1978: 242; Schuyler 1978c: 27; 1978d: 33; South 1977: 1-2; Birmingham and Murray 1987: 23). Deagan's definition would probably find agreement with most historical archaeologists. Deagan has stated that 'the field includes the study of human behavior through material remains, for which written history in some way affects its interpretation.'

The term historical archaeology and Deagan's definition best reflect the dual data base of the field. That is the use of written records in conjunction with material remains. However, the presence or absence of written documents, whether made by a culture themselves or by observers from a different culture, is not the crucial difference between historical archaeology and prehistorical archaeology. Historical archaeology can be conducted in areas for which there are no written records, such as parts of Africa (Posnansky and Decorse 1986; Schmidt 1978) where oral history has been substituted for the written record when examining the archaeological record of the recent past. The major difference between historical and prehistorical archaeology is in the range of artefact types present in the material culture of those societies studied. Historical archaeology involves the analysis of the material remains of techologically advanced complex societies, whether these remains are found associated with the manufacturing society or in satellite societies, or societies which gained the items by trade and have thus been influenced by the original manufacturing society in some way. For example, the study of the material remains from a nineteenth-century miners' camp, even if there is no documentation for this site, would be seen as historical archaeology. So too would the study of a late eighteenth-century Aboriginal camp in Sydney, if it exhibited historical artefacts; although such a site would most likely involve a prehistorian as well and the study more correctly referred to as protohistorical archaeology.

W. H. Holmes said, 'Archaeology is the great retriever of history ... it reads and interprets that which was never meant to be read or interpreted' (Cotter 1978: 18). This is one of the reasons why we do historical archaeology. The advantage of doing archaeology on sites with written records is that it provides a unique way of examining the past from two data bases, which complement one another and produce a picture of the past with more depth of field and a clearer focus than could be produced using either historical or archaeological data in isolation. Historical archaeologists, are in a unique postion to contribute not only to archaeology and anthropology, but also to history and to a number of related disciplines. Historical archaeology can supply historical data not available through documentary sources or any other sources. Its value to history lies in those areas where there is a dearth of documentary evidence such as for those groups 'excluded from historical sources because of race, religion, isolation, or poverty' (Deagan 1982: 171). Connah (1988: 3-4) has argued along similar lines for Australian historical archaeology pointing out that written records for disenfranchised groups were 'usually written by their social and economic superiors who were often ill-informed, if not prejudiced witnesses.' Another limitation of the written record pointed out by Connah is that the records 'will often tell us that something was done, but will be uninformative about how it was done.' Furthermore, Connah states that the information contained in written records:

> is what the writer says happened— this may not be the same as what actually happened. The writer may have been deliberately dishonest, prejudiced, poorly informed, mistaken, or may merely have made a slip of the pen. In contrast, relevant archaeological evidence may be able to show us what really happened. Thus the historical document gives us evidence comparable to that of a witness in a court of law; archaeology provides evidence similar to that of the forensic scientist.

Coutts (1976) has demonstrated how archaeology can contribute to aspects of history that have little documentation. Coutts used archaeology in order to make a contribution to the little known early post-contact history of southern New Zealand, in specific regard to the whaling industry. The results of this work were able to throw light on early contacts between Europeans and Maoris in the southern regions of New Zealand and on the early European exploitation of this region.

However, historical archaeology is more than 'historical supplement' (Murray and Allen 1986: 92). The anthropological background of its practitioners means it is also able to produce 'cultural images of the past that are more complete and to some degree different than those generated from documentary history' (Schuyler 1978b: 1). This is because the documentation available to the historical archaeologist enables him or her to go beyond determining the emic aspects of culture but also permits him or her to reconstruct the etic aspects. This ability to know the value and beliefs, to know what the people under study were thinking (the emic view), is unavailable to the prehistorian, who is restricted to reconstructing only the etic aspects of the culture under study (Davidson 1988: 23). This is the advantage of historical archaeology and where its potential lies. Alas this potential has seldom been reached. The integration of historical and archaeological information has often been poor.

In Australia, historical archaeology has a narrow time frame. With the exception of some maritime and Macassan trepang-processing sites dating from the seventeenth-century, its temporal framework begins with the first fleet of British convicts in January 1788.

Though its time frame may be relatively short, it covers a diverse range of subject matter: maritime archaeology, the archaeology of standing structures, landscape archaeology, the archaeology of mining ventures and their communities, industrial archaeology, urban archaeology, and restoration and preservation archaeology. One of its principal concerns, as demonstrated by the theme 'Archaeology and Colonisation: Australia in the world context' of the 1987 Annual Conference of the Australian Society for Historical Archaeology, is that of the British colonial experience. This is an area of research that Australia shares with North America. Historical archaeologists in both continents are asking very similar questions, such as:

> What cultural baggage was brought by the settlers from their homeland? How did previous interaction with the environment influence their approach to the new landscape? How was their approach modified by the nature of the new land, its resources and its inhabitants?
>
> (Davies and Egloff 1986: 46)

Although the term historical archaeology is new, the subject is not a new field of research. Deagan (1982: 151) has stated that:

> Much of the earliest archaeology conducted in Europe was historical archaeology because it was concerned with civilizations documented in some form by written records. The origins of archaeology in the fifteenth century resulted in a tradition emphasizing the classical sites of Greece, Rome, and the Biblelands using both documents and objects as research tools.

Even in the United States historical archaeology predates prehistoric archaeology, with its first excavation taking place in 1622 and historical archaeology being used in 1797 to settle a political dispute between Britain and the newly established United States (Deagan 1982: 151). These exceptions aside, historical archaeology of the recent past is a twentieth-century development. The discipline as we know it today was being practised by the 1930s by such people as Woodward (1978), who did pioneering work on historical artefact analysis, especially glass beads used by American Indians. But it was not until the 1950s that the archaeology of the recent past emerged as a new area of scholarly research in both North America and Great Britain, and not until the 1960s that the field gained momentum and blossomed into the modern discipline. This development was spurred on by the establishment in America of the annual Conference on Historic Sites Archaeology and the publication of its proceedings, the founding of both the Society for Historical Archaeology (in America) and the Society for Post-Medieval Archaeology (in Britain) in 1967, and the establishment of respective journals, and the founding of the Australian Society for Historical Archaeology in 1970.

Although the Australian Society for Historical Archaeology was formed in 1970, it was not until 1983 that it was able to publish its own scholarly journal. This reflects the relatively late inception of the field in Australia during the late 1960s. The beginnings of historical archaeology in Australia can be traced to the investigation of the three widely separated sites at Port Essington in the Northern Territory, Irrawang in New South Wales and the Fossil Beach Cement Works in Victoria. Since this early pioneering work, significant excavations have taken place in both the academic and public spheres of archaeology (Birmingham and Murray 1987: 20), but excavation has not been the only work conducted by historical archaeologists. As a result of a need to protect sites from an ever increasing amount of developmental destruction, the major area of work for historical archaeologists in the late 1970s and 1980s has been in the cultural resource management sphere. This has resulted in a large number of sites being recorded without excavation. Unfortunately, this documentation has often been poorly or incompletely published.

Another problem facing historical archaeology in Australia, which has been detailed by Murray and Allen (1986), has been the failure of Australian historical archaeologists in general to recognise the potential of, or develop, theory. Murray and Allen (1986: 89) state that there has been little development beyond the level of method and technique, and that 'Greater attention to theory building is therefore considered necessary'. Birmingham and Murray (1987: 21) have pointed out that historical archaeologists have in recent years begun to ask more searching questions of material remains:

> in an effort to provide information about social organization, subsistence, trade and communications— even demography and ethnicity, thereby providing more detailed reconstructions of past ways of life and evidence for an understanding of the cognitive processess of many years past.

Poorly developed artefact analysis in Australian historical archaeology is related to the relatively late emergence of the discipline in this country, coupled with few practitioners. This has meant that the archaeologist working with historical sites has had to acquire a knowledge of artefact classes far beyond the normal range of most prehistorical archaeologists. The historical archaeologist has had to become a jack of all trades when it comes to artefact analysis. What is desperately needed is for historical archaeologists to specialise in certain areas of artefact analysis, such as fauna, ceramics, glass, nails, and so on.

Specialisation in artefact analysis will also solve another problem in Australian historical archaeology noted by Connah (1988: xv) and Murray and Allen (1986: 87, 89). This is the failure to go beyond the description of artefact classes and move into detailed analyses which produce meaningful insights into past lifeways, contributing to understanding past human behaviour, and the functioning and evolution of cultural systems. Cleland and Fitting (1978: 244) pointed out this very same problem for American historical archaeology over twenty years ago. Since then this situation has been remedied in America. Deagan (1982: 153) has stated that:

> historical archaeology has made a rapid theoretical progression from descriptive and chronological concerns, through cultural-historical studies, to problems of culture process, cognition, and archaeological principles.

In Australia we are moving in a similar direction, as noted in the above quoted passage from Birmingham and Murray (1987: 21), and experimenting in developing hypotheses, such as Birmingham and Jeans' (1983) 'Swiss Family Robinson' model of colonisation. In many ways this process has mirrored the situation in British prehistoric archaeology forty years ago, which had an over emphasis on what Clark (1971: 14) described as narrow 'typological and stratigraphical studies', prior to the development by scholars such as Clark and Higgs of what has become known as economic prehistory and the subsequent development of broader theoretical concerns. Historical archaeology has emerged from its 'preliminary stage of groping' (Clark 1971: 13) and is now attempting to extract from the archaeological and historical record, meaningful statements about past human societies. Clark (1971: 13) has stated that this cannot be achieved:

> through the mindless accumulation of data . . . What is required is that both the recovery and the analysis of data be informed and directed by coherent and explicit theory— explicit because it is only when theory is acknowledged that it is likely to be adequately tested.

Murray and Allen (1986: 85) have argued that:

> the successful development of historical archaeology in Australia can only occur if research options are expanded through the development of clearly defined archaeological problems of greater than local significance.

Birmingham and Murray (1987: 33) list a number of areas where historical archaeology could be successfully applied, which would expand research options and further the discipline in Australia. By expanding our area of research to tackle ever more complex issues the discipline will be furthered. In Australia today the discipline is making rapid progress and it is facing head on the challenge to contribute insights into the past beyond those available from the historical record.

2.2 Faunal analysis in Australian historical archaeology

Over the last forty years archaeology has shifted from detailed accounts of artefact morphology to the investigation of broader issues, such as the behaviour patterns and economy of past human populations. Crader (1974: 161) has stated that:

> This holistic approach has led to more basic concerns relating to subsistence economy and ecology. An integral part of this approach is the analysis of faunal remains from archaeological sites due to the realization that a great deal of information regarding human subsistence patterns can be gained by the careful and intensive study of animal bones in an archaeological context.

This is what faunal analysis can contribute to Australian historical archaeology.

Faunal analysis is the term used to describe the shared methodology used by zooarchaeologists and ecological palaeontologists (Klein and Cruz-Uribe 1984: 1). Zooarchaeology is the identification, analysis and interpretation of animal remains from archaeological sites. The goal of zooarchaeology is to establish a more complete picture of our past environment and way of life, to the extent that the animal remains allow.

