

**ARCHAEOLOGISTS AS ARTEFACTS. TOWARDS A
BIOGRAPHICAL UNDERSTANDING OF FORMATION
PROCESSES IN THE HISTORY OF AUSTRALIAN
ARCHAEOLOGY**

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Statement of Originality

This is to certify that to the best of my knowledge the content of this thesis is my own work. This thesis has not been submitted for any other degree or other purposes. I certify that the intellectual content of this thesis is the product of my own work and that all assistance received in preparing this thesis and sources have been acknowledged.

Signed:

Michael David Lever

Abstract

Archaeologists traditionally think of the past through things; through a base unit of analysis that is the artefact. In this thesis I argue that the basic unit in the study of the history of archaeology can also be archaeologists themselves, functioning as artefacts of the discipline. Further, that many of the analytical tools applied to the study of artefacts apply well metaphorically, to the study of the discipline as a whole. I argue for the need to evaluate the individual archaeological practitioner with reference to a collective assemblage - their community of practice, and also to observe and critique the interplay between individual practitioners. In this thesis I examine three liminal individuals in the history of Australian archaeology; Dermot Casey, Elsie Bramell and Alexander Gallus and I highlight the nature of their relationship to the discipline as a whole.

The level of in-depth study I have applied to each of my case studies is necessary to tease out sufficient contextual understanding of individual circumstances. My findings from these three case studies is sufficient to robustly challenge the existing paradigm of the development and growth of Australian archaeology. To date, histories of Australian archaeology have portrayed the discipline as shifting abruptly from an amateur to professional state with the excavation of Fromms Landing by John Mulvaney in 1956. This chronology is grounded on Mulvaney's own history of the discipline, and several subsequent works that largely accept Mulvaney's proposition. My work indicates that this amateur-professional divide is untenable, as persons fitting either category of amateur or professional were active in Australian archaeology throughout the 20th century. I contend that the primary factors that have shaped Australian archaeology through the 20th century were biographical and social forces, enmeshed in and reifying archaeological disciplinary culture.

While writing these biographies, I have been cognisant of the need for reflexivity. I have reflected on my own biographical influences in approaching my topic as much as I do in evaluating the effects of biographical factors on the archaeological careers of the three case study individuals. This is an intimately personal thesis – from my lived experience, to my analysis of other lives, for evaluation by you.

Acknowledgements

This thesis has been a long time in the making, and I will restrict acknowledgement to those directly involved in its formulation, else the acknowledgements would be the longest chapter of the thesis. In chronological order I would like to thank my parents - my mother for teaching me to read at a precociously young age, and my father for demonstrating lifelong by example that reading can be one's primary form of recreation. To the late Professor Alan Crown, who in our conversations over my shop counter as I sold him bagels and lox on Sunday mornings, gave me the idea of entering academics as a mature age student, and to his departmental successor Professor Suzanne Rutland who generously eased my way into the academic world. Thanks to Professor Roland Fletcher for guiding me in my initial engagement with archaeological theory and its history, and to Professor Tim Murray in encouraging me to take this study further.

Particular thanks are due to the late Professor Bruce Trigger, a man of both outstanding intellect and humility, for first patiently responding to email questions on theory from this Australian university student, and then equally patiently tolerating my enthusiastic excesses during my time studying under him at McGill University. Thanks are also due to Hilary du Cros for providing me with a copy of her honours thesis, and to her and staff at the Australian Museum for facilitating electronic delivery to me of recorded material. I am also grateful to the school archivists at Eton, England, and St George High School, Kogarah for their generous provision of images and information. Gratitude is due to Dr Alexander Gallus Jr, who very kindly responded fully to my 'out of the blue' enquiries regarding his late father. Bilquis Ghani is a friend I first met 20 years ago during our undergraduate years, and who while completing her own PhD steadfastly stood by me with her husband Ferdinand through my mental health issues, covid, and life in general. My thanks cannot match the generosity shown.

My supervisors at The University of Sydney have been a great source of academic and personal support. Professor Annie Clarke and Dr Steve Brown have both repeatedly gone out of their way in assistance, particularly, whenever possible, to ensure that the university has met my needs, not solely the reverse – as I will describe in the following section.

A note to the troubled reader

This is not the thesis that it could be, nor the thesis that I feel it should be, or would be at best. But as the Australian aphorism goes; *could, would and should were great horses, but the race is over*. This is the best thesis I could manage while struggling with at times debilitating mental health conditions which saw me suspend candidature more than once. I do not raise this to seek consideration in your critique of my thesis. Only to assure you if you too are struggling, that with the appropriate supervision and health support, a PhD thesis is not an impossible goal even if life is cruel at times.

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Chapter 1: A personal introduction

1.1 A WORD ON NAVIGATION

There are parts of this thesis – such as the following chapter, which may not read in the manner of a thesis at all. And that is good and intended, as what I hope to do in this work is to ask some questions about the history of archaeology in Australia that may not have been previously asked, and to do so in a manner and from a perspective not previously adopted. A chief question is what would a better understanding of the personal lives and interaction of archaeologists lend to our comprehension of how Australian archaeology has developed?

It does take a while (2 chapters) before I have sufficiently set the scene to address the three case studies to which I am constrained by space. I hope I have not only brought these individuals to life for you, but more importantly have raised sufficient evidence and questions to allow for the serious queries that I raise at the end of this thesis regarding the tenability of histories of Australian archaeology as they currently stand.

1.2 CIRCULAR TIME AND NARRATIVE

I must admit that I almost never read the introductions to any book. In my eagerness to dive into their main content and in my self-assuredness that I will independently derive the author's intent from my reading, I generally dispense with reading introductions altogether. There is certainly a degree of arrogance in this – in my perception that it is what I as a reader derive from the text that is important, rather than the meaning to which the author might steer me in the introduction. Having said this, and nevertheless at the same time being constrained by academic protocol to provide an introductory chapter to this thesis, I feel obliged to both myself and to the reader to provide content in this introduction that is something more than just a re-presentation of content that is already found in the thesis itself. I hope what follows here provides insight to the personal motivation behind undertaking this thesis, into the topic I have chosen and the manner that I have adopted. In any event, I hope this thesis proves to be a 'readerly text' (Barthes 1970) not only because I would like to engage the reader with my thinking, but also out of the conviction that unless I am able to express my ideas in a straightforward and easily comprehended manner, then there is almost certainly something lacking in my understanding of my own thesis.

It was naïve of me to think that I could undertake a thesis on biography without being brought to deep self-reflection on my personal understanding of the nature of time, life and the passing of both. If nothing else, this process of reflection has brought me to acknowledge the conditioned responses and ideas that abide within me as a result of having spent most of my life in a relatively sequestered ultra orthodox and Hassidic Jewish community.¹ It has led me to recognise that although my immersion in the secular academic world over the past twenty years since I left the Hassidic community has been a fairly thorough one, and that I have currently an agnostic humanistic perspective worldview, there are nevertheless underlying embedded conceptual structures that have continued to fundamentally shape my approach to studies that involve the passing of time - and the understanding of time itself. These realisations have only served to reify my perception that biographical approaches are indispensable to understanding the formation of individual careers and disciplinary movements - the central theme of this thesis. I will briefly outline some of these background influences on my thinking here.

As a child and later an adult. I was not, at any time, physically cut off from the wider secular world around me. Rather, from early childhood an entire suite of Jewish conceptual and behavioural norms were entrenched as normative. The outside secular world was depicted as aberrant, or only to be tolerated in so far as was necessary to allow Haredi lifestyle to continue to operate in the manner that it considered itself to have operated for several millennia. School was needless to say segregated into separate institutions for boys and girls, and my school ran on the principle that at least as many hours in the day should be spent on religious studies as were spent on secular studies (comparable schools overseas where government restrictions were not as stringent, often saw non-religious studies dispensed with altogether). This made for long school days. By high school the day commenced with morning prayers at 7.30 am and concluded by 5.30 pm, after which one was supposed to make one's way promptly home for dinner and homework. The environment was incredibly cloistered, and language played a large part in this segregation. Classical Hebrew was one's second (or first) mother tongue, being

¹ Ultra-orthodox is not a term used in the orthodox Jewish world. The term used is Haredi, which translates literally as those who quake. Much like the reasoning behind the English term Quakers, the word conveys a sense of dedication beyond the letter of the law. Hassidut (Hassidism) is a spiritual Jewish revival movement (individual Hassid, plural Hassidim) that commenced in the 18th century. A Haredi level of religious observance is assumed among Hassidim. One may be Haredi without being a Hassid, but not a Hassid without being Haredi.

taught as integrated into daily life from the earliest time one could speak and was heard around one from birth. Classical Hebrew formed the basis and subject of religious instruction from kindergarten through to the end of primary school, at which time Aramaic texts were introduced, along with modern Hebrew. Throughout this period, Yiddish operated in the background as a vernacular, until one reached tertiary Yeshivah² stage when Yiddish often became the formal language of instruction and also the language of various texts to be studied.

At the tertiary Yeshivah, the compulsory hours of attendance at the study hall were 7.00 am to 9.30 pm with about a total of two hours of breaks for meals during the day. Leaving the Yeshivah premises was forbidden other than for pre-approved purposes and the Haredi community acted as 'spotters' for the Yeshivah administration in reporting Yeshivah students seen outside grounds at times or in places that they were unlikely to have had permission for. A fairly impermeable social sphere was therefore developed in which time constraints, language insularity and religious commandment all combined to create a world where the non-Jewish outsider was considered so foreign that one firstly would not feel confident approaching them socially, and, secondly, one would fear divine and social retribution for breaching the religious bubble one existed in. In such a setting it is hardly surprising then that worldviews and understandings could be firmly entrenched, even where they differed greatly from those of the surrounding society. At the time I left the Haredi world, I had operated under the belief that the universe was 5,761 years old. Not only had a lot more happened in that short period of time than in the billions of years that astronomers and western science generally spoke of, but the nature of that time differed significantly to western scientific time too.

1.3 DREAM TIME AND TALMUDIC TIME

My awareness that my personal conceptualisation of time differed from the western mainstream was sparked by a number of instances in reading anthropological literature and in discussion with Aboriginal people, particularly around the conceptual reality that has never been satisfactorily translated into English - the Dreaming, or Dream Time. The nature of the

² Yeshivah lit. 'place of seating' refers to a school where all or most of the syllabus consist of traditional Jewish knowledge taught through Jewish pedagogic means, to males between about 15 and 25 years age. The first Yeshivot are documented to the first centuries Before Common Era (BCE). Women's learning institutions are a newer institution, dating from the later 19th century and bear a variety of titles other than Yeshivah.

relationship between the present and the Dreaming (if the two can be divided) resonated strongly with me, and these encounters with the Dreaming made it apparent that my inherent understanding of time is also often a cyclical or spiralling one, as is characteristic of the Dreaming, rather than solely the linear transect of time characteristic of western thought. It has been startling to recognise the similarities between Aboriginal Dreaming and traditional orthodox, particularly Hassidic, Jewish perceptions of time and its directionality. W E H Stanner refers to the Dreaming, as 'everywhen', a state of being that is not fixed to the past or present (Stanner 1979, p.4) and much Haredi Jewish thought and life takes place in a similar cosmology where the past lives in the present and the present enlivens the past. Even now, I frequently find myself in an 'everywhen' of mental discussion with departed friends and mentors who seem to be just as present as during their physical lives.

This understanding of fluidity of time in Jewish thought and the permeability of the past / present relationship is not something that is intensely discussed or written about in everyday Jewish literature or life - it is an inherent part of cultural knowledge that is simply 'there' in the same manner that the Dreaming is 'there' for Aboriginal people, and only requires translation or exposition for outsiders. Although the notion of time as a created and malleable entity does receive considerable attention in the school of mysticism and subsequent Hassidic thought deriving from the Lurianic movement,³ this exposition is within a body of literature largely unstudied other than by Jewish adherents to specific Hassidic schools. In the following few paragraphs I will provide selected examples of the Jewish conceptualisation of non-linear thought as based in literature more widely studied than Lurianic thought, but more importantly also from practice and custom.

A powerful documented instance of this traditional Jewish mode of thinking about time and heroic episodes in time as non-linear is provided in the Old Testament Book of Esther and subsequent commentary on it. The Book of Esther was written during the 4th Century Before Common Era (BCE) (Berlin, Brettler, & Fishbane 2004). Towards its conclusion (Chapter 9: 28)

³ Isaac Luria (1534-1572) was a leading rabbi and mystic in Safed, then Ottoman Syria. He is considered a father of contemporary Kabbalah, the mystic aspect of Jewish thought. His teachings formed the core of many Hassidic teachings as transmitted by the founder of Hassidism, Rabbi Israel Baal Shem Tov (1698-1760). Hassidism itself diverged into multiple schools. The school which placed most emphasis on open study of Lurianic literature (Chabad or Lubavitch Hassidism) was led between 1767 and 1994 by male members of the Schneersohn family.

The Book of Esther contains the stipulation to remember and celebrate the events that it depicts, using the phrase: "וְהַיָּמִים הָאֵלֶּה נִזְכָּרִים וְנַעֲשִׂים בְּכָל דּוֹר וָדוֹר". This can be translated as "these days shall be remembered and celebrated in every generation" but is more literally translated as "these days shall be remembered and recreated in every generation". The implication of the latter translation is that through remembering and celebrating events that occurred in the 'past' such as those in the Book of Esther, particularly through the physical re-enactment that the traditional festive practices contain, these events actually reoccur in the present.

Of greater similarity to Dreaming is the notion that through re-enacting these events of the Book of Esther in the present, the events referred to as 'in the past' are themselves reinvigorated and occur 'again' in the heroic past (Schneerson 1998, p.51).

Such fluidity in notions of time is further reflected in later Talmudic writings. The Babylonian Talmud was codified between 500 Common Era (CE) – 700CE and is the core text of Jewish jurisprudence and religious law. The Talmud takes this theme of human participation in recreating events further than the example provided above, stipulating that by observing the weekly Shabbat (sabbath) day with its ceremonies and performances, one becomes a partner with God in the creation of the world. This partnership is both through a renewal of creation in what would be referred to as 'the present', by infusing the present world with enriched spirituality. However, in addition to this, the human being currently observing the Shabbat retrospectively becomes a partner in the initial creation of the world that the Talmud considered to have occurred some 6,000 years ago (Babylonian Talmud Tractate Shabbat, 1988, p.119b). This partnership was not left as an intellectual concept but was strongly reinforced behaviourally with the weekly observance of the Shabbat as a day on which most use of technology ceased for 24 hours, and when it became patently obvious that life could proceed without the technological advances of the past several thousand years. Current observers of the Shabbat, along with their ancestors of old, inhabit the same, coterminous technological domain – at least for one day a week.

The retrospective correction of past misdemeanours, or of participation in past events was further emphasised through several all-night vigils placed through the year. On the first night of Shavuot, the festival celebrating the giving of the Law on Mt Sinai, tradition insisted that one remain awake throughout the night studying Torah, in order to both prepare oneself for the giving of the Law that would be spiritually enacted the next day, and also to rectify and

compensate for the failure of the Jewish people to have held such a vigil several thousand years ago when the original events of Shavuot are supposed to have occurred. There is something inherently bonding about spending an entire night awake with company, and each synagogue running a vigil program takes on the aspect of a time travel machine in which participants, sequestered from the sleeping world outside, together take their place in an alternative dimension and time.

Similarly on the seventh night of Passover a vigil is held to prepare oneself for the spiritual re-enactment of the splitting of the Red Sea during the Jewish Exodus narrative.

In all these cases, the word re-enactment is insufficient and potentially misleading. For the intent is that although the event occurred once before in 'written / linear time', nevertheless it is understood that the event will happen again, not only in the present but also in the past, and with a vitiating effect on the present. The present does not necessarily only draw on the past but sends power to the past and creates it. Again, Dreaming is a particularly accurate conceptual metaphor for these notions of time and practice in which dance, song and ceremony in the present exist as much in the present as in the past - they 'sing up country' and bring into being the physical manifestations of song-lines in the form of ancestral beings vitiating anew. This enlivenment takes place even though from a western perspective the landforms referred to as newly sung ancestral beings predated the ceremony and appear unchanged by it. Song and dance are intrinsic parts of Haredi, and particularly Hassidic worship and celebration too. Through song and dance, their motions and effects are no longer only in the past or present, but in an everywhen of spiralling chronology.

It must be pointed out that the Dreaming and these Jewish practices and theological approaches are distinct and quite separate entities that developed at very distinct places and times. It has not been my intent here to appropriate Dreaming cosmology and practice. Rather, to use Dreaming as an illustrative instance of non-linear time that may be more familiar to the reader of this thesis than is Haredi thought and practice.