The history of zooarchaeological analyses for historical archaeological sites is comparatively short. The first North American historical site faunal report was not published until 1960 (Jolley 1983: 64). Since this time, until comparatively recently, there has been a lack of attention to faunal remains from historical archaeological sites. Jolley (1983: 66) has stated that, 'There has been a consistent lack of concern for faunal studies by a number of historic sites archaeologists.' This is still the case in Australia. The lack of attention to faunal remains is surprising when one considers that animal bones are 'often among the most numerous materials found in excavation' (Bailey and Grigson 1987: 17). During the last decade, especially in North America and Britain, there has been considerable advancement in the analysis of faunal remains from historical archaeological sites. There have been some major contributions made in Britain by faunal analysts such as Maltby (1979; 1984; 1985a; 1985b), Noddle (1975), Grant (1984), and Coy (1983; 1985), who have all published numerous faunal analyses primarily dating from the medieval period. In the United states the work of Losey (1973), Cumbaa (1978), Fradkin (1980), Lyman (1977; 1979; 1987), Dansie (1979), Rosen (1978), Schulz (1979), Schutz and Gust (1983), Langenwalter (1980), Honerkamp (1981), Reitz (1987), Reitz and Cordier (1983), Reitz and Honerkamp (1983), Reitz et al. (1987) and Reitz and Scarry (1985) stand out as major contributions to historical archaeology and archaeology in general.

Ritchie (1986: 587) has stated that faunal remains 'contain a considerable amount of cultural information which can be deciphered through analysis'. Ritchie also notes that they have the advantage of being of little interest to fossickers. This can be useful in sites which have been gone over by bottle diggers, because information can still be extracted in some instances even though the exact provenance may not be known. Such a case was the analysis of faunal remains from the Half Way House Hotel site, Central Otago, New Zealand (Bedford 1986). Faunal remains from historical sites also have the potential to advance the field of zooarchaeology far more than prehistoric faunal studies (Jolley 1983: 68), and produce highly reliable answers to the questions posed by the investigator. This is a result of the dual data base of historical archaeology. The tighter controls that historical documents offer historical archaeology should allow better resolution of analytical problems in zooarchaeology. Jolley (1983: 69) has stated:

> These documentary items hold the potential for establishing accurate meat yield calculations and determining whether the total meat yield or the butchering unit is the proper analytic tool.

The interpretation of faunal remains should be given an integral role in the reconstruction of past economies, settlement patterns and societies. Only by such an integrated approach to past remains will valid inferences about status, butchering and dietary practices be forthcoming. Bailey and Grigson (1987: 17) have stated that 'The retrieval of faunal remains should play an integral role in excavation strategy.' It is only through this approach that faunal remains can be used to test models. For example, a well excavated sample of archaeological fauna from a number of sites in a region of the same temporal context can be used to test subsistence models generated from ethnohistory or historical documents. Faunal remains can generate a range of information to be used by archaeologists with diverse areas of research interests, who may operate using different paradigm bases. For example, Kent (1987: 537-8) has stated that:

> Faunal remains provide subsistence, resources exploitation, and environmental reconstruction data to ecologists. To cultural materialists they provide data on economics, diet, and general behavioral strategies.

Faunal remains from historical archaeological sites can generate information concerned with a diverse range of topics, such as:

1. Economic strategies.

2. Variations between different cultural systems (Jolley 1983: 75).

3. Aspects of a culture's technological system (Jolley 1983: 71).

4. Socioeconomic status (Crader 1984a; Garrow 1987; Jolley 1983: 71; Lyman 1987; Otto 1980; Schulz 1979; Schultz and Gust 1983).

5. Differences in social and ideological systems (Jolley 1983: 71).

6. Political factors (Jolley 1983: 72).

7. Food taboos (Jolley 1983: 72).

8. Different cultural preferences for food and food preparation (Jolley 1983: 72)

9. Ethnicity and ethnic differences (Jolley 1983: 71).

10. Intra-site function differences (Jolley 1983: 74).

11. Seasonality (Jolley 1983: 74-75).

As can be seen the potential applications of faunal analyses to historical archaeological sites are both numerous and varied. However, the analysis of historical faunal material has a long way to go in Australia before it reaches the standards of overseas reports. Historical archaeological faunal analysis in this country is still in its infancy. This is best exemplified by the meagre two sentences on faunal analysis in a handbook on historical archaeology in Australia published in the late 1980s:

> Analysis of bone material for information for information or [sic] butchering, food refuse and dietary patterns is yielding interesting results on American historic sites. Analysis of bone from Regentville now in process as well as the forthcoming analysis of the Hyde Park Barracks material will significantly advance this area of study in Australia.
>
> (Birmingham and Murray, 1987: 110)

Historical faunal analysis in Australia is given only a token gesture in published papers and excavation reports. Discussion involving faunal remains is crude and almost entirely without a theoretical base. To date there is no comprehensive published study of faunal remains from an historical archaeological site, such as Maltby's (1979) analysis of the faunal remains from Exeter, England. One possible reason for this, is that excavation of many historical sites has only recovered a meagre amount of faunal material. This situation may have resulted from the fact that excavations have tended to concentrate on structures rather than on midden deposits. However, it may also be a result of taphonomic factors which archaeologists have failed to take into consideration or of which they have simply been unaware.

The number of published faunal analyses from Australian historical sites is very small. Coutts and Aplin (1984), who performed a preliminary analysis on a portion of the faunal assemblage from Captain Mills' cottage, commented that 'at the present time there are no published faunal data of any substance available on comparable Australian sites'. This is still the case, with Coutts and Aplin's analysis by far the most comprehensive to date. Only six other faunal reports and four general papers, in which bone is mentioned, have been published for Australian historical sites. All are either appendices (as in the case of Coutts and Aplin) or else they occupy a very small section of an overall excavation report.

The first published faunal report available for an Australian historical site was Rowland's (1978) analysis of the animal bones recovered from the excavation of Captain Richards' House at Winterbourne, New South Wales. This analysis went little beyond listing numbers of identified fragments per anatomical unit for each identified taxa, and indicating that butchery marks were present. In 1973 Allen (1978) had published a paper in *World Archaeology*, which examined nineteenth century British imperialism, using his Port Essington work as an example. In that paper Allen (1978: 143) stated, concerning the fauna from this site, that:

> ... of the local fauna the archaeological evidence suggests only kangaroo and occasional birds provided meat, and throughout the lifetime of the settlement the garrison depended on livestock introduced from the neighbouring islands or Sydney. Pigs, cattle, sheep and goats were continually supplied, despite staggering losses on route.

These two sentences are all the discussion on fauna that Allen saw fit to include. A paper by Sanker (1979) merely acknowledged that there were 'numerous bones' recovered in his historical archaeological investigation of the Commissariat Store, Brisbane. Holmes (1979), in her published report on the excavation and analysis of artefacts from the Windsor Barracks Guardhouse, Sydney, produced merely a list of the faunal remains. An example from this list will demonstrate why Coutts and Aplin were justified in writing the above quoted sentence on faunal analysis from historical sites. We are told, for example, that bone B2 is a 'Fragment of bone, humerus, from a large animal, possibly cow' (Holmes 1979: 56). In the single paragraph of discussion on the faunal remains, we are told that one bone was burnt, some bones were cut, some bones were from 'smaller animals' and that large bone fragments may have come from cows (Holmes 1979: 24). Hope and McIntyre (1983: 49) very briefly mention the presence of bones in the Wilcannia

Common which they had surveyed. They simply state that 'The bones, at least some of which are of cattle, are very fragmented, and some are burnt.' This very short note may be excused, as this was not an analysis but merely a description of fauna in a bone midden observed in the process of conducting an archaeological survey.

Coutts and Aplin's report went well beyond the very low level of analysis evidenced in the reports and papers quoted. They set out four clearly defined aims. These were to:

1. Identify the principle taxa.

2. Determine the relative abundance of the taxa and where possible their contribution to the diet.

3. Define some aspects of the butchering, culinary and disposal processes.

4. Establish a fingerprint for the midden that can be used as a basis for the purpose of comparative analysis (Coutts and Aplin 1984: 391).

They described their methods; problems encountered and the resultant subsequent limitations on their analysis; produced a comprehensive table of their identifications, with information on age classes and presence of burning; with subsequent detailed tables on the degree of unburnt, burnt and calcined bone by weight and number; and produced a minimum numbers of individuals table. They then gave a lengthy discussion of their results.

Unfortunately contemporary and subsequently published analyses have not even approached the standard of Coutts and Aplin's report. In 1984 Andrew Wilson produced a 'complete bone analysis' (Petocz 1984: 20) for the faunal material excavated from a well in Rozelle, Sydney, New South Wales. This analysis was far from complete, not going beyond identifying the percentage of fragments belonging to the six taxa present, indicating that butchery marks were present, commenting on possible cooking modes indicated by the usual cooking practice for the joint or cut associated with the faunal remains present and concluding that the remains indicated 'a fairly low standard of living, but one not so poor as to be unable to afford the occasional good cut' (Petocz 1984: 13).

Three reports containing faunal analyses were published in 1985. Coutts published a preliminary analysis of animal bone from his archaeological investigations of the 1826 settlement site at Corinella, Victoria (Coutts 1985: 55, 73-75, 129). This analysis was summarised in a table, listing the number and weight of fragments for the identified anatomical components, for each of the taxa present. Discussion of his results was restricted to a mere paragraph. Mc Gowan (1985a and 1985b) published the results of two faunal analyses from historic sites in Tasmania. The first was from an archaeological investigation of the Risdon Cove Historic Site (Mc Gowan 1985a). In this report we are told who identified the bones, given a table of the species present and informed at which sites at Risdon Cove these species were found. A short discussion accompanies this, indicating: that the bone was highly fragmented; that indigenous animals were hunted for food, and also died naturally on the site; that some small mammal remains may have derived from owl pellets; that kangaroo bones present were all 'severely burnt', and that butchery marks were present; that domestic animals exhibited butchery marks; that marrow extraction was evidenced; and that six bone artefacts were recovered. The second report (Mc Gowan 1985b) concerns excavations at Lithend, at the Port Arthur Historic Site. In this report a table is given which tells us the species present and the number of bones or bone fragments found in each stratigraphic context. In the discussion of the analysis an attempt is made to relate the remains to the function of the area of the site from which they were recovered. It is noted that some were burnt and some had been gnawed by rodents. A brief discussion using a taphonomic argument is given as to why some bones are present and others are absent. The presence of butchery marks is noted, as is the presence of bone artefacts.

In a 1987 paper by Higginbottom on the excavation of buildings in the early township of Paramatta, it is merely noted that bone was present in some pit features and that it 'comprised mainly well-preserved chicken bones' (Higginbottom 1987: 7). In a table (page 13) on the range and quantity of artefacts, the number of bone fragments present in various parts of the site is given.

It is clear from these summaries that there is room in Australian historical archaeology for far more work on the analysis of faunal remains. Theory and method need to be developed, applied and tested. This thesis attempts to show how this might be achieved, and the benefits in understanding and interpretation that follow.

CHAPTER 3

Taphonomy in Historical Archaeology

The archaeological evidence has survived imperfectly and above all unevenly ...
(Clark 1952: 1)

Once bones are deposited out of the body of living animals, they are subject to deterioration, which could ultimately end in total disappearance of the elements.
(Haynes 1981: 361)

This chapter considers how developments in taphonomic theory can aid historical archaeologists studying bones from historical sites. It examines the similarity between the taphonomic problems faced by historical archaeologists and those faced by prehistoric archaeologists. Taphonomy is an example of a body of theory, used in archaeology primarily by researchers in prehistoric archaeology, which can aid the historical archaeologist. Historical archaeologists in Australia are failing to take advantage of recent developments in archaeological theory that are applicable to historical archaeology, such as the developments in taphonomy. In many ways, this failure to use taphonomic theory reflects a general inability amongst historical archaeologists in Australia to use theoretical frameworks in the context of problem-oriented archaeology. Australian historical archaeology could apply theory and methodology that has been developed by prehistorians and archaeologists studying historical sites in the United States and medieval and post-medival sites in Britain and Europe. The failure to develop a theoretical base in historical archaeology in Australian is not the situation in other countries. For example, taphonomic theory is used on overseas historical sites although it has not been developed to the same degree as for prehistoric sites. Maltby (1985a: 34) has stated that:

> It is important, however, that these new methodologies be applicable to all archaeozoological data. Ethnoarchaeological and experimental research can demonstrate the consequences of many taphonomic processes affecting faunal assemblages, but most of this work to date has concentrated on hunter-gatherer societies. Any methodological framework of general applicability must have the ability to isolate the processes of assemblage modification likely to be found in fully developed market economies, not just those of simpler situations. In this sense, research into the range of potential variation in the faunal residues of complex societies is not at all well developed.

Unlike Australian analyses, overseas analyses of historical faunal material, particularly British reports, do take taphonomic factors into consideration. Maltby (1985b: 22), who examined variability in Romano-British butchery practices, commented on how the effects of weathering and scavenging on bones can both 'destroy cut marks and increase fragmentation' and have 'a dramatic effect upon the frequency of observed cut marks'. He went on to state that: 'If relative frequencies of butchery observations are to be used as guidelines to the intensity of butchery, any calculations must take taphonomic factors into account.' In his earlier comprehensive study of the faunal material from Exeter, he stated that:

> The majority of the animal bone originally deposited on any archaeological site does not survive. This stark reality has to be accepted by archaeozoologists. A whole series of physical, chemical and human agencies combine to destroy all but a fraction of the original number of bone fragments.
>
> (Maltby 1979: 3)

In this same report, Wilkinson (1979: 75), who studied the fish remains from Exeter, also specifically took taphonomic factors into consideration, stating that:

> It is clear that skeletal elements are rarely present in the ratios in which they occur in the body, but much of this can be explained by the processes affecting the material between deposition and tabulation of results.

Like the faunal analysts working on material from British medieval sites, those working on historical sites in the United States are also aware of the recent developments in taphonomic theory. Jolley's (1983) review of zooarchaeology as it relates to historical sites in the United States, contains a large section dealing with taphonomy. This section goes beyond merely warning the faunal analyst of the potential taphonomic factors which if not taken into consideration can result in inaccurate conclusions being drawn from the results of faunal analysis. It summarizes the taphonomic factors involved and then goes on to exemplify these factors with a bibliography of primarily historical site case studies.

How can an understanding of taphonomic factors aid in the analysis of faunal material from Australian historical sites? There are two taphonomic problems faced in the analysis of faunal material. First, how did the faunal remains that are being examined become incorporated into a site? Fortunately this is a little easier for the historical archaeologist than for the prehistoric archaeologist. Historical sites are generally immediately identifiable by the types of artefacts present. The faunal assemblage is almost always dominated by domesticated animal remains such as sheep, cattle, pig and chicken bones, that have been butchered by metal tools which leave distinctive saw and cleaver shearfaces and marks. Historical archaeologists are not faced with the problem of large carnivores using these sites, although both domestic and feral cats, pigs and dogs are capable of depositing refuse on historical sites. Cats can leave small birds and rodents, which are generally explained away as drop deads. Both cats and dogs are also capable of depositing the remains of their meals on historical sites but these remains are generally from household refuse to begin with. Dogs are far more likely to distort results by removing elements of the assemblage. This is distortion in the sense that the action of the dogs alters the patterned association between the internal composition of the bone refuse and its human depositors (Binford 1981: 200). The only other significant factor in the deposition of faunal remains in historical sites, apart from human food refuse and human pest control (such as rat trapping) is from 'drop deads'. In historical sites these are generally easy to distinguish by their completeness, in both skeletal elements and articulation, and in their probable unsuitability as a food source.

The second taphonomic problem is the question of what has happened to the faunal assemblage since it was deposited. Faunal analysts must be able to explain why certain bones are missing. As with prehistoric sites, historical archaeologists are only dealing with a sample of the remains from any specific area of a site. Furthermore, faunal remains may be dumped in more than one location or scattered rather than dumped at specific locations. However, in general, historical archaeological sites have specific dumping areas. If a sufficient amount of such a dumping area is excavated, supposedly containing the remains of animals killed, butchered and consumed on the site, then if the assemblage lacks a full representation of skeletal elements, this has to be explained. The reasons why a full representation may not be present for an historical site are similar to those for prehistoric sites.

First, not all bone may have been dumped. Certain bones, particularly the leg bones of cattle, were used as a raw material. Bone was used on historical sites (such as St Helena Island, Queensland) to manufacture toggles, buttons, cutlery handles and a range of other useful and decorative items. The use of bone for this purpose can sometimes be deduced from the recovery of off-cuts in a midden. Bone refuse can also be fed to animals, particularly pigs and dogs. It is the latter that are most likely responsible for many of the differences between potential bone numbers or skeletal elements on the one hand and the actual bones present on the other.

Dogs and pigs may play a significant role in bone alteration and loss. Pigs were often raised at historical sites (see below) and were either directly fed domestic food scraps containing bone refuse or they had access to bone refuse at domestic dumping sites. In some situations, wild pigs may also have had access to bone refuse at domestic dumping sites. Cromwell's Chinatown (Ritchie 1983) and the Halfway House Hotel (Bedford 1986: 10) are two historical sites excavated in Central Otago, New Zealand, where pigs are known to have been raised and could have influenced the surviving faunal record. There are also numerous examples of pigs being raised at historical sites in Britain, especially those dating to the medieval period (for example: Biddick 1984). An aspect of Spennemann's (1990a) ethnographic study on the island of Tongatapu, Kingdom of Tonga, was observing the effect domestic pigs have on faunal refuse. Pigs were fed virtually all edible food waste, including bones, and they were also observed scavenging food refuse, which on most historical sites would include bone refuse. As a result of his observations, Spennemann concluded that the combined scavenging effects of pigs and dogs and the practice of feeding virtually all edible food wastes to pigs, would result in an 'almost complete loss of faunal remains' (Spennemann 1990a: 103).

Bones fed to the domestic dog are unlikely to turn up in a midden. Observations of domestic dogs have shown that they will remove from a site bones which they are fed and reduce them to fragments which may not be detected archaeologically. The degree of fragmentation depends on the relative size of the dog and the size of the bone it is gnawing. It also depends on the age of bone, that is its degree of fusion, as this will influence the density of the bone and hence its resiliance to the action of a dog's jaw. Dogs may also remove bones from a midden site itself, as may other scavengers. In Australia, the other prime candidate for this activity are foxes for historical sites younger than 1868, the year of their introduction (Troughton 1973: 189). Examination of two historical sheep-butchery discard sites at Mt. Wood Station, Sturt National Park, and at Mt. Stuart Station, near Tibooburra, and of an historical kangaroo-shooters camp, also near Tibooburra, produced evidence consistent with the faunal remains having been scavenged by foxes. This evidence consisted of bones with tooth marks made by foxes and the presence of fox faeces containing a high proportion of bone fragments.

So, part of the answer to the question of what happens to faunal remains once they have been deposited, is that domestic and wild carnivores may remove faunal elements from a site. The gnawing action of these animals may be detected on some bones. The marks to look for are discussed by Crader (1983: 112) and Payne and Munson (1985) and are detailed in Chapter 6. The identification of these marks only informs us that such action has taken place. Quantifying such a taphonomic factor can prove exceedingly difficult.

Dogs are not the only animals which gnaw bone. Rodents, particularly rats, are an agent of attrition which must be taken into account on historical sites as on prehistoric sites. For the historical archaeologist, rats provide problems by removal of bones from sites and by gnawing away parts of other bones. Experiments conducted on St Helena Island, Moreton Bay, Queensland, demonstrated that rats are capable of moving considerable amounts of bone refuse for significant distances from the site of deposition. Rats often gnaw the ends of bones, where saw and cleaver shearfaces are present. This has the effect of removing these faces and hence making determination of butchering patterns less reliable, in an area of analysis which can be difficult to begin with, considering other agents of attrition and other taphonomic factors.

Coutts and Aplin (1984) and Coutts (1985) indicated that some of the faunal remains in the assemblages they were studying were burnt. It is quite common that a proportion of bone from historical sites is burnt. At some sites, particularly ash middens, this can amount to a significant proportion. Bone can arrive at a site already burnt, from bones burnt in the act of cooking or from bones which were tossed in a fire from which the ash was subsequently dumped on the site. Bone can also be burnt after it has been deposited, from fires lit on the site and from exposure to ash deposited while still hot. The latter has been observed for the faunal material excavated from the ERT historical midden on St Helena Island, where hot coal ash was deposited on top of bone refuse and then covered by a thin layer of earth. Bone which has been burnt becomes extremely brittle and tends to break up in a site, producing a characteristic square fracture. This can lead to an accelerated rate of decomposition, as it exposes a greater surface area for other agents of decomposition to work on. It is necessary for faunal analysts to take this factor into consideration, when attempting to recreate the events that have taken place since the bones were deposited in the site under study.

The problem is how the faunal analyst takes this factor into consideration and relates it to other factors, particularly chemical weathering in enclosed soils. At this point in time there is no simple answer, nor is there likely to be one in the immediate future. However, experimental work by Coy (1975), David (1990), Shipman et al (1984), Spennemann and Colley (in press) and others, with the effects of fire and heat on bone, are slowly building up a body of data which the faunal analyst can use. A number of publications in recent years, such as White and Hannus (1983), Pate and Brown (1985), Bonnichsen and Will (1980) and Haynes (1981) have investigated the effects of different soils on bone. Nevertheless much more experimental work is needed, particularly in the area of soil chemistry and its effects on burnt bone, especially for Australian soils.

There are two other significant factors which affect bone once it has been deposited: physical and chemical weathering. Physical weathering involves a variety of actions which can cause bone to decay. This decay can make it difficult to detect butchering marks, or else it can produce shearfaces which can be mistakenly interpreted as being culturally derived. The main physical weathering action to affect bone is that of climate. Wind erosion, freeze fracture and the drying effect of the sun will all result in bone breaking up into smaller pieces. These pieces will have a reduced survival period and are much more difficult to quantify (see Chapter 4) and decide whether or not butchery marks are present. Also, on historical as on prehistoric sites, plants erode bone. Root action causes what is known as root etching, which can erase fine cut marks from the surface of a bone.