All this conceptualisation of time as fluid and recursive, and the reflection of such fluidity in individual Jewish practice is of course all framed within the overall Jewish theology that God is not only above time, but that God creates all existence, including time, constantly, from anew, and that the dead will one day arise in the messianic era (Maimonides 1168, Ch. 10). While I may

no longer subscribe to notions of an interventionist deity, the echoes of my lengthy cultural programming remain.

1.4 WHEN THE DEAD ARE NOT PARTICULARLY DEAD

In like manner again to the notion of ancestral spirits in many Indigenous theologies, the dead in Jewish orthodoxy are nowhere near as deceased as are the dead in western ideology. In Jewish law, the departed do not lose their rights as individuals. On the contrary, given that they now are not able to defend their rights, they along with the disabled, the impoverished, the aged and children, require community vigilance to ensure that their rights are not infringed. The highest office to which a lay member of a Jewish community can be elected, is as a member of the Chevra Kadisha (Holy Society), which functions as a voluntary burial organisation in which members rotate duties including washing the deceased and facilitating burial.

Once buried, the deceased still have a social role. They are visited on anniversaries of their death, before major holy days and to inform them of recent or upcoming family events. They are frequently named after, with the result that families often have characteristic sets of names or chains of names that span multiple generations. I myself have a nephew Ezekiel, son of Nachman, son of Joseph, son of Ezekiel, son of Nachman, son of Joseph – and so backwards in time. The dead are not gone, but are all around one, brought to memory repeatedly in often large families with several individuals named after the same ancestor. Their names are brought up not only in reference to their namesakes, but in a sense of veneration – in the hope that by mentioning their names in positive context, their heavenly life may improve.

If the deceased is a person of particularly holy status, they (their graves) are frequently visited by individuals wishing to honour them, to create a spiritual attachment, or by those in need who ask that the deceased should intervene and seek divine mercy on their behalf. A note may be written and read to the deceased and then placed on their grave along with the ubiquitous stone placed after any visit to a Jewish gravesite (Figure 1).

Figure 1: Hassidic women praying at the graves of the late Lubavitcher Rebbe (Hassidic leader) and his predecessor, in New York. Both graves are covered in a thick layer of kvitlech (notes beseeching intercession). Visitor's stones are visible on top of each gravestone.⁴



The average deceased however (not a saintly leader) is considered in more need of intercession by the living than the other way around, and this intercession may take very earthy tones. Traditionally every year on the Yohrtzeit (anniversary of a parent's death), the bereaved child attends synagogue, in the case of an orthodox or Haredi congregation the bereaved son leads the morning prayers, and afterwards provides whisky and snacks for the congregants (with an emphasis on the whisky). The traditional toast is 'may the soul experience elevation!', the aspiration being that the combination of demonstrated filial duty and generosity to worshipers should intercede on behalf of the departed's soul. It makes no difference how old the bereaved is, or how long it has been since their parent died. The concept continues that there is a tangible interdependent connection between the departed, their descendants, and to an extent the community that their descendants are within.

⁴ Ohel Chabad-Lubavitch, 2022. At-the-Rebbes-Resting-Place. Available from: https://www.ohelchabad.org/templates/photogallery_cdo/aid/397901/jewish/At-the-Rebbes-Resting-Place.htm (Accessed 28 July 2022)

This carries through to study, in particular Talmudic study, where Mishnaic⁵ texts from the first centuries CE are studied along with their accompanying 2,000 years of commentary, with each commentator being identified biographically and referred to by personal name or title. The mode of study is Socratic or interrogatory and discursive. The primary traditional Jewish commentator on the bible is Rabbi Solomon Yitschaki or Rashi (1040-1105CE). To the seven year old Haredi child, the thousand year-old Rashi is as familiar a personage as a football player might be to a seven year old child in wider society. This familiarity and sense of immediacy with past masters only increases as the Haredi child grows and encounters a vast array of other commentators and thinkers.

In retrospect, it was a mind set produced by the factors above – of non linear time and the presence of the deceased in the current moment, that led me to my honours thesis *No Yizkor in the Ghetto of the Dead* (Lever 2006). This thesis examined anomalous burial practices in the Jewish section of the Melbourne General Cemetery. Specifically, I identified the existence of and examined the social, religious and historical causes, behind the pauper and unmarked burials of several hundred poor Jews in the 19th century. I investigated the manner in which this conduct differed markedly from traditional Jewish practices and also differed from various local Jewish communal histories. Had I not approached the topic with the traditional Jewish assumption that pauper burial, particularly in unmarked graves was counter to Jewish treatment of the dead, I would not have had my interest piqued in the history of the location. At the same time, had I not moved outside of the Jewish community sufficiently to question the established communal histories which denied that such burials had taken place, I would not have been positioned to interrogate the evidence in the manner that I did. My approach to time, the past and people of the past in this current doctoral thesis is the product of much the same sort of tension. Of a dialectic between, on the one hand, a privileging of the past and people in it with the ability to reach out beyond their linear chronological position, and, on the other hand, a western sensibility around the manner in which communities, particularly communities of scholars, are created and continue.

⁵ Mishnah: the first redaction of oral Jewish law into written form, codified between the years 200-300CE and followed by the Talmudic redaction of oral commentary on the Mishnah, codified around 500CE

1.5 ARCHAEOLOGISTS AS ARTEFACTS

And so, to return to the main themes of this thesis, I have found that for me, those who practiced archaeology in Australia in the past, walk only a shadow's breadth apart from those who practice it now. And that to engage in the community of Australian archaeology is inherently to join and participate in the processes that have shaped it and brought it to its current state. Whether those processes are ones of inclusion, or as often documented here, processes of exclusion, they are nevertheless processes rooted in personal choice and disposition, in agency and the lack of it - and these are therefore by definition processes best understood from an intimate and personal perspective. My mind does not easily distinguish between the influence on the discipline of those who are currently breathing, and those who have passed away, even many years ago in linear time. All are enmeshed in what is still a give and take of interpersonal relationships.

Archaeology is a discipline in which a minutia of artefactual evidence, a multitude of often tiny artefacts, can be drawn together through intense analysis, through inference and deduction to constitute a narrative or image far larger than the sum of their parts. So too I contend, archaeologists are the individual artefacts, the basic unit of data, from which any history of archaeology is best constructed - a history which is grounded in an intimate understanding of who each archaeologist is, and why they have experienced careers in the manner that they have unfolded. This is archaeological history in which a fascination with and delving into the individual detail results in a nuanced understanding of not only the past, but also of the present as a product of the past - and of the present in itself as a replicator of past processes. This is a similar approach, although in total inversion, to the *Annales* history of Braudel et al. (Braudel 1972, Le Roy Ladurie 1975). In the *Annales* histories, although the individual moment is catalogued and treated in an economic sense, nevertheless the individual and their detailed momentary experiences of life are regarded as the *evenement* – the trivial event. Braudel's metaphor for his history is that of the ocean, with massive long-term events represented by currents at the sea bed, over which medium duration events represented by moderate currents flow, and above all of which individual events occur, represented by the irrelevant sea spume and wave peaks which toss to and fro without ever changing the course of the sea. Seeing archaeologists as artefacts on the other hand speaks of constructing a history in precisely the opposite manner - in which the underlying major currents of the discipline of archaeology are made up of the multitudinous events and personal details that comprise biographical life, and other apparently grander themes are inherently constituted from these details.

The methods by which I weave these multiple details into a coherent narrative are as eclectic as my education has been, and probably categorise my thinking as ‘fox-like’ under Berlin’s scheme: The British philosopher Isaiah Berlin authored an essay entitled *The Hedgehog and the Fox*, in which he attempted a retrospective classification of all major (western) intellectual figures into one of two types: The hedgehog characterises the scholar who knows a lot about a very narrow field - a lot about little, as reflected in the burrowing delving character of the hedgehog. The fox represents a thinker, who dashing from topic to topic, knows a little about a lot (Berlin 1953). There is no inherent advantage in either the hedgehog or fox form of knowing, yet I acknowledge that my approach may not sit well with intellectuals of the hedgehog camp.

Source criticism or philology and documentary analysis are two methods frequently applied in studies of ancient history, including biblical studies, but which one rarely sees applied to modern or current historical texts. I see no reason in theory or practice why these techniques cannot be applied to modern documents, with the same anticipated results of analytic insight to the intent or biases of an author, whether they are explicit or not. In weaving together the results of such analysis and research to produce a biographical figure and image of a community of scholars there is the need to apply a perspective that allows one to see what the biographer Leon Edel (1907-1997) metaphorically referred to as ‘the figure under the carpet’ (Edel 1986). The figure under the carpet is the image, invisible from the carpet’s face, of the knots and weave that makes up the structural fabric of the carpet. This viewpoint allows for a functional understanding of the dynamics of power that underly communal and interpersonal relationships. It is difficult to overstate the influence of Edel on the field of biography. Described by Barbara Caine (2010) as “the doyen of biography studies in the 1960’s and 1970’s” Edel’s practice lasted over 50 years. His lifework was summed in his five-volume biography of Henry James which was awarded the 1963 Pulitzer Prize. Edel insisted, in like manner to his view on perceiving that beneath the carpet, on a psychoanalytic approach towards his subjects, or what he termed “applied psychoanalysis”. That is, looking at some of the techniques that clinicians would such as examining the biographical subject’s personal relations, family relations, and method of approaching career. (Powers 1997). Although such methods may not find favour with literalist historians, I have to some extent also asked ‘what if’ questions regarding the lives of my case studies.

Having used Edel’s metaphor of the figure under the carpet I am afforded a segue to what I hope is a primary feature of this thesis - the use of metaphor and analogy to define and explain in relatively few words, concepts that would otherwise require significant elaboration. This too I

must admit is a cultural inheritance. When I first left the Hassidic community sufficiently that I felt I could read the New Testament without fear of divine retribution, I did so and was astonished at how culturally Jewish a book it was. Jesus after all constantly speaks in metaphor and analogy - and this should hardly have been a surprise to me given that both he and I had grown up in a Pharisaic world. A new approach - if not a testament, is what I hope this thesis will provide, in adopting an unprecedented combination of methods including metaphor, to the study of the history of archaeology.

1.6 TYPOLOGIES OF ARCHAEOLOGICAL HISTORY

Histories of archaeology to date can be brutally and probably over simplistically divided into three categories. Nevertheless, for the purpose of an introduction this division is not inappropriate. There are histories of Great Archaeologists, histories of Great Finds, and histories of the Linear Progress of Archaeology. The first two sorts of histories tend towards hagiographic accounts that gloss over what are often the considerable human and archaeological failings and personal perspectives of their subjects and tend not to probe too deeply into the interpersonal dynamics leading to their success. Wengrow and Graeber (2022) have recently elaborated at great length fundamental divisions in the conceptualisation of human history as reflected in the inherent contradictions between the philosophies of Rousseau and Hobbes. Their detailed discussion however, is beyond the scope of this summary of archaeological histories.

The Linear Progress account of archaeology tends to adopt one of two historical models of progress. Either the liberal view of human progress as being an inevitable pathway through enlightenment from 'savagery' to civilisation, or a socialist perspective which sees much the same overall trajectory, although achieved through dialectical struggle and revolution (Walter 1969, p.30). In the case of histories of archaeology, the liberal view sees archaeology progress from less to more developed models of archaeological methods and theory, while the socialist perspective sees archaeology progress through continual change and revolution between schools, people and ideas.

In contrast, I approach this thesis from a personal and political standpoint of anarchism. Rather than elaborate at length what anarchism is or is not, it is enough for current purposes to state that anarchism is not the desire for chaos, but 'anarchism is the political elaboration of the psychological reaction against authority which appears in all groups' (Walter 1969, p.27). With application to history and progress, anarchists often do not see progress at all, whether in a

linear or dialectical manner. On the contrary, anarchism often sees human history as a continually repeating struggle between the empowered and the disempowered, between the haves and have-nots, between those who want to govern and those who do not want to be governed. This tension is never resolved, and human history continues to lurch from standpoint to standpoint. This coheres perfectly with my conception of the formation of the community of Australian archaeology as inherently unpredictable and largely influenced by the personalities of archaeologists not only at any one time, but through time, as the fickle inheritances of one generation's actions continue to exert force beyond the grave.

1.7 CASE STUDIES

In this thesis I examine at depth the careers of three archaeologists. They provide case studies into how a nuanced understanding of biographical detail can shed light onto interpersonal relationships and eventually the development of the Australian community of archaeology.

1.7.1 Dermot Casey

An individual whose contribution to Australian archaeology has been both of long-lasting repercussion and also under-recognised is Dermot Casey, who constitutes the first case study in this thesis. Without going into detail as to the manner of his contribution and lack of recognition, it is enough to say here that through developing a detailed and nuanced understanding of Casey's social standing, upbringing, personality and position relative to the academic archaeological world of his day, it has been possible to arrive at a biographically-informed comprehension of not only why Casey would be satisfied to play a background role in the emergent field of Australian archaeology, but why too his input as an unqualified honorary ethnologist would be permitted to carry the unrecognised weight that it did amongst leading archaeologists of the time.

In many ways Casey represented a fortunate resource for the nascent discipline of Australian archaeology. His practical field abilities, possession of field equipment, and capabilities in running excavations to international standards – all complemented, augmented or, at times, simply compensated for deficiencies in these fields, certainly within the assets and skillset of John Mulvaney. I will argue that the success of partnership between Casey and Mulvaney rested precisely upon the presence of a significant social status gap between the two, due to which it was appropriate for Casey to offer expertise and services gratis, and also due to which Mulvaney perceived Casey as a helpful outsider, rather than a potential rival from within the circles of

academe. Recognition in itself is almost certainly something that Casey avoided - and his efforts to evade recognition have largely been successful.

Researching and writing on Casey and Mulvaney brought me into a world that I have had very little personal exposure to - that of middle and upper class Anglo-Australian males in the early to mid twentieth century, or at any time whatsoever. Investigating the next case study - that of Alexander Gallus, felt like immersion into almost an over-familiar world.

1.7.2 Alexander Gallus

It is the late 1970's and I look around me in my classroom of the Yeshivah College Boys High School in St Kilda, Melbourne. The unheated room is painted grey - as grey as our uniforms. The only touch of individualism in our dress is our choice of colour in our yarmulkes⁶ – which at any rate are largely constrained to dark blue. Of my 30 or so schoolmates, only one I believe has parents born in Australia. Everyone else is a child of migrants, a child with often distinctive surnames, and a child of parents who even to our ears had odd accents. We know our parents' stories even though we have never heard the largest part of them. They had lives in Europe described fleetingly, and then came a silence in the narrative, a silence that was never broken. Afterwards, the story picked up with odds and ends of Displaced Persons camps in Europe, applications for asylum or refugee status, interviews, trying to follow family around the world, and often the random fortune that brought them to Australia rather than anywhere else on the globe. Australia - where a reinvention began that might have drawn on prior knowledge, but which often entailed simply accepting that what was gone was gone, along with all the accumulated expertise that previous life may have entailed.

Gallus, although not Jewish, fits almost seamlessly into this narrative. He was highly qualified holding two doctorates, and at a young age had risen to head the prehistory department of the Hungarian National Museum. The Soviet occupation of Hungary carried one message for previous holders of Hungarian public office – to flee. And so, Gallus with his young family joined the world familiar to me from so many tales - eventually arriving in Australia. Gallus' level of education, breadth of reading and width of thinking was far beyond that of the emergent Australian archaeological world that he encountered. His mode of thought often proved

⁶ Traditional Jewish brimless headwear – sometimes referred to as a skullcap

incommensurable with the empirical approach predominant in Australia, defined by them as the 'Cambridge school'. It is a sad contrast to the case of Gallus that my schoolmates' parents - most of whom spoke at least four or five languages, were often able to use their languages to great advantage in at least two ways. Firstly, in forging business relationships with other émigré communities, but probably more importantly in the flexibility in thinking and approach to problematic situations that their multilingual capacity gave them. The more broadly, and flexibly Gallus thought however, and the more widely he cited international precedent and literature, the greater was the resistance to his foreign ideas by Australian archaeologists. Gallus eventually found himself effectively stymied by the Australian archaeological community due to his intellectual approach being thoroughly at odds with theirs. In contrast, migrants bringing their divergent views and multilingual talents to bear on business situations in Australia often enjoyed great success in a world where credibility was predicated on accumulation of money in new ways, rather than thinking and publication in conformity to the norms of the 'Cambridge school'.

This depiction and my previous metaphor of the hedgehog and fox largely relate to the manner in which one acquires and validates data. I would like to offer here a separate metaphor that seems apt for the different manners in which Gallus and his Australian peers processed and used archaeological data. That is, that many thinkers can be methodologically separated into two categories of peggers or painters. If one recalls the small wooden boards with cut-out shapes, through which children drive small wooden shapes using little mallets - then one has an understanding of the pegging mindset. To peggers, ideas are primarily categories that should be sorted as efficiently as possible into their correct corresponding locations (peg holes). This is not to belittle pegging- multivariate and cladistic statistical analysis are pegging tasks. For peggers, the purpose of research and study is largely then the categorisation of data and the creation of typologies for it according to rules and regulations of sorting. To painters however, the notions of rules and regulations are quite extraneous. Concepts are not seen as shaped pegs or discrete categories but as colours which can and should be combined, smeared, splattered and spread to produce an outcome that evokes something quite beyond the sum total of the material utilised. Neither approach is right or wrong, each has its place. I'd rather drive over a bridge built by peggers and buy art made by painters than the other way around. Yet in the case of Gallus and Australian archaeology it does not seem there was sufficient breadth of education and cultural understanding for most Australian archaeologists to comprehend the greater picture that Gallus sought to paint.

1.7.3 Elsie Bramell

Elsie Bramell is in many ways the most difficult of these three case studies to simply capture. Casey and Gallus can both be envisaged as embodiments of relatively binary oppositions - or combinations of binary oppositions such as upper class vs middle class, not formally qualified vs formally qualified, foreign vs Anglo Australian, idealist vs empirical. In each of these cases it is understood that Casey or Gallus really had no desire themselves to become their opposite - or they actively exercised agency not to become their binary opposite. Yet, in Elsie Bramell's case, the unresolved question still is, to what extent did she want and choose to be what she became, and to what extent was her later life too, a binary opposite of all she may once have dreamt for? Certainly, a rapid overview of Elsie's life indicates that from an early age she was independent, talented and motivated. An accomplished student and high school captain she went on to become one of the first anthropology graduates from the University of Sydney. This was at a time when university attendance, let alone in a subject such as anthropology, was still a rarity among Australian women. Specialised degrees such as nursing and education were far more normative choices for young women of the 1920's. Yet, as Elsie's career went on to demonstrate, she was far from normative, completing her masters in Anthropology on the topic of law in New Guinean society, and addressing scientific conferences on her thesis topic.

From the time she was appointed to the Australian Museum, it became clearly evident that Elsie Bramell was more dynamic an individual, certainly in public engagement, and more highly qualified than her assistant Fred McCarthy. Yet, after a romance bloomed between Elsie Bramell and Fred McCarthy an inevitable choice had to be made. Under the Public Service code of the time, spouses could not work together. In an era when male salaries and prospects for advancement almost certainly always outweighed female opportunities, even where such a disbalance of talent was present in favour of the woman, it simply would have made no financial sense for Fred McCarthy to have stepped down in favour of Elsie Bramell. That is, if his and wider patriarchal social attitudes would not have found the notion altogether ridiculous. It is hard to see what other choices may have existed at the time - certainly not for a couple who likely intended to start a family. Maternity leave did not exist, the prospect of pregnancy out of wedlock would likely have resulted in social and family shunning - and probably in a loss of career anyway.

It is easy today, to look back and retrospectively lament Elsie's loss of a brilliant career. My question here is, even without giving up on the perspective that sexist bias and patriarchal

oppression are inherently evil facets of human behaviour, can we understand that in a world where it was not perceived that any alternatives existed, that it may be understandable for people to have taken the path most trodden and to conform with patriarchal expectations? Or, do we hold Elsie Bramell as an example of brilliance who was forced from her career, and thus raise her to prominence as another victim of sexism in all its apparently timeless pervasiveness? I wonder too, in keeping with themes explored in this thesis of the enmeshed nature of influence - what the ongoing influence of Elsie might have been directly on her husband Fred McCarthy, and on the discipline of archaeology generally, through her influence on her husband after her official retirement? This seems currently undeterminable not only formally in terms of the conceptual influence that Elsie may have had, but more broadly undeterminable in terms of the support that Elsie likely provided Fred McCarthy's archaeological career from a domestic perspective. It could not have hurt surely to have lived with someone as experienced and more qualified than oneself in archaeology and anthropology, when it came to even tasks as prosaic as preparing for fieldwork or debriefing oneself from it? In many ways then it is disappointing that so little evidence is available on Elsie Bramell. For she would otherwise certainly have constituted a most valuable insight to the way that social relationships and structures shaped Australian archaeology.

1.7.4 A word on style

'Rabbi Gamaliel the son of Judah Hekhasid used to say...nor can a timid person learn...' (Mishnah Avot 1984, item 2.5).

The western world, particularly the Anglo-western world, permits, encourages and at times adulates all sorts of ritualised violence in the name of sport. It is quite permissible and indeed admirable for an individual in Anglo-western society to attempt violently to impact other individuals regardless of potential injury to them, so long as codes of fair play and arbitrary rules are abided by. This is not constrained to the less educated classes. Highly educated and qualified individuals engage in sports including Rugby Union and boxing at university and professional levels, incurring and inflicting injuries on others that would result in criminal charges off the field. The playing field is often held as a suitable (once termed manly) venue for the venting of aggression and for the display of total commitment to enmity and violent action embedded in deeply acculturated mores such as team endeavour or team leadership. "Go hard or go home" is a theme often heard or seen enacted and embedded in the Anglo-western sporting globe.

In marked contrast, Anglo-western academe is generally conscientiously polite with norms of constrained and impersonal verbal and written expression providing parameters for even those relatively rambunctious outbursts of disagreement at conferences where the generally rare face to face encounters between opponents are likely to occur. The notions of appropriately culturally curated 'scholarly' text and expression are upheld as necessary to the process of bringing thought to print. This modelling serves to stylistically subdue the expression of opinion and information that might arise through other modes of anecdotal and emotive expression – a subjugation that is underpinned by the inherently Anglo-western sentiment that the expression of emotion in academe is in itself an undesirable trait that runs counter to logical thinking.

Violence on the sports field, and politeness, emotional restraint, and hushed tones in the library. The world of Haredi Jewish scholarship is a mirror image to Anglo-western academe. Sport does not exist. It may be tolerated as an outlet for physical energy for young children, but by the time a child reaches their early teens engagement in sport is frowned upon as a frivolous distraction from study, and violent interpersonal contact is abhorred. This doesn't mean that aggressive endeavour is not rewarded or aspired to - it certainly is, but solely in the intellectual world, where the gloves are off and the art of aggressive argument is honed from an early age. The style and content of vigorous debate is observed from an early age – through watching parents and elders in conversation. Once the young (male) student commences the study of Talmud in Yeshivah from the age of 12 or so onwards, the behavioural modes of rapid, cutting verbal and written argumentative study are formally introduced.

The classical method for this is through study in the chavrusa⁷ system. In the chavrusa system students are divided into pairs and they turn their attention to texts being studied by the class as a whole. The expectation is that through a process of loud and animated argument with each other they will come to the nub of the text at hand. The more vociferous, vicious and cutting the debate between them, the more highly it is rated. The louder the Yeshivah study hall as a result of often hundreds of student pairs vocally arguing, the more highly the study hall is rated for the diligence of its study ethic. One does not complain that this noise is a distraction, for merely being aware of the noise is in itself seen as an inability to concentrate adequately. Just as on the Anglo-western sporting field where applause is given for the utter demolition of the opponent,

⁷ Chavrusa from the singular chaver or partner.

so too in the Yeshiva, a total tiuvtah (Aramaic) or opschlog (Yiddish), representing a complete and utter dismantlement of an opposing intellectual position, is considered an elite achievement. And in the Yeshivah as in the sporting field it is not considered appropriate to bear a grudge over a fair defeat, after all in the Yeshivah too it is very much an expected matter that one will “go hard or go home”. The implications of this are fairly obvious. I still play no sport other than aggressive academic endeavours and despite the best efforts of my supervisors it is possible that my manner may cause offence to some.

In summary, it is deeply ingrained in my thinking that historical time flows in an unsteady manner - events and people in the past may and do have effects on the present out of all scale to their seeming importance at the time, and we can connect with those who are no longer living through engagement with their works and attitudes. This is of course can be read as antithetical to scientific archaeology, but such is not necessarily the case. I do not at all suggest that, for example, Carbon 14 dates read as anything other than what they are. However, in terms of understanding the human past of archaeology itself, I believe that the discipline’s formation and current state is the result of far messier, enmeshed and biographical factors than has previously been considered.

In the next chapter I will provide a thematic analysis of the manner in which histories of archaeology have been carried out to date, and I will provide a brief methodological explanation of the means by which my analysis of the archaeological past differs from such predecessors in treating archaeologists themselves as metaphorical artefacts. I will then explore at length the biographical detail I have retrieved and analysed relating to each of my case studies. Dermot Casey is treated at greatest length in two chapters (Chapter 3: and Chapter 4:) Alexander Gallus in Chapter 5:, and Elsie Bramell in Chapter 6:

Chapter 2: Histories of archaeology

It is at this point that I would *like* to turn to an overview of archaeological histories, scanning through the literature of the past century, and presenting the reader with the novelty of my proposed approach as compared to all that has been written before. One existing work prevents me from honestly claiming such novelty. This work is the biography of the archaeologist Tessa Verney Wheeler written by Lydia Carr (Carr 2012). Carr's work is brilliantly insightful, deeply researched, wide ranging in scope and profoundly perceptive. Just as I undertake a biographical examination of several Australian archaeologists in the hope of gaining incremental insight to formative processes within the discipline in Australia, so too Lydia Carr has examined the place and role of women in British archaeology before World War Two, through the case model of Tessa Verney Wheeler. Lydia Carr introduces the term 'Archaeobiography' (Carr 2012, p.18) to describe what was then a new sub-discipline combining the three fields of archaeology, biography and history. This is a term which I feel deftly captures the essence of the methodology I have elaborated.

Despite this thesis bearing some similarities to Carr's work, I propose that my approach is still thematically distinct from those of the archaeological histories that I summarise below, including that of Lydia Carr (2012). My opening stance is theoretical - that the discipline of archaeology is a community formed through the explicit and often implicit inclusion of some and the exclusion of others. Further, that through examining the factors surrounding exclusion or inclusion of liminal contributors to Australian archaeology, we can gain a better understanding of the operation and manner of inclusion and exclusion of archaeologists and the formation of the discipline as a whole. The notion of 'archaeologists as artefacts', as parts of an assemblage that can be analysed at an individual or collective level, and at both levels at once, allows for the exploration of biographical themes as they relate to the individual archaeologist and also their relationship with the wider discipline-as-community. Rather than examining one major case study at length, as has Lydia Carr, I will examine three case studies with the aim of identifying possible common themes of inclusion or exclusion of these individuals and the manner in which the archaeological community has grown around them. Before embarking on my three case

studies, it is important here to set out the intellectual framework in which the history of archaeology generally and my perspectives in particular have developed.

2.1 MASTERS OF THE PAST

The most authoritative author on the history of archaeology is widely recognised to have been Bruce G. Trigger (1937-2006). His two editions of *A History of Archaeological Thought* (Trigger 1989, 2006) have served as standard textbook and reference works for the discipline for over 30 years. Perhaps as a function of their wide ranging and encyclopaedic nature they include very little in-depth biographical information on any of the many archaeologists whose works they depict. The same encyclopaedic nature of these works makes them difficult to effectively thematically summarise. In contrast, in a relatively brief item from 2001 that was republished in an edited work released after his death (Murray & Evans 2008) Trigger's chapter on Historiography in archaeology (Trigger 2008) looks back in far more succinct summary at the unfolding course of the history of archaeology than is provided in his two versions of *A History of Archaeological Thought*.

Trigger (2008, p.361) makes a telling opening salvo in this chapter on historiography by observing that 'the history of archaeology has been written mainly by professional archaeologists who have no training in history' with resultant issues in the quality and construction of archaeological histories to date (I hope that as I am a professional historian, I evade Trigger's critique). In this chapter too Trigger also provides us with his broad outline of a classificatory system for histories of archaeology which he divides into three, namely, Popular Histories, Intellectual Histories and Social Histories. These are curious divisions. Despite my great respect for Bruce Trigger and his work, I consider that these three categories of Popular Histories, Intellectual Histories and Social Histories are so broad and frequently enmeshed, that histories of archaeology (and particularly those of earlier times) consist of inextricably intertwined mixtures of all three themes.

Such works touch on great excavators and their great finds as popular history, aspects of the technical and intellectual advances of these excavators as intellectual history, and the historical and social setting to these excavators and technical and their intellectual advances as social history. In the section below, I remark on this complex and enmeshed characteristic of histories of archaeology from the early twentieth century onwards. It must be observed that such mixed and melded histories are still currently popular, with particularly prolific output by Brian Fagan

(Fagan 1978, 1985, 1994, 1995 & 2004). Rather than undertake a thematically separated analysis of archaeological histories along the themes suggested by Trigger, I will follow a chiefly chronological course.

2.2 TRACING THE HISTORY OF THE PAST

The first English-language history of archaeology that I have found is that by Stanley Casson (1889-1944). Casson was a scholar of classics at Oxford University who travelled widely on archaeological fieldwork and also through his military postings. His 1939 work (Casson 1939) *The Discovery of Man. The Story of the Inquiry into Human Origins* does not, as the title might suggest, largely investigate evolutionary human origins. It does note Darwin's publication of *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life* (Darwin 1859) as 'the greatest event' in the history of inquiry into human origins, and Casson does spend some time on prehistoric Scandinavian and European development. Nevertheless, by far the greatest proportion of Casson's attention is turned to the origins of 'culture' as deemed significant to him, namely Greek culture with Egypt as a possible precursor. Casson seems caught between his elitist lauding of the ancient Greek world on the one hand; 'in the long run it is always the Greek way of thought that emerges' (Casson 1939, p.91), and his humanistic insistence on the other hand that accepting common human origins means that an equality of humanity exists.

Casson situated this understanding of common human origins as opposed to the situation where the past is ignored and individual cultures invent for themselves distinctive and exclusive identities; 'jettison the past and you can claim to be unique....a mental and intellectual debauchery' (Casson 1939, p.19). Dismissing both the extremes of diffusionism and independent invention, Casson steered clear of comparatively ranking 'Great Finds' archaeology, by equally celebrating the methodologies and talents, rather than only finds, of those archaeologists active in his lifetime – Schliemann at Troy, Evans in Crete and Flinders-Petrie in Egypt: 'when the methodology of archaeological practice comes to be written, the name of Petrie will stand with that of Schliemann at the head of the list' (Casson 1939, p.277). Yet, Schliemann, Evans and Flinders-Petrie are only names in Casson's work, with no biographical detail to lend them context. The broadest sweep of context provided by Casson overall is his claim that the study of the past is unnatural to the human mind, and is a pursuit only taken up by those civilizations who have managed to rise above the struggle for existence.

Casson further proposed that the widespread disinterest in the past, which he observed in the 'current dangerous climate', served the purpose of muddying the single origins of the human race and therefore allowed parochial causes to come to the fore (Casson 1939, pp.13-19). This latter statement was timely, particularly given the large emphasis being placed at the time (1939) on construction of an Aryan identity in Nazi Germany through cooption of previous archaeological work by scholars such as Gustav Kossina, and in particular through the activities of the Ahnenerbe – Hitler's SS ancestral heritage research unit (Arnold 1990, Hale 2003). Despite his disputable claims on behalf of Greek supremacy, and his puzzling speculation regarding the motivation for attention to the past or ignorance of it, Casson nevertheless produced a work that was in content, scope and balance probably superior to any history of archaeology authored over the subsequent 50 years, up to the release of Bruce Trigger's *A History of Archaeological Thought* in 1989.

The most prolific author in the history of archaeology during the mid twentieth century commenced publication after Casson. Glyn Daniel (1914-1986) taught at Cambridge University and was a long-standing editor of the journal *Antiquity*. In his *A Hundred Years of Archaeology* (Daniel 1950), Daniel's writing tone is authoritarian and broad sweeping. One presumes the reader is led to accept at face value Daniel's insistence that the desire to study the past is one that is naturally embedded in the human mind (Daniel 1950, pp.14, 29). Daniel dismisses social or political, and in particular Marxist, explanations for the increasing interest in archaeology or its internal growth and insists that archaeology had grown through the development of its internal explanatory mechanism of the three-age system, which in turn had led to internally generated disciplinary growth (Daniel 1950, pp.53-56). Daniel presents this sequence of developments as linear and near automatic. Daniel constrains his analysis almost wholly to European prehistory, although unavoidably, it seems, with one chapter noting the methodological advances derived from Near Eastern archaeology which had then informed European techniques (Daniel 1950, pp.190-227).