Once bone is deposited in the site, chemical weathering in conjunction with the above-mentioned taphonomic agents can result in the total decay of the bone. Although the age of historical sites is but a fraction of the age of prehistoric sites in Australia, the nature of the soils in many historical sites is such that bone weakens structurally, becomes friable, and may even decay totally. It is important to note that these effects will not be uniform across species or within species, due to differential bone densities. This makes the excavation of the bone difficult and limits the accuracy of results from analysis. To date, the sample bias which may result from chemical weathering has not been taken into consideration by those analysing faunal remains from historical sites.

Although in the past those analysing historical faunal assemblages have failed to take taphonomic factors into consideration, this situation is changing. This chapter has attempted to show that the taphonomy of bones is as important a consideration in the analysis of historical faunal assemblages as it is for prehistoric assemblages. The problems historical archaeologists are faced with are the same as those for prehistoric archaeologists. Hopefully in the future we can have a co-ordinated approach in research and in the exchange of information between these two groups of archaeologists.

CHAPTER 4

Faunal Methodology

... it is really meat we are interested in, not bones.
(Daly 1969: 148)

4·1 Information from historical fauna

More attention is now paid to faunal analysis by historical archaeologists than in the past. This in part results from advances which have been made in the analysis of faunal material from prehistoric sites in the last twenty years, and also in is part due to the realisation, by historical archaeologists, of the wealth of information which can be extracted from faunal remains. Reitz et al. (1987: 307) have stated that 'the goal [in faunal analysis] is to explore human interaction with animals'. By exploring this 'interaction', a diverse body of information can be extracted in order to answer the many questions posed by archaeologists. In Great Britain, historical archaeologists studying medieval sites are particularly interested in extracting information concerning husbandry practices. They are concerned with issues related to economic history, such as the improvement of stock, the development of new breeds, and the function of the animal in the economy. They want to know whether the animal was being raised for its meat, wool, hide, milk, traction capabilities or as a mode of transport. In the United States, historical archaeologists studying the colonial period have tended to concentrate on extracting meat weights from their faunal remains. The reason for this has been to enable them to answer questions concerning relative dietary importance of the different taxa represented in a site or sites; questions concerning socioeconomic status; questions related to the ethnicity of a site's occupants; and questions concerning acculturation. Other information can be extracted from faunal remains: in order to investigate seasonality of site use, hunting patterns (if wild animals are present), site attrition, source of supply of meat, techniques of butchery and methods of marketing.

These questions are answered through a two-step process, involving recording from the faunal remains what Wing and Brown (1979: 118) and Horton (1984: 256) have termed primary data, and then by extrapolating secondary data from this. Depending on the questions being asked, primary data can be gained by recording the following information during the initial processing of faunal remains: identity (skeletal element, side from which element derived, taxa from which element derived); position of bone (spatial and temporal); modifications to the bone (evidence of butchery, evidence of pathology, evidence of taphonomic processes such as weathering, action of carnivores, root etching, and burning); state of epiphyseal fusion; state of tooth eruption and tooth wear; indicators of sexual dimorphism (such as spurs on the tibiatarsals of domestic roosters); bone measurements; and amount of bone (numbers and weights). Secondary data is derived from the analysis and interpretation of this primary data and is used, for example, to establish age and sex profiles of the fauna represented in the assemblage, and it is also used to quantify the faunal remains.

These two kinds of data are then used to interpret behavioural practices in the past. For example, Payne (1985: 145) has used age profiles derived from sheep/goat remains to determine husbandry practices which reflect an aspect of a community's economy. Payne has stated that:

> When milk production is the main aim, for instance, animals are often killed at a very young age (often within the first month), so that they consume less of the milk; while when meat production is the chief purpose, kill-off of young animals is deferred until they reached the desired size and carcass weight (often between 6 and 12 months in sheep and goats, but often even older); and when wool or hair production is the main aim, few immature animals are killed.

4·2 Methods used to age faunal remains

Quantification of faunal remains may draw on data about age at slaughter. Historical archaeologists are much more fortunate than their prehistorian counterparts when it comes to age determination of the faunal remains from their sites. This is because the considerable efforts which have gone into designing methods to age bones have used data based primarily on domesticated animals. In part this reflects the requirement for a method to determine age profiles of excavated fauna by archaeologists working on the process of domestication and on early husbandry practices. But it is also because modern domesticates represent a large, reliable comparative sample, to which access is considerably easier than for wild animals.

There are a variety of skeletal criteria available which can be used to age faunal remains from historical sites. These are methods based on epiphyseal fusion, closure of cranial sutures, tooth eruption and replacement, tooth wear, the annual deposit of cementum on tooth roots, size and form, and qualitative features, such as signs of age-related pathology, greenstick fractures, malignant tumors, and arthritic conditions (Silver 1969: 287). Of these techniques, epiphyseal fusion and tooth development and wear have been commonly used in establishing age-profiles for faunal remains from historical sites.

Epiphyseal fusion and tooth development and wear methods are based on the assumption that bone growth, tooth eruption and tooth wear occur more or less at a constant rate amongst individuals of the same species. Because bone and tooth development, and tooth attrition, are not without variation between individuals and breeds of the same species, such studies provide relative ages, not absolute ages. It is an important point to realise, that published age-profiles are not documenting chronological age but developmental age. This is rarely explicitly stated in faunal reports. Any historical archaeologists who base conclusions on absolute age determinations of the archaeofauna are deluding themselves, as are those readers whose knowledge of faunal analysis is such that they are unaware of this point. Grigson (1982: 15) has pointed out that because we are looking at age ranges and not absolute ages, a variety of differing interpretations of the data can be presented. Watson (1978: 97) has detailed this point by looking at age-profiles resulting from epiphyseal fusion data. From this it is clear that conclusions made in the past from age determination data will have to be called into question, and that in the future we will not be able to be as positive in our conclusions, and that these conclusions will not be as detailed as in the past.

This point about relative versus absolute age has been made by a number of authors (Bowen 1978; Grigson 1982; Hillson 1986; Maltby 1979; Silver 1969; Watson 1978). Variation, between individuals of a species, of developmental changes such as epiphyseal fusion and tooth-eruption, results from a number of factors but those which most likely cause the greatest range of variation are breed, stock-management practices and environmental conditions.

Developmental age may tell archaeologists a lot more than true or chronological age. The data themselves are evidencing a biological process which does not always take place after a specific period of time has elapsed. This is because the process (either the fusion of an epiphysis, or eruption of a tooth) is related to biological development and fortunately this does occur in a set sequence. When archaeologists ask questions of age-determination data, they are usually much more concerned with the stage of the animal's development than they are with its exact chronological age at time of death. The reason archaeologists are interested in being able to provide an age in months or years for an animal, is based on the assumption that there is a relationship between chronological age and developmental state (or age). Yet the range which may exist in this relationship may be of such a magnitude as to make this relationship of little use to archaeologists. Since state of development is really what we want to know, developmental age circumvents the difficulties inherent in attempting to define absolute age-determination criteria.

There are other reasons why the data extracted from age determination analyses may not produce reliable information on areas of interest. Silver (1969: 283) pointed out that the accurate assessment of the age of an animal requires four criteria be met. These are:

1. That the animal under investigation originated from a species or breed for which the age-characteristics are well documented.

2. That the nutritional state of the animal is known.

3. That most of the teeth and bones from it are present.

4. That the animal is not yet skeletally and dentally mature (that is, epiphyseal fusion has not taken place at all centres of ossification and not all teeth have erupted).

Archaeological material rarely complies with the first three of these criteria, though the fourth is often met when dealing with historical faunal assemblages. In the majority of cases domestic faunal remains on historical sites represent the remains of animals killed for meat. If an animal is raised for meat consumption, then it is likely to be slaughtered before reaching skeletal or dental maturity. Although this fourth criterion is met, the first is rarely met because of variation between individuals of a species. The possibility that contemporary data on modern breeds may not be applicable to earlier periods is an additional complication. Silver (1969: 283) pointed out that the main reason why the second criterion is rarely met has to do with the fact that the state of nutrition 'can only be guessed at, usually from the bones whose age is to be determined, which leads to a danger of circular argument.' The third criterion is rarely met because of the difficulty in determining which bones came from which individual. This last point raises a difficulty for establishing age-profiles, noted by Cribb (1985: 76) because the number of individuals under study is not known. The difficulties involved in quantifying faunal remains are discussed below. In quantification, especially using the minimum number of individuals (MNI) method, age information is required. There are also other factors which affect the reliability of age-profiles, such as the poor recovery of material and taphonomic factors.

The technique of determining age by epiphseal fusion is based on the premise that the epiphyses of different bones fuse at relatively specific stages in the development of an animal, and that these stages and their sequence is standard for a species. By recording the proportion of fused and unfused examples for each skeletal element it is possible to portray a relative age-profile for the species represented by the bones (Cribb 1985: 76). Tables of the relative ages at which different epiphyses fuse for the main domesticates have been established (see Silver [1969] for frequently used tables).

Use of the epiphyseal fusion method suffers from the difficulties mentioned above. It also has unique problems. In his study of kill-off patterns in sheep and goats from Asvan Kale in Turkey, Payne (1973: 283) put forward two objections against using epiphyseal fusion as a means of determining the age of the fauna he was studying. The first objection was that the fusion sequence ended relatively early. Sheep live seven to ten years and sometimes older, but the last group of epiphyses fuse between three and three-and-a-half years. His second objection was that of differential preservation. Unfused bones are less durable than fused bones, therefore there is bias against younger animals. The extent of this bias is at present undeterminable. Not only may there be a differential preservation bias against younger animals but in some sites there may be a bias against older animals, as a result of mature (that is, fully fused) bone being retained as a raw material for manufacturing a variety of artefacts.

Variability between individuals as to when fusion takes place is a very serious problem. This variation arises from a number of factors such as sexual dimorphism, differences in breeds and environmental influences. Another difficulty is that authors rarely define exactly what they mean by the term fused. Bull and Payne (1982: 67) have stated that:

> this might be as early as the first moment at which the epiphysis no longer separates from the shaft, or as late as the last moment at which the fusion line can still be seen; and these are at least a year apart in some cases.

Despite the difficulties associated with the epiphyseal fusion method it should not be scrapped. It should be used in conjunction with other techniques. This is because the other techiques are not without their share of problems. Also, in the case of the other most popular technique, tooth development and wear patterns, samples of mandibles and maxilla or even loose teeth are often found in insufficient numbers to enable age-profiles to be constructed from tooth data alone.

Teeth are particulary useful in age-determination studies because, unlike fusion which is complete early in an animal's life, changes in tooth development occur throughout the life of an animal. In dentally immature individuals, tooth-eruption sequences can be used as a basis of age-determination; while in dentally mature individuals, tooth-wear patterns can be used as an age-determination method. There is another technique, which involves analysis of the microstructure of teeth in order to count annual cementum deposits on the tooth roots. This third method is little used because of the 'time, equipment, and expertise in section preparation that the method requires' (Klein and Cruz-Uribe 1984: 45), and the fact that the method is destructive. It has only once been applied to an historical archaeological faunal assemblage. Coy et al. (1982) used a cementum annulus method to study a group of Anglo-Saxon cattle molars from Southampton in England.

The tooth-eruption method is based on the premise that the teeth of individual members of a species erupt in a set sequence at set times. By comparing an archaeological example with the generalised pattern, an estimate of relative age at death can be determined. The tooth-wear method is based on the assumption that the attrition of the enamel and dentine (of mainly the occlusal surface) is a continuous process which reaches certain stages at specific ages. Archaeological examples are compared to generalised patterns for specified ages and an estimate of relative age is concluded. Payne (1973) exemplifies well the use of attrition sequences in order to age an archaeological sheep/goat assemblage.

Apart from the advantage that tooth-age-determination methods span the life of an animal, there are another three advantages that such age-determination methods have over epiphyseal-fusion methods. Firstly, teeth are more durable than bones, and secondly, this durability is relatively stable for all age classes (except perhaps very juvenile animals where the enamel has not fully developed), meaning that age-profiles based on teeth are less likely to be biased by differential post-depositional destruction between different age classes. Finally, teeth are generally easier to identify to species than are bones (Klein and Cruz-Uribe 1984: 43).

A variety of dental age sequences based on tooth eruption have been put forward for domestic animals (Brown et al. 1960; Grigson 1982). As with various epiphyseal sequences proposed, Silver's (1969) eruption sequences are widely used. A problem with age-determination studies based on tooth eruption has been a failure on the part of authors to define what they mean by eruption. They do not usually state whether they are referring to gingival emergence of the tooth, appearance of the tooth in the surface of the jaw, or the tooth coming into wear. If they are referring to gingival emergence of a tooth, then the subsequent methodology is suspect because there is considerable variation in the timing of gingival eruption. Chaplin (1971: 78) and Hillson (1986: 181) have both noted variation in eruption timing, but also consistency in the order of eruption. Like Hillson (1986: 181), Chaplin (1971: 80) recommended the analysis of the stages of development (developmental ages) which he termed 'group frequency analysis'.

Tooth-wear methods suffer from difficulties just like the other age-determination methods. The major difficulty is that the premise on which they are based, that the rate of tooth attrition is constant for an individual animal during its life and also constant within a species, can be shown to be incorrect. That varying rates of wear can be shown to exist, seriously undermines the validity of using these methods. The principal factors influencing the rate of wear are the nature of the feed type and the mineral matter attached to or contained within it (Chaplin 1971: 86). The degree of soil (grit) ingested with feed in different environments has been shown to affect the rate of dental attrition. Variation in the ingestion of abrasive particles causes variation in tooth wear. This is particularly noticeable in areas of poor feed, areas suffering from drought, or areas which are over-grazed (Grant 1978: 104). Studies in New Zealand and Australia have demonstrated that there exists a relationship between the amount of soil ingestion (and consequently severity of tooth attrition) and stock numbers (Klein and Cruz-Uribe 1984: 52). Clearly, this difficulty is pertinent to Australia with its history of overstocking and droughts. The nature of the feed itself can also be expected to influence the rate of wear. Domestic animals are unlikely to be raised on the same feed throughout the year, particularly in winter, when during feed shortages animals may be fed 'harder and more abrasive foodstuffs' (Grant 1978: 104). In ruminants, the movement of the lower jaw in feeding is vertical and transverse. In cattle it has been noted that there is consistently more mastication in the vertical direction when the animal is fed on chopped feed, than when eating hay or grass (Grant 1978: 104). These variations in feed type may well differentially affect the rate of wear. Another factor associated with the nature of the feed, which will differentially affect the rate of wear, is the opal phytolith content of the feed. Opal phytoliths are composed of extremely hard silica and are possibly the single most important factor causing tooth wear in herbivores. That different pasture and fodder plants have differing concentrations of opal phytoliths, means that animals raised on different pastures can be expected to exhibit different rates of wear (Grant 1978: 104).

There are other factors which affect the rate of tooth wear between individuals of the same species. An assumption in the method is that teeth erupt and come into wear at approximately the same time for all individuals. Yet it has already been pointed out that there is considerable variation amongst individuals of a species in the timing of tooth eruption. Clearly, factors which affect the timing of the tooth eruption sequence will increase the age range associated with different wear patterns. There are also individual pecularities which affect the validity of the conclusions made from tooth-wear studies. These include variation in the hardness of enamel and dentine between different individuals of a species. Also, differential preference, whether as a result of injury, disease or some other factor, as to which side an animal chews, has been noted (Grant 1978: 104). This means that the rate of wear can vary between the left and right sides of the mouth, within a single animal. Finally, premature incisor loss is a fairly common occurrence amongst modern and possibly ancient sheep breeds (Grant 1978: 104). Any premature loss of incisors will have an effect on the rate of wear on the remaining teeth. Despite all these criticisms of the tooth-wear method, use on archaeological material has produced promising results, and further research into the method would seem worthwhile (Grant 1978: 105).

Tooth-wear and tooth-development methods are often used together. Three combined eruption-attrition schemes for age-profiles are available for sheep and goat mandibles. These are Ewbank et al. (1964), Grant (1982), and Payne (1973).

4·3 Quantifying faunal remains

One of the primary objectives of faunal analysis is to be able to quantify the remains of different species represented in a manner which permits the quantities to be compared with one another for some specific purpose. The most important piece of primary data involved in quantification is the identified specimen. The identified specimen, or rather the set of identified specimens, is basic to all quantification techniques; and there have been many methodologies put forward to quantify faunal remains.

The title of Gautier's (1984) paper: 'How do I count you, let me count the ways?' reflects the current state of quantification methodology. There are a multitude of techniques available for the faunal analyst to choose from. However, three methods have received far greater attention and use in the literature. These are methodologies based on the number of identified specimens (NISP), the minimum number of individuals (MNI), or the bone weight of identified specimens. Of these three methods, the MNI is the most frequently used, with the NISP running a close second.

Much of the controversy which surrounds the issue of quantification, is based on the growing realisation by zooarchaeologists that all the standard techniques suffer from problems which Grayson (1984: xix) has described as being 'far from benign and . . . far from fully understood'. In the early 1970s a number of researchers such as Bökönyi (1970), Chaplin (1971) and Perkins (1973) noted that there were problems involved with quantification, Perkins (1973: 367) stating that 'it is high time that we take a more careful look at the techniques that we have been using'. In recent years there has been a growing pessimism in the literature concerned with quantification (Gilbert and Singer 1982: 21). This is partly based on a realisation of the flaws in the methodologies themselves but also on a growing awareness by those analysts concerned with quantification issues, of the importance of sample size and of the limitations imposed on the data by taphonomic factors. It is unfortunate that the reverse of this situation is not true. Few taphonomists have given much concern to the very real problems which exist in zooarchaeological quantification. Grayson (1984: 179) has brought this situation to the attention of zooarchaeologists with a pertinent example:

> The problem is well illustrated by the fact that the most important work on cave taphonomy, and the most detailed analysis of the Kromdraai, Sterkfontein, and Swartkrans faunas, published to date is built on a one-paragraph discussion of quantification.

The realisation of taphonomic issues by analysts concerned with quantification is important. This is because no matter what technique is used, they are all affected to varying degrees by taphonomic factors. More and more analysts are beginning to appreciate that taphonomic factors may bias the proportion of species in assemblages.

The simplest quantification method is that using the number of identified specimens (NISP). The NISP method determines the relative abundance of different taxa by first summing the number of identified skeletal elements or fragments for all taxa and by letting this number equal 100 per cent. The percentage of NISP belonging to any taxon of the total reflects its relative importance. The mathematical representation of this method is:

$$\text{Relative Frequency of Taxon X} = \frac{\text{NISP for X}}{\Sigma(\text{NISP for all species})} \times 100\%$$

The method is not as popular as it once was, due to a number of assumptions which must be made in order to give credence to the results. These assumptions are:

1. That unless articulated to another bone, every individual fragment identified to one species is used to measure that species abundance relative to other species.

2. That all bones, from each species are equally affected by taphonomic factors.

3. That the number of identifiable skeletal elements is the same for all species.

4. That each species is exploited in the same manner.

5. That the butchering practices are the same for each species.

The unpopularity of the method results from the fact that these assumptions are simply not justifiable. Three factors which have kept this method alive are that it does not suffer from the problems of aggregation, as MNI does, that it is relatively easy to calculate, and that should further excavation take place at a site, then the subsequent NISPs are additive to the originals.

The first assumption in the NISP method is the cause of its most serious problem. This assumption can never be tested, and the likelihood that it is incorrect for all archaeological sites means that the data is interdependent. By this it is meant that one never knows whether or not the individual skeletal elements or individual bone fragments are independent of one another, that is, whether or not they are the sole representative of an individual animal, or whether or not they and other elements and fragments originate from the same animal (Grayson 1973: 432).

As with the first assumption, a great number of authors have criticised the NISP method because it is impossible at present to delineate the effects of taphonomic variables (Bowen 1978: 152; Krantz 1968: 286-287; Perkins 1973: 368; Reitz and Scarry 1985: 16). The inaccuracies resulting from differential preservation (Bowen 1978: 152; Grayson 1979: 201; 1984: 21; Lyman 1979: 536; Payne 1972: 68; Perkins 1973: 368) and differential butchery (Bowen 1978: 152; Chaplin 1971: 65-66; Grayson 1979: 201; 1984: 20-21; Payne 1972: 68; Perkins 1973: 368; Reitz and Scarry 1985: 16; Uerpmann 1973: 310) are the taphonomic factors most often put forward as objections to the method. Differential butchery strategies, whereby lower percentages of the bones of some animals arrive at the site, and the fact that butchery can result in many more fragments coming from a larger animal than from a smaller animal, have serious repercussions in terms of the accuracy of the NISP method, and the validity of the fifth assumption.

Fragmentation of bone within sites poses even more difficulties for the NISP count. This is due to different ways different analysts treat fragmentation. For example, the way different analysts record separate fragments which fit together from the same site will result in different counts for NISP. The problem of inflated NISP values resulting from bones which were deposited intact but later broke up in the site, has not been solved.

The third assumption, that the number of identifiable skeletal elements is the same for all species is the simplest to refute. Firstly, different species have different numbers of bones to begin with, although this is not as serious a problem as it appears. It is easily corrected for (Perkins and Daly 1968: 99), but unfortunately this often does not happen (Payne 1972: 68). More serious are the objections that some species are more easily identifiable than other species (Bowen 1978: 152) and that not all skeletal elements are equally identifiable (Payne 1972: 68; Watson 1979: 128-129). These last two objections, in combination with differential preservation, mean that the NISP technique tends to exaggerate the importance of smaller-sized species (Klein and Cruz-Uribe 1984: 25).

Although the NISP method is simple and quick to use, compared to other methods such as MNI, its use is not recommended. As Chaplin (1971: 67) put it:

> Its use is, however, time completely wasted for it allows no comparison to be made between any two sites because the bias which is certainly present cannot be detected or determined . . . Since the majority of bone studies have a comparative purpose we must seek other methods which allow direct comparisons to be made between different sites, cultures and geographical regions.