Daniel came from a science background having initially entered Cambridge to read geography, he had already studied geology at Cardiff University. Through his service with the RAF in the Second World War, Daniel had developed considerable expertise in analysis of archaeological sites from aerial photographs (Renfrew 2004), and this scientific background likely underpinned his ontological approach to the discipline. I propose that this is reflected in Daniel's strong positivist bent, declaiming that prehistorians are capable of determining 'the facts' of the past as

they are (Daniel 1950, p.323). This follows from his insistence that archaeology is a largely a scientific set of techniques which is a handmaiden to the humanity of history: 'Archaeology is a technique by which facts are obtained for the construction of history and prehistory; and prehistory like history, is humanity' (Daniel 1950, p.311).

Such positivism seems to have underpinned the several subsequent works issued by Daniel on the history of archaeology. Following his *A Hundred Years of Archaeology*, published in 1950, Daniel released his curiously titled *A Hundred and Fifty Years of Archaeology* only 25 years later (Daniel 1975). This is effectively the same work as Daniel (1950) largely only updated by changing references to recent developments, to read as those of the 'last ten to 30 years' and insertion of sections on the nascent New Archaeology (Daniel 1975, p.370). Daniel's notion that interest in the past is an inherent human quality is repeated (Daniel 1975, p.381), as is his insistence on archaeology as a methodological contributor to 'the art of historical interpretation and writing' (Daniel 1975, p.371). Daniel's positivist bent is continued with his insistence that the history of archaeology is of great importance as it permits the understanding of the development of the discipline through an 'accumulation of facts' (Daniel 1975, p.374). Daniel winds a selective path between identifying archaeology as a scientific discipline that had developed in a linear manner through accumulation of facts on the one hand yet insisting that it must be beholden to the purposes of humanities on the other. Such 'facts' in Daniel's mind outweighed speculative exercises including those that he termed the philosophical 'whimsey' of Gordon Childe's Marxism (Daniel 1975, p.375).

Puzzlingly, Daniel prefaces his bibliographic essay in this work (Daniel 1975) with the words 'there has to date not been a comprehensive history of archaeology'. Given that Daniel 1975 is effectively the same work as Daniel 1950 one would assume that Daniel did not consider his own earlier work to have been a comprehensive history of archaeology either. I believe this is the case given the frequency and regularity with which Daniel utilises similar disclaimers in other works. His *The Idea of Prehistory* (Daniel 1962) is a small pocket size volume for popular use, with the familiar line 'No authoritative and definitive history of archaeology and prehistory has yet been written' (Daniel 1962, p.168), and with an almost identical line appearing in his second edition of the work (Daniel & Renfrew 1988, p.205).

Perhaps the clearest summary of Daniel's understanding of the unfolding of archaeological history can be derived from shorter academic works such as his address at the Peabody Museum

in 1966 (Daniel 1968), or his richly illustrated work for the popular market: *A Short History of Archaeology* (Daniel 1981). Both of these items rely heavily on the ages and stages explanatory scheme of the development of archaeology, in particular as had been propounded by Gordon Willey in his speech on the history of American archaeology at the same conference at the Peabody Museum that Daniel attended (Willey, 1968), and which seems to have formed the basis of Willey's influential co-authored work on the history of American Archaeology several years later in 1974 (Willey & Sabloff, 1974).

In his work *A Short History of Archaeology* Daniel (1981) concludes with a chapter on 'The Great Themes'. This lists 20 principal themes in the history of archaeology, a summary which reads as a chronologically progressive cumulative check list of technological and methodological developments in the development of the discipline. Despite the title of the chapter as devoted to 'Themes', the list is quite devoid of thematic interconnections and certainly of any philosophical or social threads and movements that may have vitiated or brought about the development of these changes. In summary, Glyn Daniel saw archaeology developing in linear manner with little influence by the personalities or philosophical positions of those who participated in it.

Slightly predating the works of Glyn Daniel are those of Kurt W. Marek (1915-1972), who wrote under the pseudonym C.W. Ceram. Marek chose to write under a pseudonym to avoid identification with his previous career as a propagandist for the Third Reich. Ceram's works were published in German and translated to English, and therefore do not strictly fall in the parameters of English-language histories of archaeology. Nevertheless, they were extremely successful in English and deserve some mention as early histories of archaeology. Ceram's first work *Götter, Gräber und Gelehrte* was published in 1949 and released in English in 1952 as *Gods, Graves and Scholars*. Both of Ceram's works (Ceram 1952 & 1958) are stridently anti-theoretical and Eurocentric, making no mention of theoretical advances by Thomsen, Worsae, Pitt-Rivers or Childe in his chronology, nor did Ceram mention any North American archaeologists. Both works are intended to be amenable to the average reader of the public (Ceram 1952, p.5). Ceram wavers between denigrating non-literate societies, and yet at the same time seeking in such societies the source for beginnings of the Eurocentric world from which he wrote and which he considered to be the seat of global enlightenment (Ceram 1958, p.302). By 1958, Ceram had read Daniel's work (Daniel 1950), which Ceram noted as the best overall survey of prehistoric archaeology available at the time (Ceram 1958, p.316).

At the same time that Daniel released his *Hundred Years of Archaeology* (1950), and in the period that Ceram's works were being translated to English, Stuart Piggott (1910-1996) released a biography of *William Stukeley. An Eighteenth-Century Antiquary* (Piggott, 1950). This was based on Piggott's B. Litt thesis at Oxford University and, in this work, Piggott explored many themes that in the future would become characteristic of wider histories of archaeology. Piggott weighed Stukeley's findings and understandings against criteria and thinking that were current in 1950 (Piggott 1950, pp.xi, 6, 70, 109, 175) and selected thematic items that Piggott perceived as practices still in use at the time that Piggott wrote (Piggott 1950, pp.58, 181). Piggott demanded that any such evaluation of methodologies across time needed to be considered in the context of the thinking of Stukeley's time (Piggott 1950, pp.xi, 71, 109). The value of Piggott's biography is diminished by his engagement in historical psychoanalysis in which he deems Stukeley's thinking as at times 'muddled' (Piggott 1950, p.140) or 'clear' (Piggott 1950, p.114). This is most notably evident in Piggott claiming that Stukeley had undergone a 'psychological disturbance' (Piggott 1950, p.130). Despite these weaknesses, the work is a notable, early and exceptional endeavour to understand the development of antiquarianism, a precursor to archaeology, through a detailed and intimate understanding of one of its chief and celebrated practitioners. Stukeley had spent much time studying Stonehenge and the Druids, and it was to this theme of *The Druids* that Piggott returned in a second work on the history of archaeology released in 1968 (Piggott 1968). Piggott's work on Stukeley (Piggott, 1950) approached the history of archaeology through a historically nuanced lens. *The Druids* (Piggott 1968) took this approach a step further, in studying the history of the study of the Druids as reflective of the intellectual environment and prevailing ideology of the time: 'Every age...has the Stonehenge it deserves' (Piggott 1968, p.192).

2.3 A QUALITATIVE TURN

In the same year that Piggott published on the Druids, Bruce Trigger released a short work that explicitly addressed the methods of prehistory, but which in effect also set the tableau for his subsequent prolific works on the history of archaeology. This work was *Beyond History: The Methods of Prehistory* (Trigger 1968). Although the work specifically focused on the methodologies of prehistory as practiced at that time, Trigger made brief mentions of the predominance of historical worldviews in shaping the nature of archaeological practice and thought (Trigger 1968, pp.2-3, 33, 51). Tellingly, Trigger's 'Further Reading' suggestions at the end of this work (Trigger 1968) contain only one recommendation for reading in the history of archaeology, namely Daniel (1962).

Perhaps spurred by this evident lack of academic histories of archaeology, ten years later, in 1978, Trigger released a seminal work on the topic, his *Time and Traditions. Essays in Archaeological Interpretation* (Trigger 1978). This is a compendium of 13 related chapters, only four of which had been previously unpublished, and of which two (chapters IV and V) were written specifically for the work. The remainder represented the increasing specialisation by Trigger in the history of archaeology over the past decade through his publications in journals, in contribution of chapters to edited volumes, and in delivery of papers at local and international anthropological and archaeological conferences and university seminars. One chapter that was written specifically for Trigger (1978) is Chapter IV 'Archaeology and the Idea of Progress' (Trigger 1978, pp.54-74). In this short, ten-page chapter it is possible to see the effective ideological and methodological outline of Trigger's subsequent and far more comprehensive histories of archaeology (Trigger, 1989, 2006). In the chapter, Trigger departs explicitly from Daniel's conceptualisation of investigation into the past as a natural human activity. Rather, Trigger places inquiry into the past as something culturally specific and further differs with the notion of simplistic linear histories as satisfactory explanatory frameworks. It is worth citing Trigger at some length here to gain a sense of his conviction around this:

"It is impossible to explain the development of archaeology as resulting merely from curiosity about the past, since such curiosity is itself a social product and in most instances is satisfied by speculation and mythology" (Trigger 1978, p.55).

Or, more explicitly with reference to the notion that archaeology may have developed in the sequential scientific manner depicted by Daniel, Trigger proposes that:

It is impossible to account for the development of archaeology in isolation from broader intellectual fashions, which in turn are, at least in part, complex reactions to changing social and economic conditions. These relationships are so complex that it may be impossible to isolate all of the factors that have shaped the development of archaeology or even to say which of them have been most influential (Trigger 1978, p.56).

The remainder of chapter IV in Trigger (1978) sketches out the chronological and thematic template to which both editions of *A History of Archaeological Thought* would eventually adhere (Trigger, 1989; 2006). There is little to no biographical detail in Trigger (1978); in fact, such would

perhaps seem counterproductive to his claim that a proof of the advance and validity of archaeological theory, was that it reached similar results independently by disparate scholars and in locations that were widely separated such as the United Kingdom and United States of America (Trigger 1978, p.95). In this model it is the uniform nature of findings rather than the disparate nature of contributors that is of interest. This might fit with Trigger's own retrospection on his career as a historian of archaeology in which he stated his goals had been 'to strengthen an epistemology grounded in moderate relativism, in opposition to ones based on either positivism or extreme relativism' (Trigger, 1998). It appears that Trigger viewed Daniel's model of archaeology as a science progressing through data acquisition as overly positivist, while he may have also viewed the attempt to inherently embed the development of the discipline in biographic factors as overly relativistic.

The positivism to which Trigger referred and reacted may also have been closer to Trigger's home than across the Atlantic where Daniel worked. With their *A History of American Archaeology* (Willey & Sabloff, 1974), Willey and Sabloff drew an image of American archaeology as having formed in discrete stages or periods, against a background of a 'Speculative Period (1492-1840) that contributed little if anything to the subsequent 'Classificatory-Historical' periods and the 'Modern Period'. These later periods were characterised by the progressive building of methodologies to be applied to objective data. It is noteworthy, that while Willey and Sabloff constrained their history to that of America, Trigger was already at this stage cross comparing and analysing histories of archaeology around the globe.

By this time too (the mid-1970s), an event had unfolded which would influence Bruce Trigger's approach to the history of archaeology to the end of his career, and which would profoundly influence my personal understandings of archaeology, its history and theory – including why I am currently writing this thesis. In 1975, Bruce Trigger commenced teaching an undergraduate course in the History of Archaeology at McGill University in Canada. From the time that Trigger commenced teaching that course it had been his intent to write the book which he felt naturally grew out of this teaching (Trigger 1989, p.xiii). His first attempts to do so resulted in the brief work mentioned above *Time and Traditions. Essays in Archaeological Interpretation* (Trigger, 1978) and, on something of a tangent, also an intellectual biography of Gordon Childe (Trigger 1980). His full publication of this course took shape as *A History of Archaeological Thought* (Trigger 1989). Bruce Trigger taught this same course on the History of Archaeological Thought, continually refining its content, on an annual basis every winter semester for 30 years. I was

extremely fortunate to obtain an exchange scholarship from the University of Sydney to study under Bruce Trigger and to take this course in 2005, in the last year it was delivered before his death in 2006.

I bring this up, not to promote my connection to this eminent scholar, but from quite different motivations altogether. I am well aware that the history of archaeology is not a topic that kindles great enthusiasm among many – at least anecdotally such was my experience in trying to explore the topic as an Australian undergraduate. And I am further aware that the history of archaeological theory has, until the last decade or so, lagged yet further behind general archaeological history on the scale of undergraduate enthusiasm in Australia. Bruce Trigger had kindly answered my second-year undergraduate email questions on the history of archaeological theory and I thought it logical to try and study under him. What took me by surprise was the enthusiasm for the topic that he generated among the second and third year undergraduates taking his course in Montreal, Canada. Bruce Trigger was anything but a ‘cool’ panderer to student sensibilities. An older gentleman invariably conservatively dressed in pressed trousers, shirt and tie, he stood at the front of a small lecture room and read from handwritten reams of foolscap paper for the solid duration of every lecture - no graphics, slides or overheads - as the course proceeded through *A History of Archaeological Thought* (Trigger 1989) from cover to cover. And yet simply through his excitement for the topic, he generated a level of investment in the subject that I’d never witnessed in Australia.

Undergraduate students would leave the small lecture room and immediately sit on the floor in the hall just outside the doorway to discuss the content of Professor Trigger’s lecture. And here is a neat turn: Both editions of *A History of Archaeological Thought* (Trigger 1989 & 2006) are such weighty slabs of dense and condensed text that they make heavy reading even for those inherently interested in their content. Without the human element to bring the text to life, I doubt I would have persevered through an undergraduate reading of the work. And ironically it seems possible that without the opportunity to teach and bring the content of these works to life to undergraduate students, Bruce Trigger may never have been brought to author these works either. The biographical and lived experience then is in this case clearly inextricable from the formation of the published work.

Both editions of *A History of Archaeological Thought* (Trigger 1989 & 2006) are too lengthy to allow for an in depth summary here. It is my (possibly biased) view, that with the first edition of

A History of Archaeological Thought there appeared in press for the first time a balanced and comprehensive history of the discipline, not constrained to the English speaking world, but in a worldwide setting. In forming a background to his work, Trigger opened by exploring the relevance of the history of archaeology to the discipline of archaeology, then tracing archaeology through Classics and antiquarianism to the adoption of scientific methods. At this point Trigger explored different modes of interpreting scientific data as affected by the socially, politically and philosophically diverse settings within which archaeology is practiced. Yet, throughout this, the individual lived experience, the biographic life, is little if at all referenced as a possible generator of change or motivator in the pursuit of an archaeological career, let alone as an agency that might be primarily responsible for the manner in which archaeology itself has developed. In contrast, first published in 1991 in French and only two years after Trigger (1989), the work *The Discovery of the Past* by Alain Schnapp, released in English in 1996 (Schnapp 1996), offers an insightful and comprehensive history of archaeology, distinguished from Trigger's approach perhaps by Schnapp's greater emphasis on the discontinuities and uneven processes by which archaeology had reached its current state at the time of his publication.

2.4 A GATHERING OF IMPETUS

By the time that the first edition of *A History of Archaeological Thought* came to press in 1989, the history and specifically the historiography of archaeology had already garnered sufficient attention that a specific conference on the topic was held in May 1987. Bearing the title 'Explaining Archaeology's Past: The Method and Theory of the History of Archaeology', this conference was hosted by the Center for Archaeological Investigations at South Illinois University, Carbondale. The proceedings of the conference were published as 17 short chapters in a work entitled *Tracing Archaeology's Past: The Historiography of Archaeology* (Christenson (ed) 1989). Although overtly organised into thematic sections, it is quite evident that many chapters fit across such categories. Chapters covered a wide array of topics and they include an Examination of the Role of Ideology And Institutions In Shaping Classical Archaeology In The Nineteenth And Twentieth Centuries (Dyson 1989), an examination of British and Allied institutional history (Chapman 1989), a History of archaeology and the Liberal Arts curriculum (Bender 1989) and an analysis of Recent Trends in American Archaeology in historical perspective (Sabloff 1989).

Most broadly, papers looked at what could be presentist or anachronistic tendencies in histories of archaeology, that attempted to retroject into the past modern archaeological methods and

approaches such as social archaeology (Chippindale 1989). Meltzer provided insight into the need for a history of archaeology precisely through a critique of Daniel's proposition of linear accumulation of knowledge. For, as Meltzer contends, it is not only that knowledge exists, but how it comes into existence and through whom it does so that it is likely to be accepted or rejected by subsequent scholars. Without a history to contextualise the uptake or rejection of knowledge, it would not be possible to gauge the grounds on which such knowledge was adopted or left behind (Meltzer 1989). In his own chapter, Christenson, the editor of the work, perceptively observed that at that time (1989) most archaeologists who had ever practiced archaeology were still alive, that the archaeological academic community was small and therefore older archaeologists were potentially able to exert negative influence on the careers of historians of archaeology who may have written unfavourably about them (Christenson 1989, p.164). The risk of offence weighs on historians of archaeology and risks the publication of hagiographic 'goody-goody' history (Christenson 1989, p.167). Perhaps of most relevance to this thesis is Christenson's observation that although archaeological concepts only appear as they are published, and it is with these that we engage, nevertheless the personal and public and academic lives of scholars are so inherently intertwined that one cannot understand one without the other (Christenson 1989, p.167).

In a category of her own in this edition is Alice Kehoe who piercingly critiqued what she depicted as 'the chronicles of consensus historians' with explicit reference to Glyn Daniel, Willey and Sabloff and their 'Whiggish' histories (Kehoe 1989, pp.102,105). Kehoe attacked those principles accepted as normative by Daniel, Willey and Sabloff as not in fact representing 'scientific' or neutral development of data, but rather reflecting 'a legitimization of bourgeois power' (Kehoe 1989, p.99) and that the characteristic explanatory schemes of such histories, in particular their reliance on stages (notably the three stage schema), was simply a reflection of western fascination with three-phased explanation, ranging from classic mythology to the 'Three Little Pigs' (Kehoe 1989, p.106). Kehoe's critique needs to be understood in context of her own experience as a woman who was then in the position of carrying out research from a non-prestigious position in a discipline dominated by middle class white men, as she described in a critical history of American archaeology authored by her some ten years later: 'And to you bastards who are too important to engage with someone lacking a prestigious position, I thank you, too for illuminating the social structure of American archaeology. Without you, I couldn't have written this book' (Kehoe 1998, p.ix).

To return to the 1980's, the proliferation of archaeological biographies had accelerated when, in the space of less than two years, three biographies appeared - all treating that most enigmatic of archaeologists, Vere Gordon Childe (1892-1957). As mentioned previously, Bruce Trigger had published an archaeological intellectual biography of Childe as almost a side-product of Trigger's growing interest in the history of archaeology (Trigger 1980), in the same year Barbara McNairn published an analysis of Childe's method and theory which, while mainly adopting a Marxist analytical perspective, also incorporated a biographical approach (McNairn 1980); and lastly, one year later, Sally Green released a biography of Childe with far more human content than is held in Trigger or McNairn's works (Green 1981). One year later again and Jacquetta Hawkes biography of Mortimer Wheeler by was published (Hawkes 1982). Biographies of Childe continue to appear, with an archaeological biography of him edited by David Harris in 1994 (Harris 1994), with chapters by eminent archaeologists including Bruce Trigger, Michael Rowlands, John Mulvaney, Leo Klejn, Kent Flannery and Colin Renfrew. And interest has not waned, with the recent release of the lengthiest and most specialised biography of Childe to date – *The Fatal Lure of Politics. The Life and Thought of Vere Gordon Childe* (Irving 2020). Irving's work, at 380 pages of dense political analysis, is likely outside the reading of many archaeologists interested in Childe. Nevertheless, for all the political complexity that Irving's work contains, his biography does rotate around a man chiefly known for archaeology.

2.5 ONGOING POPULIST BIOGRAPHIES

The publication of archaeological biographies continues, in part as full length books such as Douglas Given's biography of A.V. Kidder (Givens1992). Givens (1944-2004) wrote at some length about his experience and motivations in authoring his biography of Kidder (Givens 2008 (1992a)). He noted that at the time (1992) 'archaeologists have only begun to explore the issues of biography in charting the development and nature of their science' (Givens 2008 (1992a), p.180). Givens continued 'the history of any discipline, contrary to the thinking of Collingwood ⁸, is made up of participants who have emotions and life experiences' (Givens 2008 (1992a), p.181).

⁸ R. G. Collingwood (1889-1943) was an Oxford Don, archaeologist, historian and philosopher. As an idealist, Collingwood held that the past could only be retrieved through historical imagination and the reconstruction in the historian's mind of the thoughts of historical individuals. (Collingwood 1946)

I must wholeheartedly agree with Givens' statements on the topic, which in essence cohere with much of the motivation for this thesis. However, Givens' biography of Kidder, little lives up to these aspirational statements. His work commences with a fair degree (eight pages) of intimate childhood detail on Kidder - but these details seem unrelated to any later parts of the biography. From the time Kidder gained his PhD and commenced as an independent scholar, he seems – in Givens' work – to sail through life like a grand ship cruising through smooth waters, unperturbed, and similarly causing little if any biographical wake.

The degree to which Givens focusses on Kidder's archaeology, while apparently ignoring other events in Kidder's life, is well encapsulated in the events (or lack of reporting on them) surrounding Kidder's marriage. The entire episode of Kidder meeting, becoming engaged and married to his wife is presented in two brief sentences (Givens 1992, p.24). This brevity hardly seems to fit with Givens' above-cited statement requiring an insight to the emotions and life experiences of the historical subject. It is not that material was lacking to inform this biography on Kidder's romance and marriage— on the contrary, Givens' bibliography makes it evident that voluminous archives of Kidder's personal and official correspondence, diaries and manuscripts were available to him.

A far more empathetic picture of Kidder is gained through the several pages of photographs sequestered in the centre leaves of the book, and from very lengthy direct quotes from Kidder's diaries reproduced in appendices. Perhaps this lack of a human face for Kidder results from his lengthy career and the need to somehow condense a summary of it into a relatively brief book of less than 300 pages including appendices and notes. Yet, still, if the aim was to chart 'emotions and life experiences' (Givens 2008 (1992a), p.181), perhaps a more selective thematic approach might have served Givens better.

Other such full length biographies include Winstone's biography of Leonard Woolley (Winstone 1990), Drower's biography of Flinder's Petrie (Drower 1995), Fagan's biography of Graham Clarke (Fagan 2003), Inglis' biography of R. G. Collingwood (Inglis 2009), Hauser's skillfully woven biography of O.G.S Crawford and his impact on British Archaeology (Hauser 2008) or Sheppard's deeply insightful and theoretically enriched biography of Margaret Murray (Sheppard 2013). Yet, apart from these full length biographies, a distinct two-fold new trend is observable in the way that new biographies and histories of archaeology in general are being brought to press. The first of these developments is the omnibus or encyclopaedia approach to archaeological biographies

in which large numbers of past archaeologists are briefly treated by a wide range of often commissioned authors. Tim Murray's edited *Encyclopaedia of Archaeology. The Great Archaeologists* (Murray 1999) presents 50 commissioned biographies. A compendium on *Great Women in Archaeology* by Cohen and Joukowsky examines 10 case studies (Cohen & Joukowsky, 2004); while Brian Fagan's edited work *The Great Archaeologists* (Fagan 2014) covers 70 archaeologists treated by 40 authors.

The focus on great archaeologists in these works is fortuitous for this thesis because I seek to examine liminal individuals, those who may well have been 'great' but who have not been marked as such on the rolls of archaeological history. The existing fixation on 'great', on the biggest and the best, may well make for successful marketing of omnibus biographical archaeological collections that in themselves reinforce notions of 'greatness', but may equally not contribute to an in-depth understanding of how the discipline of archaeology has formed, in all its interactions and mechanisms - not only those interactions between its largest cogs.

I contend that a focus on the individual and detailed level is necessary, whether in depicting lives of archaeological 'greats' or others, and that this level of detail is unlikely to be available in the brief biographies such as those commissioned for Murray (1999) or Fagan (2014). The study undertaken for this thesis would corroborate the findings by Kelley and Klimko (Kelley & Klimko 1998) regarding a Canadian collection of papers *Bringing back the Past. Historical perspectives on Canadian Archaeology* (Smith & Mitchell 1998). It was the view of Kelley and Klimko that:

"...these papers show either implicitly or explicitly, that administration, research and political views and beliefs are more interrelated than archaeologists often perceive. An archaeologists work is framed by and constrained, by the social, economic, political and interpersonal relations in the workplace... Further, these non-scientific factors affect research in terms of the questions asked, the methodologies employed, and the inferences that result" (Kelley & Klimko 1998, p.10).

Kelley and Klimko noted that despite many reasons to have aligned with either processual or post-processual archaeology, Canadian archaeology predominantly adhered to locally prevailing culture-historical modes of archaeological explanation. Kelley and Klimko's observation regarding the fine detail of factors that might affect the research and career path of an

archaeologist is particularly relevant in the context of this thesis which attempts to examine just such evidence. So, is the observation by Park, that the primary influence on career and publication by Canadian archaeologists was not reading of published works, but their personal interactions with mentors and peers (Park 1998).

The second of the new developments in publishing biographies and history of archaeology arose through the establishment of the *Bulletin of the History of Archaeology* (BAH) in 1991. This has, I believe, also played an important role in changing the way that histories of archaeology are envisaged and brought to print. BAH is an online journal that publishes manuscripts as they become available. As a result of this, I believe there has been a centralisation of writing on the history of archaeology, and the article-length pieces in BAH seem to have largely supplanted the book-length omnibuses and biographies that once prevailed.

To a large extent this may be due, as noted by Murray and Evans (Murray & Evans 2008, pp.1-12), to the situation that the history of archaeology has so proliferated and developed in highly divergent ways, that it is near impossible to categorise or to include within a meaningful set of categories in one book or work, the full range of currently practiced approaches to the topic. The article-length format of predominant publication also seems to lend itself to greater attention to the social or 'everyday' aspects of this discipline, which Tim Murray had noted in a lengthy collection of his essays *Milestones in Archaeology. A Chronological Encyclopaedia* as having been little explored up to the early 21st century (Murray, 2007). In this work, Murray explores wide ranging themes in which he flirts with, but never fully engages with, notions that may undercut the very existence of such a thing as 'a discipline of archaeology'. Murray 'wonders whether the notion of there being a discipline of archaeology with overarching perspectives, approaches, methodologies and techniques was a viable one' (Murray 2007, p.xxii); and, more poignantly, that there existed 'an intensified sense of ontological and epistemological schizophrenia for archaeology. Practitioners live in two worlds, those of the natural and human sciences' (Murray, 2007, p.298). At strongest, Murray observes 'there is a darker side to variety when archaeology is dismembered by its constituent specialities' (Murray 2007, p.308). The propositions advanced by Murray, and the conclusions that I draw from them, would sit comfortably with the position proposed in this thesis - that archaeology is largely a social and communal construct in which membership is bestowed or withheld on a wide range of specific, often rapidly, changing values and personal perspectives and biases, many of which may have little to do with academic achievement.

Histories of archaeology continue, and not always by archaeologists with historical training. This can be problematic, to the opposite effect of that observed by Bruce Trigger in his noting that histories of archaeology had seldom been written by professional historians. For, the values and understandings that often go unstated between archaeologists may be so foreign to historians that they are not capable of perceiving where the past may contain grievous wrongs against the archaeological record. I contend that one such glaring case is evident in the work by the highly respected and prolific historian Tom Griffiths. In *The Art of Time Travel, Historians and Their Craft* (Griffiths 2016), the first chapter deals with Eleanor Dark (1901-1985). Dark was the author of evocative historical fiction. Of most relevance to Griffiths' chapter is her work *The Timeless Land* (Dark 1941) which was subject to multiple releases and translations. Griffiths promotes Dark as 'Australia's most influential historical writer of the twentieth century' (Griffiths 2016, p.17). Griffiths enthusiasm for Dark is barely constrained, he gushes in adulatory manner at all aspects, from her writing to homemaking, to her love for the bush and walking in it. And this tone of adulation continues through a sequence of depicted events that could do little but thoroughly infuriate any archaeologist reading Griffiths' narrative. The Darks, we are told, wanted a cave retreat in the Blue Mountains (in addition to their home). They found a cave which they kept secret and then 'They cleaned the floor of the cave with crowbar and mattock and by dynamiting the larger rocks' (Griffiths 2016, p.21). I do not believe it has ever been legal to vandalise parts of a national park to suit one's entitled whim for a writing retreat. Worse though is that, from an archaeological perspective, habitable caves, particularly in the area around Sydney are almost invariably locations of high archaeological potential. This is reflected in the heritage legislation contained within the NSW *National Parks and Wildlife Act 1974*, and one would think such archaeological values would surely have been at least known in principle to the Darks who chose to obliterate any such archaeological evidence with dynamite, crowbar and mattock. Even if by some stretch of the imagination this was not known to the Darks, any archaeologist writing about such activity would almost certainly have found themselves morally bound to expose and criticise such behaviour for what it was. Griffiths is not an archaeologist, and the chapter on Eleanor Dark continues in unmitigated effusive praise.

I return to the current tendency for publication of histories in journal articles. With the diffusion of approaches, and the apparent acceptance of difference that this diffusion entails, one formerly strident archaeological voice seems to fade to the background. That is, the use of archaeological history as polemic. Just as Orwell stated 'Who controls the past controls the future. Who controls the present controls the past' (Orwell 1949), so too those who own the

history of archaeology - those who can lay claim to being inheritors of the discipline's lineages of practice and thought - they can lay claim to the archaeological present and aim to shape the future of the discipline. Ironically, among those who have most stridently and at times effectively used this method include those with the most outspoken opposition to historical methodologies in archaeology. Lewis Binford's autobiographical pieces speak loudly on such themes. In these he portrayed himself as the oppressed and wronged victim of the erroneous and conservative establishment (Binford 1972, 1983). In opposition to the positivist archaeology of Binford, Ian Hodder composed an equally historically based offensive in his opening passages to *Symbolic and Structural Archaeology* (Hodder 1982). As noted above Alice Kehoe (Kehoe 1998), utilised the past willingness or reluctance of conservative scholars to participate in her research as a means to portray them as reactionary members of forces acting against the good of archaeology.

2.6 AUTOBIOGRAPHIES

Archaeological autobiographies are relatively few. The archaeological autobiography is the literary form perhaps most problematic for the historian of archaeology to evaluate. This is largely because the images drawn upon by the autobiographer in creating a narrative self depiction that winds between publicly available data, are formed from material frequently known only to the autobiographer, and which are often not independently verifiable. Processes of literary elaboration or obfuscation are particularly difficult to identify when they are written by academics who have made their life career out of carefully crafting text. Sometimes however, such bias is clearly evident. An example of such bias is *Conversations with Lew Binford: Drafting the New Archaeology* (Sabloff 1998), in which it is evident that the interviews which are almost verbatim reproduced in text, were largely an opportunity for self aggrandisement by Binford.

Perhaps the relative infrequency of archaeological autobiographies can be explained by a perception that there is something inherently and distastefully self-promoting about the notion of their publication and that their authorship sits uneasily with the currently bifurcated vision of academe in which open self-effacement and modesty sits side by side with a behind-the-scenes desperate struggle to publish faster than one's peers. Perhaps it was the fact of being elevated by great age that saw the distinguished British archaeologist Margaret Murray (1863-1963) publish her autobiography in her hundredth (and last) year, as *My first hundred years* (Murray 1963). Perhaps too, as an example of such proposed characteristic academic self effacement, Margaret Murray commenced her autobiography with the words: 'It is very disappointing to

have had no adventures; other people have them but not me. So here goes for the record of a life without a single adventure' (Murray 1963, p.5).

To advance more than 30 years forward and to the southern hemisphere, Carmel Schrire's autobiography (Schrire 1995) spends much time situating the author against a background of her colonialist inheritance and examines motivations and effects of her work among First Nations people in light of this background. It may be that this thematic narrative was perceived by Schrire as elevating her account above an entitled personal retrospective that was simply autobiographical.

In summary, the history of archaeology has evolved not only in response to changes in the practice of archaeology itself, but to social and institutional understandings as to what constitutes archaeology and a befitting history of it. The depiction not only of archaeology, but of public engagement with it too can widely vary. Casting back to Casson's early work (Casson 1939), I found a scholarly emphasis on pursuit of culture and its origins that, expressed puzzlement at the lack of popular interest in archaeology and the past. Yet, only 11 years later, Glyn Daniel wrote in broad terms of the great archaeologists and their discoveries, while explicitly taking for granted public interest in archaeology. Needless to say, Daniel's findings of public interest in archaeology went hand in hand with his efforts in which he himself popularised archaeology on radio and later television. C.W. Ceram too (Ceram 1952, 1958), successfully concealed his past as an instrument of the Third Reich and wrote in sweeping populist terms that elevated the literate society of his readership above the non-literate societies he often portrayed. Homing in far more closely on a single figure in history, Piggott's work on William Stukeley likely engaged too closely in detail to engage a wider audience (Piggott 1950). Piggott's succeeding work on *The Druids* (Piggott 1968) more likely attracted widespread and popular readership.

It was only from the year of Piggott's *The Druids* (1968) that serious literature came to publication that addressed archaeology not only as a progress of discoveries and great men, but as an intellectual movement too. Trigger (1968), Willey and Sabloff (1974) and later Trigger (1978) form the first bases of academic inquiry into the internal processes of the archaeological discipline, in a manner that does not seek to aggrandize or dramatically overstate the importance of spectacular archaeological finds. With Willey and Sabloff (1974) and later Trigger (1989), the omnibus, internalist approach to thematic history of archaeology had effectively

reached its peak. I am uncertain whether it is due to a general shift towards online publication, however in the years since the second edition of Trigger's *A History of Archaeological Thought* (Trigger 2006), the trend has been either towards publication of articles on the history of archaeology in journals or edited volumes, or on the other hand the release of 'coffee-table' style works – such as Fagan (2014).