The MNI method to be discussed next, was just such an attempt.

The minimum number of individuals (MNI) is the most commonly used method to quantify faunal remains. MNI has been defined by Shotwell (1958: 272) as being 'that number of individuals which are necessary to account for all of the skeletal elements (specimens) of a particular species found in the site.' The method was brought to the attention of English-speaking archaeologists by White (1953a). It had, however, been applied by Russian archaeologists as early as the 1880s (Casteel 1977: 125), and had been used by palaeontologists since the late 1920s (Grayson 1979: 203). White (1953a) advocated that MNIs be determined by counting the rights and lefts of the most numerous skeletal element representing each identified species, and using the highest value as the estimated MNI. The method used most by archaeologists these days is still basically that proposed by White, but refinements have been added to the technique. These include taking matching pairs of elements (according to characteristics of age, size and sex) and aggregation units (based on stratigraphic units and features) into consideration. The MNI is then determined by the sum of the number of matched pairs, plus unpaired lefts, plus unpaired rights (Krantz 1968: 286). Care should be taken, however, in using size as a pairing criterion, as the left and rights of a bone from the same individual can vary in size (Chaplin 1971: 70). Honerkamp (1981) and Reitz and Honerkamp (1983) present examples from historical archaeological sites where pairing and units of aggregation have been taken into consideration in MNI estimates.

There are many ways in which MNI can be calculated. Consequently, a great range of resultant MNI values is possible, depending on which method is used. No matter which method is used, it is based on similar assumptions to those for calculating NISP and therefore suffers from some of the same problems. With the exception of the first assumption for NISP (see above), the other four assumptions are basic to MNI.

The MNI method has one great advantage over the NISP method. That is, that the variables it produces are essentially independent. The problem of interdependence associated with the NISP method is solved (Grayson 1973: 433; 1979: 203; 1984: 27-28). It is for this reason that the method can be used to assess the relative dietary importance of different taxa (Lyman 1982: 359-360), and why it is valid to use it in further statistical manipulation (Grayson 1973: 433). The method has other advantages over the NISP method. Basically MNI is strong where NISP is weak (Klein and Cruz-Uribe 1984: 26). The method is unaffected by the possibility of one species having more skeletal elements than another species in the same site. It is unaffected by differential butchering strategy. By this is meant that the introduction of a whole carcass or part carcass does not affect the MNI. Finally, compared to the NISP, it is relatively insensitive to differential fragmentation between species or faunas (Klein and Cruz-Uribe 1984: 26).

However, the MNI method is not without problems. Grayson (1978) has demonstrated that the MNI values calculated for an assemblage are a function of sample size (NISP). Because the MNI method is based on the NISP, it suffers from many of the same difficulties which have been found to flaw the NISP method (Plug 1984). Differential preservation of faunal remains between and within sites invalidates the MNI method for inter- and intra-site comparisons, until such a time as we can reliably compensate for this. Taphonomic studies have made major contributions in recent years, but we are still a long way from being able to calculate with a fair degree of accuracy specific bone loss from a site. Taphonomic factors can result in the NISP count for a particular bone being artifically high relative to other bones. This means that taphonomic factors can determine the skeletal element selected for the MNI count. The result is that the MNI method is often reflecting factors such as use of bone for manufacturing artefacts, element density, and differential disposal of elements, instead of the hoped-for measure of relative abundance (Gilbert and Singer 1982: 32; Perkins 1973: 368).

Another problem with the MNI method is that it over represents rare species (Grayson 1978: 54; Klein and Cruz-Uribe 1984: 32; Payne 1972: 69; Uerpmann 1973: 311). What is meant by rare species is a taxon represented by only a few bones. Without conversion to meat weights, the method also over emphasises the importance of small animals, if the values are taken at face value to represent relative dietary importance. For example, a site might produce MNI figures of 20 for rabbits and 2 for cattle. If we assume for argument's sake that the cattle and rabbits were totally consumed, then the cattle clearly would have contributed more meat than the rabbits and therefore would have been much more important in the diet. However, MNI figures, taken at face value, do not demonstrate this. In fact, if meat weight is not taken into consideration, the exact opposite to the correct conclusion would result.

A concern raised by a number of authors (Grayson 1973: 438; 1978; Jolley 1983: 65; Klein and Cruz-Uribe 1984: 26; Lyman 1979: 537; 1982: 359; Payne 1972: 69) is that because different researchers used different calculation methods, MNI estimates may not be comparable between different assemblages. Unfortunately, there are many different methods in vogue, and few authors stipulate the method they used to calulate MNIs in enough detail to allow comparability with other published data. Klein and Cruz-Uribe (1984: 28) have suggested a solution to this problem. They suggest that 'analysts regularly separate rights from lefts, that they employ only discrete criteria such as epiphyseal fusion (or the lack of it) in matching, and that they record fragments as specific fractions of whole bones.'

The most serious objection raised against the MNI method concerns errors arising from use of different units of aggregation. The MNI values can be calculated using one of three types of aggregation unit: arbitary excavation units, natural or cultural units of stratification, or the entire sample from the site can be treated as a whole. When different units of aggregation are used in calculating MNI for a single assemblage, it has been found that the greater the number of units used, the greater is the value of the resultant MNI (Casteel 1977: 126). Grayson (1973: 434) was the first western archaeologist to note that:

> ... the minimum number of individuals calculated from a set of faunal material from an archaeological site will be affected by the way in which this material is grouped ...

This effect had been discussed as early as 1958 by the Russian faunal analyst Paaver (Casteel 1977). The implication of this effect is that when MNI is calculated using different units of aggregation, then the results of that calculation are not necessarily comparable (Grayson 1973: 438). Realisation of this problem of aggregation has called into question the main advantage of the MNI method over the NISP method. That is: just how independent is the data? As Grayson (1984: 66) put it, 'minimum numbers are necessarily independent of one another only when one can be sure that the faunal aggregates from which they are defined are totally independent of one another.' It is suggested that natural and cultural units of stratification be used wherever possible, and that arbitary units not be used, as they contain the greatest potential for introducing the problems of interdependence into the MNI method. The problem of variation in the use of different units of aggregation removes much of 'the attractiveness of minimum numbers as the basic unit in the quantification of relative taxonomic abundance' (Grayson 1984: 67).

The MNI method that White (1953a) introduced to western archaeologists did not take into consideration the possibility that some of the lefts and rights of skeletal elements may not match one another and therefore an error is introduced into the calculation. White was aware of this problem when he introduced the method. As he put it:

> This may introduce a slight error on the conservative side because, without the expenditure of a great deal of time with small return, we cannot be sure all the lefts match all the rights.
>
> (White 1953a: 397)

This explanation for not considering the error introduced by unmatched pairs was given some credibility by Flannery (1967: 157) who expended 'a great deal of time with small return' in an attempt to see if all the lefts matched all the rights. White (1953a: 397) also rejected the calculation of MNI by dividing the sum of the most abundant paired element by two. He believed that this would 'introduce greater error because of the possible differential distribution of the kill (unless the site is completely excavated)'. However, this approach has been advocated by Binford (1978; 1984a).

Binford proposed this approach, which he has termed the minimal animal units method (MAU), because he believed that the archaeological record is more likely to evidence consumption of segments of animals, rather than that of whole animals. Thus it appears he hoped to solve one of the problems of White's method that Honerkamp (1981: 93) has pointed out, that the assumption that 'one element of an animal is equivalent to the entire animal being used and discarded at the site' is simply not testable and in many cases, especially for urban historical archaeological sites, blatantly false. Grayson (1984: 89-90) has provided a number of objections to the use of this method. The primary objection is that if one is interested in calculating segmental units then Binford's method provides inaccurate results. Grayson provides an example to demonstrate this point:

> How many femoral segments are present in the assemblage of 70 right and 30 left femora ... ? It is clearly not 70 (the minimum number count), but it is also not 50 (Binford's count). It is 100.

Chaplin (1971) presented a paired elements method for calculating the MNI. He termed this method 'the grand minimum total, GMT'. This method was basically a variation on that proposed by White. It varied from White's in that it took age and size features into consideration as a means of quantifying the age structure, to allow for estimating the numbers of matched and unmatched pairs in a sample of elements. This is believed to give more reliable MNI estimates. Sex was deliberately not taken into consideration due to the dearth of methods available to sex elements reliably, and the virtual impossibility of sexing fragments (Chaplin 1971: 71).

Like Chaplin, Krantz (1968) has provided a method using matched pairs of bones. In an assemblage, when left and right elements of the same kind can be paired off as coming from the same individual, then the MNI is eqivalent to the number of pairs for a specified element (that which yields the highest MNI values), plus the number of unpaired lefts and the number of unpaired rights. Use of this method yields MNI values higher than that obtained by White, and values which more accurately reflect the original number of individuals involved (Krantz 1968: 286). Krantz was aware of the problem that differential preservation between taxa within the same site could introduce error into standard MNI calculations. He was also aware of the problem in using matched paired methods, of how to compensate for loss of elements of a pair. What he really wanted to get was an estimate of the number of animals in the original population which was as close to the original figure as possible. Casteel (1977: 126) has noted that this approach to quantification has rarely been applied in faunal studies. The reasons for this are varied. Firstly, as noted by Flannery (1967), methods involving the pairing of elements are very time consuming. Secondly, the validity of the method has not been proved. When Casteel (1977) tested the method with empirical data, it was found to produce distorted results. Finally, Grayson (1984: 87-88) has stated in regards to this method and two other modifications of the method (Fieller and Turner 1982 and Nichol and Wild 1984), that 'Pessimism is fully appropriate.' For a detailed explanation as to why these methods should be viewed with pessimism, see Grayson (1984: 87-88) and Klein and Cruz-Uribe (1984: 36).

Bökönyi (1970) proposed that paired methods should take age and size into consideration. In the method he advocated, he determined if left and rights of an element matched on the basis of whether or not they came from the same age/size category. Bökönyi defined twelve age/size categories which he regularly used. These categories were determined on the basis of first dividing a set of elements into four age classes (juvenile, sub-adult, adult, and mature and senile). Each of these four age classes was subsequently divided into one of three size categories (small, medium and large), thus arriving at the twelve age/size classes. Criticisms which can be directed at this method are:

1. That it is time consuming (although this is not a fatal objection to the use of the method).

2. The degree of subjectivity involved in deciding in which size category a bone should be placed.

This second point has been mentioned above, a possible solution would be to introduce objective measures in the allocation of size categories.

As indicated above, the advantages and disadvantages of the NISP and MNI methods mirror one another in the areas of independence and aggregation. For this reason, both Klein and Cruz-Uribe (1984) and Grayson (1984) have recommended exploring the relationship between NISP and MNI. Investigations experimenting with the relationship between NISP and MNI have revealed that these two counts are related to one another in a very predictable way. Grayson (1984: 67) has stated that this relationship can be predicted by the formula:

$$\text{MNI} = a\,(\text{NISP})^b$$

In this formula a and b are constants which have to be determined. As a result of this relationship, Grayson (1984: 62-3) has stated that:

> the information on relative abundance that resides in MNI counts generally resides as well in the NISP counts, and if relative abundance is the target of analysis, there would seem little reason to spend the time and effort to calculate minimum numbers.