Throughout this process of changing histories of archaeology however, much has remained the same in the way that the lives of archaeologists are portrayed in relation to their practice and discipline. They are either portrayed as Finds related Great archeologists - heroic figures who make great discoveries (Carter, Schliemann, Evans and Flinders Petrie are archetypal figures), or they are Theory related Great archaeologists – methodological and theoretical heroes who make great contributions to the development of theoretical aspects of archaeology (Montelius, Thompsen and Childe are early such contributors). In both cases the predominant approach has been to engage in relatively little in-depth analysis of the biographical background of the archaeologist being studied, and similarly little in-depth analysis of the interpersonal relations and the reality of the archaeological world as a community within which their careers developed and grew. This then is the point of departure for this thesis – to attempt a fine grained biographical analysis of several liminal individuals of Australian archaeology in order to obtain a more nuanced image of the manner in which the discipline has developed.

I would like to return here to Lydia Carr's biography of Tessa Wheeler (Carr 2012), and once more, in the light of the above, sketch out the richness and multidisciplinary colour of approach that Carr displays in this work. Carr understands well the realities of archaeological fieldwork - not only its technical aspects, but also the interpersonal relationships that are frequently characteristic of and established between people with particular roles on a 'dig'. In tandem with this archaeological understanding, Carr displays archival research at depth, and the ability to adroitly match this archival information to such archaeological understanding in the production of an 'archaeobiography' that is situated within an understanding of women's historical and current disadvantages in the archaeological world. I have not set out here to examine disadvantage, rather to observe whether mechanisms of disenfranchisement can be observed between individuals who remained liminal to the mainstream practice of Australian archaeology. Nevertheless, the key points of Carr's archaeobiography are essential to my thesis - the understanding of the nature of archaeological work, and archival research that sheds light on the

manner in which such archaeological work may have been undertaken, and its opportunities extended or withheld.

2.7 A WORD ON METHODOLOGY

In this thesis I examine the biographies of several archaeologists and I make analytic observations regarding their careers relative to the development of Australian archaeology. Yet this thesis is only a dent, a bare mark, only several knots in the rug of what I believe a richer biographical analysis of the development of Australian archaeology could be. But the broad biographical and historical picture that I propose would require many years labour by many hands and would be an ongoing elaboration to keep up to date. One could well ask then, what would be the point of such a demanding and detailed history in the first place, when prolific textbook histories of archaeology such as Trigger (1989; 2006) are already available? I feel that the answer largely lies in the democratisation of archaeology and in the realisation that for every 'Great Archaeologist' there are many more archaeologists who have made great contributions to the mainstream of the discipline yet who have never gained such a title or appropriate recognition. And too, there are those archaeologists who have operated on the fringes of the archaeological community and whose work has been little publicly recognised. In the sum of those who have not been deemed 'Great' and these 'fringe practitioners', I would contend that we are dealing with the vast majority, an unseen majority, of the archaeological world.

And yet, at the same time that I call for democratisation, I have to admit that the process of detecting potential candidates for study has been anything but a statistically neutral method of identification. For, at the same time that the candidates whom I have studied have been hallmarked by the relatively little attention they have received in Australian mainstream archaeological literature, they also needed to stand out in some manner sufficiently to come to my attention as individuals whose achievements were not reflected in disciplinary histories. The often chance manner in which these individuals came to my attention is outlined in the next paragraph.

Casual conversation in 2015 with Dr Christopher Davies, Director of the Australian Institute of Archaeology, led to my first hearing the name of Dermot Casey, and the inkling that perhaps a worthwhile history could be gained by investigating this intriguing individual. Dermot Casey is the first and lengthiest case study in this thesis. In subsequently browsing volumes of the Victorian journal *The Artefact* I could not help but note the frequency of contribution to it by an

Alexander Gallus- someone whose name had not appeared in my readings of Australian archaeology to date, and who proved to be a fascinating second case study for this thesis. While attending an archaeological conference in Australia, I filled time between sessions browsing archaeology books for sale at substantial discount. I was very pleasantly surprised to find that a series of feminist and gender anthologies of papers by Australian scholars had been published in the 1990's. Purchasing whatever copies were available, I found that one of these volumes *Women in Archaeology A Feminist Critique* (duCros & Smith 1993), was dedicated to Elsie Bramell. No further mention was made of Elsie Bramell in this work, and as fascinating a person of study as she has proven to be, relatively little more has come to light regarding her. As much as has come to light regarding Dermot Casey, Alexander Gallus and Elsie Bramell, information on them remains scant compared to their better-known peers. Much of the process of biographical writing for this thesis has proven similar to John Gaddis' description of it, as being like time-lapse photography, where the historian's task is to attempt to fill in the gaps between photographs, where in this case the photographs are snapshots of Casey's, Gallus' or Bramell's life as often viewed only through windows in the walls of their more established colleagues and the greater data available on these established scholars (Gaddis 2002, p.115).

Identifying such liminal individuals then relies on a combination of achievement and constraint - that they have somehow become sufficiently identified in literature or archives as persons of achievement, yet at the same time that their identification has not been to the extent that they are widely known. In many ways this resembles processes of discovery in both history and archaeology. Although archaeology may attempt to vest its processes of discovery in far more scientific terms, using mechanisms such as predictive modelling and landscape analysis, nevertheless much discovery in both archaeology and history, as in science, remains a matter of chance. Whether it be preservation of archival material, or the incorporation of material into archives in the first place - or the preferential taphonomic preservation of a site in a location that has otherwise been subject to significant impacts; liminal individuals, their archival and historical record, and metaphorically the archaeological record often are only encountered by the researcher as a matter of chance – if they happen to be looking. Or to paraphrase John Gaddis: biography is only possible if the historical individual is brought to our attention by the survival of some source or structure (Gaddis 2002, p.121).

One cannot define liminality without contrasting it to the main body relative to which it is liminal. This approach is familiar to most archaeologists under the guise of comparison of

individual or select artefacts to an assemblage. I continue my metaphorical application of treating archaeologists as artefacts to my identified subjects of Dermot Casey, Alexander Gallus and Elsie Bramell – all of whom preliminary research would indicate are under-represented in the history of Australian archaeology, I then contrast these individuals with their participation in the community of scholars of Australian archaeology.

The notion of a community of scholars was first explicated by Paul Goodman in his 1964 work *Compulsory Mis-education and The Community of Scholars* (Goodman 1964). Goodman proposed that since the 12th century when western universities commenced taking their current shape, there has existed a socio-economic and self-regulating community of scholars. This community self-determines entry and expulsion to and from it, and its terms of membership are as much predicated on their unwritten socio-economic (and often gender and religious) criteria as they are on any academic stipulations (Goodman 1964). Only six years after Goodman's work, Pierre Bourdieu published similar findings in his *Reproduction in Education, Society and Culture* (Bourdieu & Passeron 1990 (1970)). Bourdieu proposed that education generally, in particular tertiary education, served primarily as a mechanism of social reproduction for the socio-economic class that had dominated and formed the tertiary system to date - the middle class. The middle class possession of cultural capital – of the habitus / lifeways associated with success in education, combined with education itself to create middle class wealth and power. This thesis does not examine the creation of wealth and power. It does however examine the ways that elements of lifestyle, class, foreignness and gender – all of which may constitute elements of habitus, have combined to exclude or marginalise Dermot Casey, Alexander Gallus and Elsie Bramell from full participation in, or from recognition of, their participation in the community of scholars of Australian archaeology.

I have here avoided carrying out a prosopography, or collective biography of Australian archaeology at any given time, in the manner that Stephanie Moser has so adeptly produced (Moser 1995). Although as I have mentioned, describing the liminal requires understanding of their position relative to the mainstream, I feel that this relationship emerges in itself through an in-depth exploration of the lives of these liminal individuals. Adoption of a biographical approach to exploring relations between the liminal and the mainstream is one that is well overdue in archaeology. In their work *The Turn to Biographical Methods in Social Science* (Chamberlayne, Bornat, & Wengraf 2000), the authors described as a paradigm change, the extent to which there had been a shift of thinking towards biographical methods in shaping the agenda of

research (Chamberlayne, Bornat, & Wengraf 2000, p.1). The biography here is not seen as simply a narrative of life events, but as an investigation that is grounded in the relationship between the agency of the individual and broader structures (Chamberlayne, Bornat, & Wengraf 2000, p.3). In short, the contrast between the individual and community of scholars that I have outlined above. This approach was presented over 20 years ago as being newly paradigmatic in the social sciences by (Chamberlayne, Bornat, & Wengraf 2000). Yet, no such paradigm seems to have swept writing on archaeological history. Perhaps this is due to an underlying and often overwhelming tendency towards empiricism and positivism among many archaeologists (Johnson 2011). Similar positivism has been noted by Maynes et al (Maynes, Pierce, & Laslett 2008, p.98) as underlying social science resistance to the use of personal narratives and biographies in disciplinary history.

Such resistance to exploration of individual agency and to the role of liminal biographies in the formation of a history of Australian archaeology is understandable in a discipline such as Australian archaeology that often defines itself by its very empiricism, or which considers positivism to be theory-free common sense.

Throughout this thesis I make mention of the foundational work in the history of Australian archaeology carried out by Stephanie Moser (Moser 1997). At times I cite Moser in agreement, but more often not so as I believe the methodology employed by her feeds into the prioritisation and emphasis of the very existence of the category of 'professional' archaeologist, a category that is of dubious utility and which further marginalises liminal practitioners. Moser (1997)

As will be elaborated shortly, this thesis does not adopt a strict positivist ontology, for to do so would disable the use of hermeneutics which is among the most long-standing and fundamental methods of historical analysis available to me. Hermeneutics arose to particular prominence in 18th century Germany. In a longstanding difference, the theorist and historian Leopold von Ranke (1795-1886) insisted it was the role of history simply to identify and chronicle facts. His shorter-lived opponent, but likely more influential in the long term was Johan Gustav Droysen (1808-1884) who was of the opinion that: 'Facts themselves do not speak except through the words of someone who has seized and understood them' (Muller 2008, p.29). It is the second last word in this quote 'understood' that is crucial. Droysen saw the accumulation of facts as only the first step in history, with the second and chief step, being the vitiation of these facts through *verstehen* / understanding. *Verstehen* was in Droysen's mind an empathetic process through which the historical individual could be comprehended in and of themselves. The

hermeneutic turn then consists of understanding a particular person or phenomenon as part of a whole - and in comparison to that whole, in the explicit understanding that the whole had been constructed by the historian. Without the basic tool of the hermeneutic turn there would be little capacity to evaluate my case studies against the larger picture.

The literature on the writing of biography is itself replete with metaphors (Gaddis 2002, p.128). A particularly powerful metaphoric piece is that provided by the celebrated biographer Leon Edel (1907-1997). Edel describes the process of untangling and making sense of a biography as one of discerning 'the figure under the carpet' (Edel 1986). In this metaphor, Edel refers to the manner in which any knotted carpet has a pattern that is visible on its face side. Yet this is merely a face value deception so to speak – the true functional manner by which the carpet hangs together and is constructed - the figure of its myriad and complex knots and ties, are visible on the rear side of the carpet only. It is the biographer's task, Edel insists, to develop the skill and insight, that through perceiving the face of the carpet, one can deduce the figure underneath it. Without explicitly doing so, Edel has provided an almost perfect encapsulation of the hermeneutic process in action – one of constant moving backwards and forwards between what appears to already be, and what yet might be. Edel's longer explication of the processes of biography is largely couched in phenomenological terms, emphasising the need for an emic understanding of a subject.⁹ What Edel refers to as an emic approach can here be taken as an empathetic *verstehen* stance.

A further metaphor can be brought to bear on the manner in which sources for this thesis have been identified and derived. The process of Aboriginal archaeology in Australia, largely relies on sieving soils from areas identified as of interest. Generally small potential artefacts then appear from a mass of dust and soil that has been scraped or sluiced through metal sieve plates, and which are then to be scrutinised whether they are indeed artefactual or merely more natural deposit. So too, much of the closer and more insightful evidence that I have gathered for this thesis has not constituted the majority of the material available, but has been comprised of peripheral evidence, such notes on the side of letters and personal letters themselves which seem oddly incorporated into academic archives such as the Casey archive MS 1326 at the

⁹ 'Emic' and 'Etic' are terms perhaps more familiar to linguists and anthropologists. In brief, emic relates to the study of a culture (or person) in terms of their internal elements, while etic relates to a study of the same subject, but from an external view

Australian Institute of Aboriginal and Torres Strait Islander Studies, or the Papers of John Mulvaney' at the National Library of Australia. These items provide not only chronological evidence, but also understanding of perhaps unguarded personal attitudes and dispositions. Valuable colour and depiction of personal character - even if only sometimes brief in nature has come from approaches made to organisations such as Eton School, St George High School Kogarah, Dr Alexander Gallus Junior, and Hilary duCros for her recordings of Elsie and Frank McCarthy.

Available detail on the three chief case studies of this thesis – Dermot Casey, Alexander Gallus, and Elsie Bramell has varied significantly both in scope and degree of insight. Dermot Casey's life came almost wholly to view as a result of his family's social pre-eminence and also through his generally tangential mentions in accounts that focussed more closely on his brother Richard's impressive career. Alexander Gallus certainly published fairly prolifically, but to the best of my knowledge authored no autobiographic pieces. Such biographical information was left to be disclosed by his admiring student Vincent Megaw, or many years later in correspondence with Gallus' son. Fair insight and estimation of the conditions of Gallus' life in Australia could also be gained from evaluation and analysis of the manner in which he was treated by the Australian archaeological and academic world in general.

Elsie Bramell is almost historically invisible, and it is a tribute to her brilliance that she made a mark in the annals of the Australian Museum and Australian Anthropology. It is largely by a contextual and comparative approach that we can appreciate something of Elsie's vitality. The frequency and vibrant topics of the public talks given by Elsie for the Australian Museum, in addition to the at times novel venues in which these talks were given, speak of a new drive and energy in the Australian Museum. Yet, the titles and locations of these talks are often all we know of them. Still, it is a fair assumption that these activities formed only a part of her vivifying efforts at the Museum.

While I do not adopt a positivist ontology, I must admit that emic approaches are inherently at risk of the pitfalls of intersubjectivity (Nagel 1974) and speculation. The few authors who have written theoretically around writing biography encourage such speculation: 'Biography is the simulation, not the ledger book of an existence' (Kendall 1965, p.25). Or earlier yet is Gamaliel Bradford's insistence that it is the role of the biographer to ask highly speculative questions, such as what would the lives of Cromwell or Napoleon have resembled were they born 100 years earlier? (Bradford 1932). The dangers to the biographer of engaging in such overly speculative or empathetic analysis and producing what is in effect a work of fiction is innovatively pointed out

by Michael Rustin, who observes that works of fiction have become closer to truths of many peoples subjective experience than are works of non-fiction. Fiction therefore tends to provide mental templates for many people's understanding of the world to the extent that 'the store of ideas they use in making sense of the world has probably been more shaped by so-called 'fictional works'' (Rustin 2000, p.39).

In order to avoid on the one hand, the constraints of positivism, and on the other hand the pitfalls of speculative relativism, I have attempted to evaluate evidence here through the philosophical lens of critical realism.

The historical development of critical realism parallels the early twentieth century polarisation of thought along lines of objectivism vs subjectivism, realism vs idealism, positivism vs relativism and their cognate later expressions in processual and post-processual schools of archaeological thought. These polarisations were allied with developments in quantum physics on the one hand, and phenomenological philosophers such as Husserl and later Heidegger on the other. The philosophical ramifications of relativity, saw debate arise amongst physicists, grappling with newly pertinent questions as to the nature of reality and being; whether unobservable entities could be considered 'real' in the same sense as those readily observable (Born 1950, 1953, Mayo 1954), whether any data could be considered as observer-independent, and indeed whether there was much sense in talking of the 'real' at all (Dingle 1951, Margenau 1950).