Unfortunately, like all the other procedures looked at thus far, this relationship between NISP and MNI has problems and, therefore, the results of the investigations into NISP and MNI must be viewed critically. One of the major difficulties in predicting MNI from NISP, and thus the validity of the hypothesis that NISP reflects relative abundance, is that of determining the constants a and b. The problem arises from the fact that the relationship between MNI and NISP is exremely sensitive to differential aggregation (Grayson 1984: 66). The relationship is also sensitive to other factors such as sample size. The ratio of NISP to MNI determines the number of bones per individual for a given taxon, while the ratio MNI to NISP determines the number of individuals defined per bone for a given taxon. It has been suggested that these ratios may be of some value in answering such questions as: 'Has one species been more heavily butchered than another? Has a larger proportion of the skeletons of the individuals of one taxon than of another been deposited in a site?' (Grayson 1984: 66).

The MNI measure, unlike the NISP measure, is fundamental to assessing the dietary significance of different taxa present in a site. White (1953a) introduced the MNI method to his research because he was interested in finding out the percentage that each species, in the sites that he was studying, contributed to the diet of the occupants. He was aware of the problem of over representation of small animals if MNI values were taken at face value. For this reason he advocated converting the MNI figures into relative meat weight percentages (White 1953a: 397). The calculation of MNI by White can, therefore, be seen as a means to an end, the real goal being the calculation of meat yield per taxa in order to determine relative dietary significance. It is important to note at this point that the MNI numbers calculated should not be seen as absolute figures, but as numbers to be converted into relative frequencies of abundance of the different taxa present in a site. Furthermore, this relative abundance must be seen as representing the whole temporal span from which it was excavated, and not a reflection of daily consumption, or the average meal (Wing and Brown 1979: 126).

White calculated meat-weight values for the various taxa using the formula:

MTWT = MNI x (% live weight representing meat) x (average live weight per individual)
(Lyman 1979:537)

where MTWT is what White (1953a: 397) termed 'usable meat'. Lyman (1979: 537) has more correctly defined it as 'available meat'. The percentage of live weight representing meat was based on percentage meat yields from domestic animals gained by Euroamerican butchers and also from concepts of what constitutes consumable meat. At this point it is necessary to digress and define the terms which are being used here. This is necessary because in the past some of the terms have been used interchangeably with other terms. Lyman has given the best definitions for the terms used in discussions concerning meat weights. Lyman (1979: 536) has stated that:

> *Live weight* is the weight of an animal while alive ... *Available* meat is defined ... as all parts of an animal exclusive of bone and hide, that is, *live weight* minus bone and hide weight. *Consumed* or *consumable* meat is here defined as those portions of the available meat of a species that are/were consumed by the group under study. Available meat includes muscle tissue, fat, viscera, brains, marrow, eyes, blood, etc.
>
> (Emphasis in original)

It will be necessary to define consumable meat explicitly for each and every site in order to permit realistic comparisons between sites. An objection can be made against White's formula in that it does not take differing concepts of consumable meat into consideration (Lyman 1979: 537).

The above is but one objection to White's method. There are many more but they all have a common theme. The objections stem from the generalised nature of the formula. The problem is that the formula does not take factors such as age, sex or season into consideration when calculating meat weights. That these are important factors has been demonstrated by a number of researchers (Smith 1975; Stewart and Stahl 1977). Stewart and Stahl stripped meat from carcasses of known weight, and then weighed this meat in order to calculate the percentage of live weight representing meat. These percentages were based on a pure available meat weight figure, but not a pure live weight figure, as Stewart and Stahl did not always have the hide attached to the carcasses that they were studying. This is, however, an error which can be compensated for. Their study produced results which indicated that White's percentages were consistently high (Stewart and Stahl 1977: 269). They concluded that the reasons why White's percentages were higher than theirs was his reliance on the use of modern domestic stock figures for calculating the percentages, which may not be suitable for wild animals, as domestic animals have been bred for producing high percentages of available meat relative to live weight. Furthermore, the weights of animals and the quantity of the meat and fat on them vary with seasons, age and sex. Stewart and Stahl (1977: 269) concluded that 'uniform edible meat proportions were not obtained for wild animals'. This is a very serious problem for historical archaeological fauna, because, as Jolley (1983: 65) has pointed out, there is far greater variation in individual weight between domestic animals, than between wild animals. Smith (1975) clearly demonstrated, using white-tailed deer as his example, that age and sex differences can cause considerable variation in estimated live weights using White's method. He proposed that age and sex characteristics be taken into consideration in any estimate of live weight. The difficulty in this, is that it is not always possible to assign faunal remains to an age class, let alone to a sex class. Furthermore, comparative data on how live weight correlates with age and sex are to date only available for white-tailed deer (Wing and Brown 1979: 126-127). Despite these objections to White's methodology, other researchers have advocated methods for calculating meat weights which would appear to differ little from White's and contain all the same difficulties.

Like White, Perkins (1969; 1973) has developed a methodology for determining the relative abundance of different taxa. Like White, this was but a first step in a two-step process towards a calculation of meat weight. Unlike White, whose calculation was of relative dietary importance, Perkins was attempting to calculate total meat weight per species, as represented by the faunal remains. Perkins (1973: 369) has termed his relative abundance figure the relative frequency of occurrence of each species (rf). This is calculated using the formula:

$$rf = \frac{f}{\Sigma f}$$

The frequency (f) is calculated using the formula f = B/A where A is 'the number of diagnostic elements least affected by cultural or preservational factors' for each species and B is the sum of these figures for the different species. Therefore, the relative frequency of a species is calculated by dividing the frequency per species of elements least affected by cultural or preservational factors, by the sum of that frequency for all the species present. Once the relative frequency of a species has been determined, the total meat weight per species is calculated using the formula:

$$TMW = rf \times (\text{average live weight} \times 50\%)$$ (Perkins 1969: 177).

The criticism of White's method by Stewart and Stahl (1977), Smith (1975) and others, that the method uses a set of untested average live weight percentages which do not take factors such as age, sex and seasonal differences into consideration, equally applies to this methodology. Furthermore, the determination of elements affected by either cultural or preservation factors seems far too subjective and open to errors resulting from different analysts having differing views as to which bones (even from the same assemblage) are, or are not affected.

The Wiegemethode or weight method has been used on archaeological faunal assemblages, as a means of determining relative taxonomic abundance in terms of meat weight (Casteel 1978: 71). It has not been used on a regular basis (Gautier 1984: 242), and has been applied to only two studies of fauna from historical sites in North America (Jolley 1983: 65). The method is based on the assumption that there is a fixed relationship (within an acceptable range of error) between the weight of the identified bones and the available meat of the taxa under question (Casteel 1978: 71; Chaplin 1971: 68). Available meat weight figures should be used because 'of the diurnal variation in live weight of an animal due to the ingestion of food and the longer variations in weight due to conditions such as pregnancy' (Chaplin 1971: 68). Another reason for their use is the difficulty in calculating consumed meat in many situations.

The method produces its meat weight figures, which are used as indicators of relative taxonomic abundance, by multiplying the archaeological bone weight by the appropriate constant. Constant percentage values of bone weight to live weight of 5·6 to 9 per cent have been suggested by some researchers as the appropriate percentage to use (Wing and Brown 1979: 129). Reed (1963) was the first to propose this method using a figure of 7·5 per cent (Wing and Brown 1979). In this case the relative meat weight abundances of the different taxa can be calculated by the formula:

$$rMWA = \frac{LW}{\Sigma LW}$$

In this formula rMWA is the relative meat weight abundance, LW is the live weight figure for the taxon in question represented by the faunal remains and ΣLW is the sum of the live weight figures for the different taxa present. Live weight (LW) for each taxa is calculated using the formula:

$$LW = \frac{BW}{a} \times \frac{100}{1}$$

In this formula a is the constant (percentage value ranging between 5·6 and 9), and BW is the weight of archaeological bone for a specific taxon. The simplicity of this approach must be balanced against the relative inaccuracy of results.

There are, as for other methods, a number of variants based on the theme outlined above. For example, instead of calculating meat weights for each of the species represented in the site, this could be done for each bone type, or else for all the bone from the site as a whole (Chaplin 1971: 67). Casteel (1978) gives a summary of the various bone weight methods which have received attention in the literature. All these methods suffer from a range of problems, which is the reason why these methods have proved to be unpopular with archaeologists.

The most commonly cited objection to the use of the weight method revolves around taphonomic issues which alter the original weight of bone deposited. The constants used in the method are based on modern comparative data, and it is assumed that this data is applicable to archaeological bone. Any factors which alter the weight of bone from that originally associated with the meat people utilised will invalidate the assumption and thus the method. Unfortunately there are a number of taphonomic factors which alter the weight of deposited bone. Those most often referred to in the literature concern differential fragmentation, differential mineralisation, differential leaching, differential weathering, differential burning, and differential preservation. Fragmentation becomes an issue when it exaggerates the importance of a species, when its elements are more identifiable in a fragmented state than equally fragmented elements of another species (Klein and Cruz-Uribe 1984: 35). Furthermore, fragments of different sizes are differentially preserved (Binford and Bertram 1977), recovered and identified. The method will also exaggerate the importance of those taxa whose bones are more susceptible to mineralisation (increasing bone weight) or resistant to leaching (decreasing bone weight). Both differential weathering and its opposite, differential preservation, and differential burning, will cause original bone weight to alter between taxa, and within and between sites, making intra- and inter-site comparisons invalid.

Further objections have been made to the method on the validity of using modern comparative data. Modern domestic animals show marked variation in the ratio of bone weight to meat weight, between breeds and amongst individuals within breeds (Uerpmann 1973: 311). So far, the range of this variability has yet to be calculated, even for the main domesticates for which we have the most data. Furthermore, the validity of using modern data (given the vast improvements in available meat weights resulting from selective breeding) as an indicator of the bone/meat ratio in the past has yet to be demonstrated. In addition, it cannot be assumed that the bone/meat weight ratio is something which remains constant once an animal has reached maturity. There is every reason to believe that this ratio varies depending on age, sex, nutritional state and health of an animal. Also the ratio will be different for different bones in the body. A possible part solution, though extremely time consuming and as yet not possible, would be to determine the age and sex for each element of a taxon and then calculate the meat weight that each of these elements represents. Clearly, even if it were possible it is not practical.

The most damning piece of evidence against the bone weight method is that it assumes a linear relationship between meat weight and bone weight, and this can be shown to be false (Casteel 1978). When it was first realised that there were problems with the meat weight method, it was thought that the difficulties lay in determining the correct linear factor in converting bone weight to meat weight (Grayson 1984: 172). It is now realised, however, that no such correct linear factor could be found, because, as Casteel (1978) has demonstrated, the relationship is in fact allometric or curvilinear. Grayson (1984: 172) has succinctly summarised this point:

> Thus in order to estimate meat weight from bone weight, one may not use an equation of the form $Y = a X$, where Y is the predicted meat weight of the taxon involved; X, the weight of bone for that taxon; and a, a constant to be determined empirically. Instead, the actual relationship for individuals is of the form $Y = a X^b$, where both a and b are constants to be determined empirically.

Casteel (1978) tested the hypothesis of a linear relationship using data from domestic pigs. He concluded that predictions made using the weight method were highly unreliable as the error associated with them could range from 28% to 2243%. This is a direct result of the relationship being curvilinear and not linear as previously thought. Casteel also demonstrated, using his data on domestic pigs, that the relationship applies to individuals and not to composite aggregations of the faunal remains. Let us consider, for example, a small sub-adult animal having bone weight X_{bw}, yielding meat weight X_{mw}, and a large adult animal having bone weight Y_{bw}, yielding meat weight Y_{mw}. If the two bone weights (X_{bw} and Y_{bw}) are combined to give bone weight Z_{bw}, the resulting meat weight Z_{mw} will not be equal to X_{mw} plus Y_{mw}. This is because the relationship between bone weight and meat weights is exponential. It is thus inappropriate to use aggregates. Since it is impossible to separate bones from an archaeological assemblage into specific individuals the bone weight method is rejected as a valid means of inferring meat weight.

It has been demonstrated that the traditional indicators (NISP, MNI and bone weight) of relative taxonomic abundance, and relative dietary contribution (in terms of meat weights), are plagued by theoretical flaws and methodological impracticalities. For these and other reasons, the use of allometric models to elucidate relative abundance and relative dietary contribution look more promising. Allometeric models take into consideration the fact that the relationship between bone weight or skeletal dimensions, and body mass is not linear but exponential. Use of 'allometric models places original body mass predictions on a more sound biological basis' (Reitz et al. 1987: 304). The reason for this is that skeletal mass and skeletal dimensions are scaled allometrically to body mass. It is a biological law that size must relate to shape and function (Reitz and Cordier 1983: 246). As body mass increases, there is also an allometric or exponential increase in the proportion of skeletal mass and a proportional increase in the dimensions of bones. Therefore, the most useful data for archaeologists are 'bone weight and linear dimensions of weight bearing skeletal elements' (Reitz and Cordier 1983: 238).

Since the relationship which exists between body mass and bone weight, or body mass and skeletal dimension, is exponential, it can be expressed by the formula:

$$Y = \beta X^{\alpha} \qquad \text{(Simpson et al. 1960: 396-397)}$$

In this formula, Y is the dependent value, the archaeological unknown, such as live weight or available meat weight. X is the independent value, the archaeological known, such as total bone weight or the specific dimension of a specific element. The values for α and β are obtained from comparative skeletal collections where the live weight of the specimen is known (Reitz et al. 1987: 305). From this it can be seen that two values can be calculated from the data, either live weight or available meat weight. The exact requirement will decide which archaeological known or X is used. If live weight is required, then bone weight is used for X, or if available meat weight is required, then a linear dimensional measurement is used for X (Reitz and Scarry 1985: 18).

The method has a number of advantages:

1. It is based on more sound biological principles than the other methods discussed so far.

2. It eliminates the need to calculate average weights, and thus eliminates much of the criticism directed at the bone weight method and meat weight estimates calculated from MNI.

3. It offers the potential to derive nutritional components for the identified taxa from the derived meat weight estimates.

These advantages have been recognised by historical archaeologists. Reitz and Cordier (1983: 248) have used the method to assess the relative dietary importance of marine and terrestrial resources for two historical sites from the Atlantic coastal plain of the United States. Reitz and Honerkamp (1983) have also applied dimensional allometry to cattle, sheep and pig remains from an historical site.

Allometric methods seem quite appealing, and much optimism is expressed in the literature (especially historical archaeology literature) about their application to faunal assemblages. They are, however, not perfect. To date, one of the main reasons why the method has not been applied to more assemblages is that the necessary values for α and β

are not always known, or else the data base from which they have been extracted is not sufficiently large for analysts to place faith in the results. This problem is solvable, through the calculation of values for α and β based on a larger data base. Application of allometric methods in archaeology is relatively recent and therefore time will be required in order to collect more data, so that more reliable values of α and β can be calculated. The skeletal mass allometric method also suffers from many of the problems of the bone weight method. Like the bone weight method, the skeletal mass allometric method assumes that there is no change in the weight of bone once it has been deposited. This assumption cannot be substantiated and in all likelihood is erroneous. Skeletal mass allometric and dimensional allometric methods can be applied only to individuals, and cannot be applied to composite aggregates of fauna, for the same reasons as were put forward for the bone weight method. The near impossibility of determining individuals from the assemblage is a serious drawback to the success of this method. Finally, a problem possibly more relevant to historical archaeology and urban historical archaeology than to prehistory, is that the method assumes the utilisation of whole carcasses. This is almost certainly not so for the majority of historical archaeological sites. The problem is potentially solvable by modifying the method so as to produce meat weight figures for carcass portions, rather than for the whole individual. A final problem which may be encountered using the dimensional allometric method, is that preservation and butchering factors may affect the sample size of elements able to be measured (Lyman 1979: 538).

To conclude this discussion on allometric methods, dimensional allometry promises to be the most reliable method for quantifying faunal remains so far advocated. In several preliminary studies it has been shown to predict live weight much more accurately than any of the MNI methods or the bone weight method (Reitz and Honerkamp 1983: 15). This method 'involves fewer assumptions than the other methods and has the advantage of being based on empirical biological principles' (Reitz and Honerkamp 1983: 15). Casteel (1977: 133) has, however, stated that until more detailed experimental testing of allometric measurements are conducted and this method is demonstrated to produce consistently reliable results, then the 'encouragement of development and testing of new approaches should be urged.'

It has been noted in the above discussion that one of the difficulties involved in the quantification of faunal remains is the invalidity of the assumption that whole animals were consumed. Binford's (1978;1984) MAU method was an attempt to solve this difficulty, but this method did not achieve its goal. Lyman (1979) has also produced a method to deal with this difficulty, which has been applied to historical sites (Branstner and Martin 1987;

Lyman 1979). This may be termed the 'butchering unit method' after the term Lyman gave to his basic unit of analysis. The method will be discussed briefly here, a more critical appraisal will be given in Chapter 8.

The use of the method takes into consideration the assumption that on historical sites, especially urban historical sites, and even prehistoric sites, meat arrives and is consumed on the site in units smaller than whole animals. This means that historical archaeologists have a method that corrects in its methodology for the purchasing of units of meat instead of whole carcasses, and therefore gives far more reliable estimates of relative taxonomic abundance and relative dietary importance in terms of meat weights. The method is applicable to all the larger domesticates (cattle, sheep, goats and pigs). However, it is suggested that the method is not applicable for smaller mammals (such as rabbits), birds (such as domestic chicken, geese, ducks, turkeys, etc.), and fish. For taxa which fall into this size range it is recommended that they be considered as whole individual units instead of part units, as they would in all likelihood have been cooked and consumed as whole animals. In order to determine whether to analyse a taxon by whole or by part units, it would be necessary to decide whether in the culture from which the faunal remains derived, it was culturally acceptable and technologically possible to cook the animal whole. Furthermore, does the NISP indicate full skeletal representation? This last factor requires that taphonomic variables resulting in differential recovery would have to be taken into consideration. If the answer to the above questions were in the affirmative, then the taxa should be examined by whole units, not by part units. The criterion is feasibility in cooking the taxon whole or not.

Lyman (1979: 539) defined his butchering unit as being that 'piece of the animal body that results from the act of butchering'. It was felt by Lyman that these units would give a much more reliable indication of consumed meat. Lyman was concerned with getting a better picture of what meat was actually consumed, rather than live weight figures or available meat weight figures. If we are honest, it is this consumed and not available meat weight that as archaeologists, we, are interested in. This does not suggest that exploring differences between available and consumed meat does not produce interesting and important inferences about a culture. Lyman has stated that his method offered more potential for studies concerned with diet than those other methods in vogue at the time that he proposed the butchering unit concept.

Lyman's butchering units are determined by 'explicit delineation of the butchering techniques evidenced by the bones' (Lyman 1979: 539). Lyman has noted that it may not be possible to determine the butchering pattern at some sites, and thus butchering units will not be definable. In the situation where butchering units cannot be defined, Lyman has suggested the use of a variation on his proposed butchering unit method. Skeletal portions are substituted for butchering units. Lyman defines skeletal portions as being 'some arbitarily defined part of the body. For instance, a quadruped carcass may be conceived of as consisting of the following skeletal portions: forelimbs, hindlimbs, cranium, rib cage, and vertebral column' (Lyman 1979: 539).

Both the butchering unit method and the skeletal portion method quantify faunal remains in much the same way as White (1953a) did. These two methods are merely further refinements of the MNI method. As such, they suffer from the same difficulties of differential preservation, sample size, aggregation, and so on that affect all the variations and refinements of White's original method. Therefore, any resultant meat weights using either the butchering unit or skeletal portion methods 'must be viewed with some caution' (Lyman 1979: 539).

It is unfortunate, but not only MNI methods 'must be viewed with some caution' but the methodology of calculating meat weights as a whole. Over recent years the literature concerned with how to calculate the relative importance of different taxa in the diet has revealed more and more theoretical and methodological difficulties (Grayson (1979: 226-227). Considerable energy has been expendended in attempting to solve the problems associated with quantification methods with varying degrees of success. Other approaches at interpreting faunal assemblages may yield more accurate behavoiural information. Butchery analysis is suggested here to be such an approach and one which deserves more attention by faunal analysts.

4·4 Concluding remarks

To say that there are problems with the method and theory involved in faunal analysis would be understating the enormity of the difficulties currently faced by zooarchaeology. These problems are not new. They have been with the discipline since its inception. What is new is the realisation that they are there and that something has to be done to remedy the situation. Central to the problem is a failing by archaeologists not directly involved with faunal analysis, to realise the limitations of the data and the flaws which exist in the methods used to extract meaning from faunal remains. Unlike many other material remains which are revealed by excavation, faunal remains generally have a high degree of identifiability. Just because faunal remains can be reliably identified to skeletal element and species, does not mean that we can expect to extract more information from bones than from other classes of material remains, which have similar, or worse, problems in analysis and interpretation. It appears that we are extrapolating far more information from faunal remains than they allow for. This point is obvious if one reads even a small portion of the ever increasing literature concerning taphonomic affects on faunal assemblages. It is not just that we are dealing with a reduced sample because of taphonomic factors, we are dealing with a sample that is also biased.

Although it appears that there are no reliable quantification techniques available at the present, the problems faced in quantification are realised and researchers are tackling the problems. However, the solutions are some way off at present. That faunal analysts who work with historical faunal remains are actively involved in testing new methodology promises remedies in the future. This is because of the tighter controls on the variables that influence the patterning of a faunal assemblage that are available to the historical archaeologist from written and oral sources. Historical archaeology, along with ethnoarchaeology and experimental archaeology, offers the ideal framework in which to test the theory and methodology of faunal analysis. This is especially the case in butchery analysis, which promises to produce fundamental quantitative results which give incidental qualification of the sizes of consumed portions of meat.