Phenomenological philosophy in contrast took reality as a self-evident given 'the external world exists, it is what it is' (Husserl 1931, p.107). In the tension between these two poles, a form of realism gained prominence, in reaction to the 'naïve' realism of positivism (Drake *et al* 1920, p.vi). These new realists acknowledged on the one hand an underlying *belief* that the world was real, material and existed independent of human observers – a materialist ontology. On the other hand, sympathetically reading existential philosophy (Putnam 1987, p.4), they acknowledged that our perceptions of this real and external world were inescapably personally, culturally and psychologically constructed, and therefore sought 'to combine the insights of both subjectivist and objectivist philosophy' (Drake 1920, p.4). Following Sellar's early work (Sellar 1916), an initial and foundational statement- the co-operative work *Essays in Critical Realism* (Drake *et al* 1920) saw scholars such as George Santayana articulate a vision of a realist position informed by phenomenology and imbued with a humanitarian spirit while maintaining a scientific attitude. Their depiction of a *belief* in an external world – rather than a statement of it

as a fact (Drake 1920, p.; Santanyana 1920, p.177; Santanyana 1953, p.522, Sellars, 1920, p.189) also Passmore (1988, p.158) represents an important resolution.

Concluding that ultimately, absolute proof of anything is philosophically impossible (Santayana 1953, p.522), they admitted that at core, our philosophies are built upon beliefs. Nevertheless, critical realism maintains that our daily experiences and mode of living are consistent with a relatively realist belief that the world does exist independent of our abilities to perceive and describe it (Putnam 1981, p.151). This external actuality may be depicted as existing independent of our consciousness (mind independent) and unaffected by our level of knowledge of it (Maki & Oinas 2004, p.1759). Further, that no belief can be absolutely true or false, - for critical realists consider such absolute poles fictitious (Santanyana 1953, p.468, Putnam 1987, p.77, Maki 2001, p.12818). Rather, that thought objects / objects of knowledge – the theories and ideas we construct as ‘reality’, and which form our perception of the external world, can be evaluated as whether they correspond to an external world or ‘actuality’ they purport to represent, by among other methods, comparison and ascertaining those aspects which seem common - a coherence valuation (Maki & Oinas 2004, p.1762, Putnam 1987, p.77). The development of critical realism, from outset then parallels elements both of positivist scientific thought, and also hermeneutic qualifications, but remains insistent upon the self-admissions of weakness inherent to both perspectives.

The principle that admitting facts are theory laden, does not mean that they do not exist in full reality, is a moderating view which when applied to archaeological representations of the past accepts that these may distort the picture of the past, but that this distortion does not mean they do not mirror the past in any way at all (Maki 2003, p 65).

In summary then, I set out here to explore the biographical past, well aware that my interpretations of evidence relating to it may be flawed, but nevertheless confident that if consistent and explicit methodologies are applied to evidence of the past, that the scale of these flaws should be constrained, and a picture of the past will be derived that does indeed bear resemblance to bygone people, events and processes. A picture that is enhanced and enriched through applying the metaphorical archaeological method to the study of its own past. The benefits of this perspective will be obvious I hope, in bringing new and more nuanced light to bear on the people and past of Australian archaeology, its formation, growth and more intimate biographical characteristics through time.

2.8 BIOGRAPHY AND HISTORY – A BRIEF OVERVIEW

Despite the literature that I have reviewed below, in 2010 Barbara Caine could still plaintively state with regards to literature on the relationship between biography and history ‘there is not yet a substantial literature to refer to’ (Caine 2010).

The relationship between history and biography has long been fraught, with historians as far back as classical times expressing the view that biography is only an inferior form of history, if it warrants inclusion as history at all (Caine 2010; Salvatore 2009). The role of the historian, it has long been maintained, is the broad sweeping and analytic examination of topics such as an era, an epoch, complex wars and population transfers. The biography of notable individuals whose lives impacted at such scales were perhaps regarded as a secondary historical undertaking, but not as themes that would occupy primary space in the corpus of an established historian.

Much of this attitude can be understood as a reflection of longstanding patriarchal dominance both in society generally and in the academic world that it produced. However, by the late 1960’s and certainly the 1970’s, the emergence of the feminist history movement opened portals that facilitated the use of biography as an effective tool for understanding the rapidly developing field of gender history (Banner 2009; Caine 2010). At much the same time, and emerging from late 20th century French scholarship, ‘Ego-histoire’ is a form of investigation that makes explicit connections between an author’s autobiography and their own intellectual output (Cole 2019). Cole (2019) brings such Ego-histoire self-observation to bear on her own sense of dislocation in moving between countries. Aurell (2020) applies similar analysis to his own life experience and scholarship noting that his interest in historian’s autobiographies was fueled by ‘the possibility of being a part of and apart from history’. Nevertheless, in seeming contrast to Ego-histoire, Aurell (2019) generally cautions against the combination of autobiographic and biographic texts, given the distinct methods of their production. Following the developments in the application of biography to feminist and gender investigations, by the 1990’s historians commenced regarding biography as providing a unique lens, not only into the life of the subject individual, but more cogently into the world within which they lived (Caine 2010). Likely spurred by these observations and the application of biography to studies of gender, Parke (1996) makes the fascinating distinction between Majority Biographies and Minority Biographies – those biographies in which the author or the subject belong or are excluded from classes of people characteristically the subject of a biography. The move towards the currently popular mode of ‘life writing’ (Caine 2010) incorporates efforts by modern biographers to fully depict the emotional, personal and public lives of its subjects, often using

sources such as letters and diaries that were traditionally little used in biography (Caine 2010). Such sources have constituted an important part of my thesis too.

Both Banner (2009) and Caine (2010) make the cogent point, perhaps not regarded considerably by some academics, that 'Biography is the major literary form in which academics can reach general audiences' (Caine 2010). It has been with this principle in mind that from the outset I have sought to write this thesis in a manner that is accessible to the general public.

Chapter 3: Dermot Casey – the early years

3.1 AN UNDER-RECOGNISED FIGURE

My first case study of a relatively liminal figure in Australian archaeology is that of Dermot Casey (1897-1977). Casey has been the subject of a recent article which investigated the under-appreciated nature of his contribution to the discipline of Australian archaeology (Spriggs 2021). Spriggs observes that Casey's name is not widely known among Australian archaeologists. Although Spriggs provides a useful catalogue of chief points in Casey's career, with some personal insights, the aim of this and the following chapter are distinct from the approach adopted by Spriggs. Here, I hope through a more empathetic and emic contextual reading of Casey's biography, to bring to light clearer and more nuanced insights to Casey, his relationship with the nascent discipline of Australian archaeology, and in particular his relationship with a linchpin figure of the discipline at the time, Derek John Mulvaney.

The scant disciplinary memory of Casey is curious, as Casey made substantial and invaluable contributions as a 'distinguished pioneer' (Mulvaney 1977, p.225) to the fledgling discipline of archaeology in Australia. Indeed, without Casey it is likely that excavations including those which have been described as 'The Dawn' of Australian archaeology (Griffiths 2017) and which founded the Australian career of John Mulvaney (1925-2016) 'the father of Australian archaeology' (Frankel 2016) could not have taken place in the successful manner that they did (Denning 1997, Inglis 1997, Mulvaney 1977). This chapter does not purport to represent a comprehensive biography of Casey. Rather, it looks to explore from documentary records what it can of a single thematic investigation regarding Casey. This theme of inquiry is the apparent discrepancy between the large influence Casey had on the formative years of Australian archaeology, and the seeming lack of recognition of his efforts even by scholars whom he assisted greatly. I demonstrate that this locus almost certainly results from an interplay of personal dispositions and historically specific social expectations and mores. Dermot Casey's personal modesty can be tied to a personal

shyness, but more broadly it cannot be regarded independently of the expectations for self-effacing behaviour characteristic of his social milieu and of the rigid notions of class and position that were current during Casey's lifetime. Teasing out a more human and possibly more emic understanding of why this pioneer of Australian archaeology is so unrecognised requires both a broad historical context and a fine-grained analytical biographical approach to interrogating and extracting nuance from the relatively small body of evidence available on Casey.

In a study of Gordon Childe (Lever 2015), I have taken some preliminary steps towards demonstrating the application of the methods of critical biography to the history of archaeology. In this study of Childe, noting the paucity of autobiographical material or of biographical sources on Childe at a personal level, I examined newly available evidence from British MI5 files held on Childe (Lever 2015). In that article, against a background of the known political and social environment of the time, I considered the levels of intelligence observation and the likely constraints on Childe that these files evidenced, and I explored the likely impacts of these intelligence activities on Childe's life. Like Childe, Dermot Casey appears to have been an intensely private individual on whom little published personal biographical information is to hand, and no published autobiographical material is available. As in my study of Childe, attempts to eke out a better understanding of Casey will largely be reliant upon critical interrogation of second- and third-party documentation. This is augmented by an investigation of the documentation and personal correspondence by Casey that is available to me from the Casey archives held at the Australian Institute of Aboriginal and Torres Strait Islander Studies (AIATSIS), and correspondence and documents in the Mulvaney archives at the National Library of Australia.

The library at AIATSIS holds an archive titled in the library catalogue as "Casey, Dermot A. Papers, manuscripts and letters on prehistory and archaeology [manuscript C.285 items incl. 192 letters] - MS 1326" (The Casey archive). The contents of these cardboard archival boxes are often not individually dated, nor individually numbered. The archive does however contain a detailed 'Finding Aid' which consists of a typewritten list of the archive's contents, subdivided by category, subcategories and sometimes date. In the following chapters my references to items in the MS 1326 (Casey archive) will refer to their category, subcategory and date (if given) as provided in the finding aid, as the most accurate citation

method available to me. The finding aid for the Casey archives at AIATSIS lists approximately 285 items including 192 letters. These provide a trove of insights not only into Dermot Casey himself, but also into his respondents who were frequently archaeologists and scholars of note. Nevertheless, this archive is by no means a complete or even a statistically robust sample of Dermot Casey's correspondence, nor can it be taken as a direct or accurate reflection of attitudes and opinions that Dermot Casey may have held. Like the archaeological record, this archive is often a largely opportunistic collection gathered through uneven formation processes. Gaps in this archive are explicitly identifiable by references in correspondence to related items of correspondence that are missing. Despite this flaw, it is an inherent aspect of archaeology and a skill of archaeologists to read meaning from partially preserved and unevenly sampled materials. So too it is with the Dermot Casey archive. While not claiming the archive to provide a comprehensive representation of, it is still a source of illumination into, what appears otherwise to have been a privately led life. I did not know Dermot Casey personally. No doubt individuals who worked with Casey may have had perspectives and information that are at odds with my findings, and greater archival evidence regarding him will almost certainly be uncovered in the future. Nevertheless, I believe that valuable added information can be obtained from the fragmentary information available to me.

Toward these aims I will begin here with a biographical sketch of both Dermot Casey, including a brief account of the broader *Sitz im Leben* (context or place in life and society) into which Casey was born. The notion of using *Sitz im Leben* as a biographical investigatory technique is long established in classical and biblical text criticism. Formulated first by the German scholar Herman Gunkel (1862-1932), the technique rose to considerable prominence in the Form Criticism school of biblical analysis (Gunkel 1903, Mihelic 1951). There it serves to enhance the investigation of real (or fictional) characters about whom relatively little is known compared to what is known of the worlds in which they lived. The technique assists in evaluation of ranges of behaviour by figures under study. Through placing reported actions by a historical figure within the social context they are reported as having occurred in, greater appreciation of the implications and normative or extraordinary nature of such actions can be obtained.

3.2 THE MAKING OF THE CASEYS

Dermot Casey's heritage was hardly one of old-world genteel status. His grandfather Cornelius Casey was born Catholic in 1811 in Liverpool. It is possible that he abandoned Catholicism to study medicine at the Protestant Trinity College in Dublin. Although Catholics were first permitted to study at Trinity in 1792, ongoing sectarian tensions existed, with the Catholic Church maintaining a ban on attendance there until 1970. As evidenced by Dermot Casey's own marriage in 1924, the Caseys were by then practicing Anglicans. In 1833, two years after his admission to the Royal Society of Surgeons, he arrived in Sydney, NSW, and then debarked for Van Diemens Land where a variety of incidents arose between Casey and the military command which were resolved by removing Casey at his own request to New Norfolk, outside of Hobart (Casey 1966, p.31). In Van Diemens Land Casey married Loetitia Gardiner with whom he had a son, Richard Gardiner Casey in 1846 (father to Richard Gardiner Casey Junior and Dermot Casey) (Hudson 1986, pp.1-2). Shortly after the family's relocation to Port Phillip in 1863, Richard Gardiner Casey was placed by his father as a jackaroo on a sheep farm near the Murray in New South Wales. He would not appear to have completed his schooling (Hudson 1986, p.59). In 1875 after twelve years of mobile employment at a variety of stations, Richard Gardiner Casey accepted an offer to manage a sheep station, Kilfera, in the Willandra region of New South Wales. Here he proved himself a competent administrator, to the extent that when he left Kilfera he was sufficiently confident to enter into a partnership, commencing in 1883, to purchase a very large station (217,000 hectares), Terrick, in Queensland. The venture went poorly. Not only had Richard Gardiner Casey not sufficiently investigated the details of the partnership offered him, but climate and the unfamiliar vegetation and landscape saw him continually struggle to break even financially. Nevertheless, on paper a man of substance, in 1888 Richard Gardiner Casey married Evelyn Harris, daughter of a wealthy merchant and Member of the Queensland Legislative Council (Hudson 1986).

Notably absent from any biographical detail on Richard Gardiner Casey is any account of his interaction as a squatter / early pastoralist, with the Aboriginal nations of Queensland. The processes of violent displacement by which squatters and pastoralists expelled Aboriginal people from their country (has been relatively better documented in New South Wales and Victoria, particularly where contemporary accounts are available from state appointed

'Protectors of Aboriginals' (Robinson 1988, Butlin 1983). It is only recently becoming evident that the brutality of these processes in south-eastern Australia paled in comparison with the often unfettered murderous nature of colonial expansion in Queensland. There, often operating far in advance of the judiciary and its oversight, squatters and pastoralists 'cleared' land of its Aboriginal owners while maintaining a culture of silence internally and externally as to their depredations (Bottom 2013, Richards 2008, Evans, Saunders & Cronin 1993). The numbers of Aboriginal lives lost in these years has been fairly described as a genocide, the enormity of which is only recently coming to light (Reynolds 2021). Recent archaeological and historical investigations have been carried out into the role of the Native Mounted Police in enacting atrocities against Aboriginal people on the Queensland frontier (Burke, Barker, Wallis, Craig, & Combo 2020, Wallis, Burke, & Dardengo 2021). The Native Mounted Police were a paramilitary force which operated in Queensland from the mid-19th century, with the last of their stations only closing in 1929. Recent estimates are that Aboriginal deaths at the hands of the Native Mounted Police and white settlers for the period between 1859 and 1897, are likely to be over 100,000 (Wallis, Burke, Barker, & Cole 2021).

It is certainly speculative to ponder what, if any, role Richard Gardiner Casey may have had in this process which played itself out while he was himself a Queensland pastoralist. I would propose nevertheless, that given the part that Richard Gardiner Casey played as an intergenerational role model in his family, it is worth lending some consideration to his likely lived experience as a white male jackeroo / station manager. And, to place this existence in contrast to the two hagiographic biographies of him (Casey 1966, Hudson 1986) including one biography by his namesake son (Casey 1966).

In 1893 Richard Gardiner Casey left Queensland for Melbourne, neither in the best financial or physical health. After a brief foray in the West Australian goldfields he was nominated through a friend to a directorship of the then-troubled company Goldsbrough Mort & Co, in which he purchased a significant holding. This position and his share holding in Goldsbrough Mort provided the basis of the family's ongoing wealth as Goldsbrough Mort developed to become one of Australia's largest wool merchants (Hudson 1986, p.158).

The Casey family background then was quite distinct from that of many leading Melbournian families. The establishment of Melbourne (and Port Phillip as the area of

Victoria was then named) was intended as a middle-class entity, free of penal colonies or convicts and in which, notionally at least, use of land would be negotiated with its Aboriginal owners. To some extent this intent was achieved – and at least in self-image Melbournians perceived themselves as a more genteel society than Sydney and those further north (Serle 1963, Serle & Grant 1957, Davison 1978, Flannery 2010).

In contrast, Richard Gardiner Casey, father of Richard Gardiner Casey junior and Dermot Casey was born in Van Diemens Land shortly after the end of a brutal war against its Aboriginal population, and within the active period of penal colonies there (Brodie 2017). Leaving school early to jackerroo, he had run remote sheep stations with mixed success, married well to a wealthy woman 19 years his junior and received career making assistance from an acquaintance.

3.3 THE WORLD INTO WHICH DERMOT CASEY WAS BORN - LATE VICTORIAN-ERA MELBOURNE

Dermot Casey was born in Melbourne Victoria in 1897 into a distinct historical, social and geographical context. The culturally vibrant and ethnically diverse city of Melbourne (current population 4.8 million) is the capital of the State of Victoria, Australia. Victoria is southerly, cooler and smaller in comparison to most other Australian states and as mentioned above, through a variety of factors (including not having been settled as a convict colony) Melbourne has always claimed for itself a more genteel status than some other Australian capital cities. Certainly, the wealth of the colony had been founded on the gains of the often wild and lawless goldfields that rushed from 1852 to the end of the 1860's. Yet this lawlessness and the massive migration associated with the gold rush also allowed for considerable social mobility, personal reinvention and for a golden veil to be drawn over an impoverished or less than salubrious past (Hayes 2017).

When Dermot Casey was born, Victorian-era gentility and claims to gentility abounded, backed by Melbourne's prestigious status at the time as the second-largest city in the British Empire (population 400,000), only outranked in size by London itself. Asa Briggs treated Melbourne as the only city outside of the United Kingdom worthy of full inclusion in his classic study of *Victorian Cities* (Briggs 1963). The mantles of gentility and class consciousness weighed heavily on inhabitants of Melbourne of the late-nineteenth to early-twentieth century. While social stratification in Melbourne may not have embodied the

clear distinctions that could be drawn in other Australian colonies such as between convict or free settlers, nevertheless stratification was clearly evident and was predicated on complex identities based on heritage, religion, rank and education, but most powerfully on wealth (Serle & Grant 1957).

The roles, behaviours, expectations and social modes of conduct of the English gentry are evident among the established elite in nineteenth-century Melbournian society, often referred to as the 'squattocracy' in reference to their rise to wealth through early land grabs and livestock raising, employing tactics of violence towards Aboriginal people that formed a template for similar actions in Queensland, described above (Wallis, Burke, & Dardengo 2021, p.3).

Wealth and respectability in nineteenth century Melbourne were clearly geographically signalled through aspects such as residence south of the Yarra River (Lancaster 1969, Davison 1979). Yet wealth alone did not suffice for entry to the social elite. The White, Anglo-Saxon Protestant elite stood apart from even those of the Irish, Catholic and certainly Jewish citizens who may have reached the financial threshold for membership in the elite, but who were deficient in social and ethnic capital (McConville 1987). The Australian gentry not only modelled themselves on England but were more assiduous than the English in self-enforcing rules of behaviour and in vigorously upholding the almost impermeable barriers between themselves and other Australians (Waterhouse 1995, pp.35 106-107). By definition gentlemen at this time did not work, and effort to gain advancement through remunerated work was 'poor form', almost a belittlement of the gentlemanly repose in which they were expected to live recumbent. Acceptable forms of competitive behaviour among the gentry were largely restricted to participation in sports, exploration and the army – achievements in which were still subject to gentlemanly codes of modesty or self-deprecation (Lawrence 1827, Miles and Savage 2012).

Despite these constraints, it was also not done for members of the gentry to be seen as indolent or idle if they were not involved in the running of family empires. This is reflected in the prodigious spirit of public service and duty to Empire demonstrated by its more privileged Australian sons and daughters through the nineteenth and twentieth centuries (Bessant 2006, Brett 2003, Menzies 1939, Sherrington and Campbell, 2004). Besides work in the public service, a respectable occupation resulting in advancement of the common good

could also be found for the gentry in the amateur development of expertise in areas such as collecting, particularly in the natural sciences and in the arts, or in the status of a man of letters, or involved in an honorary capacity assisting those pursuing these aims for pay (Pearce and Bounia (eds) 2001). The term honorary is significant. The gentleman class may bestow bequests and graciously interact with their recipients, but the notion of accepting largesse from others, let alone scholarships or even payment for assistance would be unthinkable. Assistance and involvement by the gentry in intellectual endeavour was honorary and altruistic, done for the greater good (Lawrence 1827; Stamer 1874).

Stephen Garton observes that class and gender relations in 19th century Australia were inherently enmeshed, noting historical interpretation that the notion of manliness then current 'bred a particularly virulent form of masculinism, which marginalised women' (Garton 2002, p.54). Alternatively, Garton suggests some historians have observed that it was as a result of a cross-class Australian male egalitarianism in the 19th century that the female middle class took the role of policing class distinctions and codes of respectability and gentility (Garton 2002, p.53). What is not disputed however, is that gender roles were tightly proscribed. Nor does it seem contentious to observe, that then, as now, gender and class identities were almost inseparably interlinked performative acts (Butler 1988). It is as a member of this category of the Victorian gentry, the honorary dilettante scholar, that we encounter Dermot Casey, born to the Victorian squattocracy in 1897.

3.4 DERMOT CASEY – A LIFE IN HIS BROTHER'S SHADOW

Dermot Casey was the second son born to Richard Gardiner Casey and Evelyn Jane (nee Harris). Dermot's older brother and only sibling Richard (junior) had been born in 1890 in Brisbane. The degree to which the lives of Melbourne's social elite such as the Caseys were lived under scrutiny and provided a source of popular entertainment is reflected in a newspaper report in which Dermot Casey appears as a young child. On 28 December 1905 *Table Talk* reported that on 23 December 1905 Dermot and his mother had attended 'an afternoon party and a Christmas tree' hosted by a young child at a South Yarra property. Dermot would have been seven years of age at this time. Some nine months later, in September 1906 a detailed account is given of a birthday party at the Melbourne Zoological gardens, describing events, décor and the birthday cake, and listing individually each of the

70 parents and children in attendance, including Dermot and his mother (*Australasian* 15 September 1906).

Dermot was seven years younger than his older brother Richard junior. This difference in years meant that from a relatively early age Dermot likely already lived in the shadow of his brother's achievements – a pattern that appears to have become lifelong. Their father was a demanding authoritarian with little emotional connection to either of his sons (Hudson 1986 p.13, although he appears to have struck a stronger connection with his namesake Richard Gardiner Casey junior (Casey 1966).

Richard junior seems to have functioned as something of an intermediary and perhaps protective buffer between his father and Dermot who he described as 'more relaxed and less responsive to parents' (Casey 1966, p.14). In at least one documented case and likely many more, Richard junior can be seen acting *in loco parentis* when reporting to his father (then overseas) that; 'Dermie is well and does whatever I tell him when you are away' (Casey 1966, p.17). I would propose we might already envisage here (although it may be premature) an image of young Dermot Casey as a reserved individual, happier to let his older brother bear the burden of their father's aspirational projections. Possibly relevant too, is that many years later, in 1955 when he was aged 58 Dermot was described as having a mild stammer (Mulvaney 1986). From the image available of Richard Gardiner Casey senior, little would suggest he would be a parent tolerant of a flaw in his children such as a speech impediment. But even aside from the influence of his father, it is likely that Dermot Casey's personality from early on was shaped by a willingness to let others take the public lead. A stammering individual of by the times extraordinary height, Dermot may have sought to avoid attention generally. Dermot's recorded height was 6'4" or 193cm (Morrison 1956). This is a notable stature by modern standards and would have been markedly more notable when Dermot attended Eton, when estimated average heights of English males was 5'6" or 169cm.

Dermot's older brother Richard Gardiner Casey Junior was at various times (among other achievements) a highly decorated army officer during World War I (Military Cross, Distinguished Service Order), a member of Winston Churchill's war cabinet, Governor of Bengal, and Governor General of Australia from 1965 to 1969. Richard Casey junior was elevated to the House of Lords after being created Baron Casey in 1960, was knighted in

1965 and elevated to the Order of the Garter in 1969 – the same year that he was named Australian of the Year. Richard's talents included those of academe, graduating from Trinity College Cambridge in mechanical sciences, having rowed and debated for his college (Australian Dictionary of Biography 1993).

Richard had chosen to study at Cambridge despite the easier alternative of study at the University of Melbourne. A brief study of the nature of the University of Melbourne at that time makes it apparent that degrees offered there were of distinctly lesser character, and unlikely to meet the demands of Richard Gardiner Casey senior or junior. The University of Melbourne had attempted to bolster enrolments by abolishing compulsory attendance at lectures, thus enabling country students to self-educate. Similarly, the university sought to increase the numbers of graduates by issuing large numbers of "*ad eundum*" degrees. These were University of Melbourne degrees bestowed on those who had undertaken most or all of their study overseas (Scott 1936, Selleck 2003, Macintyre & Selleck 2003).

Despite Richard's considerable achievements, Richard's wife Maie described both Richard and Dermot as shy and retiring types from childhood who took after their dour father and were both often embarrassed by their flamboyant mother Evelyn (AIATSIS Casey Archives (Casey Archives) File Reference (FR) C.a-c: no date). Maie Casey, a pioneer aviator, writer and artist was herself hardly a shy and retiring type (Australian Dictionary of Biography 1993).

In 1911 Dermot accompanied his father and his brother Richard to Christmas with an old friend and business associate of Richard senior - Thomas Hall of Weeting Hall Norfolk (*Queensland Morning Bulletin* 26 January 1911). No mention is made of Dermot's mother. Dermot would at this time have been at the transitional age of 12 years, while his brother Richard had already gained entry to Cambridge University. This trip to England by Dermot and his father probably served an educational purpose, as Dermot appears on the Eton College Register from April of that year to December 1915. According to a brief note on Casey family history by Mae Casey (Casey Archives FR: C.a-c: no date), Dermot spent his holidays while at Eton with the apparently austere Hall family. During his time at Eton, Dermot was regularly visited by his older brother who was then studying at Cambridge, and it does seem that a relationship of genuine almost avuncular care existed between Richard

and his younger brother. It would appear that the planned course was for Dermot to follow his older brothers' footsteps to Cambridge.

No great honours are listed for Dermot's time in Eton, although he did reach the rank of sergeant in the Eton Cadet Corps and was a member of the school Shooting VIII in 1914 (R. Fisher, Curator Eton Archive pers. comm. 2016).

It is in this role as a member of the Eton Shooting VIII that Dermot was photographed in the image below aged 17 (Figure 2). Seated at far left he is the only team member not looking directly at the camera. Rather he appears to somewhat wistfully gaze to the distance, and his slightly slouched and passive posture neither makes evident his considerable height, nor matches the demeanour of his fellow shooters.

Figure 2: Eton College 1914 Shooting VIII. Dermot Casey seated far left (Eton College Photographic Archives)



With the outbreak of World War One, Richard Gardiner Casey junior commenced his illustrious military career. It is notable however, that even in these early stages of the war, when perceptions of an early victory were rife, and public support for the war had not yet been coloured by the return of masses of maimed and injured soldiers, Richard Gardiner Casey junior attempted to dissuade his younger brother Dermot from enrolling (Spriggs 2021, p.2). This too would speak of a relationship in which Richard Gardiner Casey junior demonstrated a concerted protective attitude his younger brother.

Dermot did not go on to university but went from Eton to the Royal Military Academy at Woolwich before serving in France as a Lieutenant in the Royal Horse Artillery between 1916 and 1919 (R. Fisher Curator Eton Archive pers. comm. 2016). Leadership in the Royal Horse Artillery required an unusual set of practical skills. The regiment consisted of teams of horse-drawn small artillery guns, which would ride often at high speed into frequently hazardous locations, slew to a halt with guns facing their target, fire, and remove before incurring the impact of incoming reply rounds. Here Dermot appears to have found a niche for what would later be substantiated as his considerable practical and organisational

ability. In 1918 he was awarded the Military Cross for his bravery in directing his unit's fire in the face of an enemy attack, and this award was reported upon enthusiastically by numerous Australian periodicals, notably those in his mother's state of origin (Brisbane Courier 7 October 1918; Cairns Post 15 October 1918; Queensland Morning Bulletin 15 October 1918).

Although it is skipping ahead of chronology to mention here his later archaeological career, it does seem that the skills, experiences and connections gained during his First World War experience appear to have been directly drawn upon in Dermot Casey's subsequent archaeological career. Two undated items in the AIATSIS Casey archives (FR: A.a.3 and FR: A.c) comprise a doodle of a man firing a small artillery gun, and notes on surveying that utilise a primer on trigonometry issued by the 4th Australian Field Regiment, dated 1943. Certainly the 1943 primer dates to nearly 20 years after Dermot's first military engagement, nevertheless it is more than probable that artillery primers were not part of standard archaeological training material.

3.5 AFTER THE WAR

Dermot would have had only limited if any time to enjoy paternal pride in his wartime achievements. His father Richard Casey senior passed away in 1919. Reports around his death noted that he had left a stipend of £120,000 to his widow, bequests to friends, relatives and servants, and the remainder (presumably the majority of his estate) to his sons (Brisbane Courier 21 May 1919; Cairns Post 3 June 1919). The stipend of £120,000 to the widowed Evelyn Casey equates to an approximate current value of \$10,000,000 (Reserve Bank of Australia 2022). The inheritances of Richard Casey junior and Dermot Casey could safely be estimated as of the order to provide a lifetime of comfort. In the words of Dermot Casey himself "because I was left enough money by my father, I've never really had to work for a living" (Joachim 1965). Richard Casey junior immediately returned to Melbourne to take up his father's business positions (Hudson 2006).

Dermot however, possessed of considerable wealth but little in the way of obligations appears to have taken time to pursue a sporting life. He took up skiing, winning a place in a novice's race at Kosciusko (Sunday Times 25 July 1920), where his presence as a wealthy eligible bachelor did not escape the notice of the Women's News columnist (Sunday Times

25 July 1920). The following year he sailed on the Niagara for Vancouver, from the timing (mid-December) possibly headed for the Canadian ski fields (The Sun 15 December 1921). Little is available on Dermot's activities over the following few years, but by 1924 this tall wealthy sporty type had become engaged to a tall wealthy sporty young lady from South Yarra (Sunday Times 25 May 1924). His fiancée, Gwynnedd Browne enjoyed outdoors sports and riding to hounds, as did her mother Muffie Browne who in turn was the daughter of one of Melbourne's oldest, wealthiest, most socially esteemed and powerful landholders Thomas Chirnside (1818-1890) (Australian Dictionary of Biography 2019).

Apparent similarity between the family backgrounds does not stand up to closer inspection: the Chirnsides, like Richard Gardiner Casey senior, had a past in sheep raising, from which they had made their fortune. However few commonalities beyond this are found between the Chirnside and Casey sheep raising experiences. The Chirnsides held large property in the lush well-watered region of Werribee, barely 30km from the centre of Melbourne, and well within reach for social and shopping excursions. Richard Gardiner Casey senior's properties were remote, hardscrabble and parched.

The combination of notable physical height, wealth, family connections and sporty lifestyle of leisure did not escape the gushing social coverage of the contemporary press around Australia in reporting on the engagement (Adelaide News 28 May 1924; Brisbane Telegraph 28 May 1924). The couple married a few months after their engagement – apparently in the absence of the bride's parents who being overseas at the time were stood in for by John Chirnside, uncle of the bride. The effusive press coverage of the couple's marriage was Australia-wide and highly detailed (Adelaide News 3 September 1924; Argus 28 August 1924; Australasian 30 August 1924; Brisbane Telegraph 3 September 1924; Perth Western Mail 4 September 1924). Media attention to the couple's physical height appears justified. In the images below the happy couple are shown emerging from their wedding ceremony (Figure 2) and they tower head and shoulders above their well-wishers – even when standing on lower steps than the assembled crowd (Figure 3). The top of Dermot's head appears not to have been captured in frame by the photographer for *The Argus*.

Figure 3: Wedding photo of Mr and Mrs Dermot Casey (Brisbane Telegraph 3 September 1924)



Figure 4: Casey-Browne Wedding at St John's Toorak (Argus 28 August 1924)



3.6 EARLY ARCHAEOLOGY

At some time during the 1920's Dermot involved himself in archaeology including attendance at 'a London University course on archaeology' and spent time as assistant to the then Keeper of the London Museum, Mortimer Wheeler (Argus 3 February 1934). I have not been able to determine the specific nature of this university course - whether it was a formal enrolment or informal audit. Regardless, Dermot and Mortimer Wheeler appear to have formed a productive partnership. Dermot worked as a chief field assistant under

Wheeler and eventually led excavation of a Roman castle at Lydney in Gloucestershire under Wheeler's guidance. Wheeler is the subject of effusive gratitude in Dermot's publication of the excavation in 1931 (Casey 1931). This article on Lydney by Dermot is largely descriptive, but technically proficient by standards of the time, and contains beautifully taken photography (a specialisation of Dermot Casey) which documents a very neatly and methodically excavated site. The article had been previously read by Dermot to the Society of Antiquaries of London of which he became a member in 1931. By 1934 Dermot had been appointed a Fellow of the Royal Archaeological Institute. Regardless of the privilege that Dermot may have brought to his relationship with Wheeler, and their likely bonhomie as fellow military men, it appears likely that Dermot's archaeological work stood on its own merits. Many years later Wheeler was to describe Dermot Casey as 'one of the most skilful and percipient excavators within my knowledge' (Wheeler 1955, p. 98).

3.7 RETURN TO AUSTRALIA AND ETHNOLOGY

By the time his 1931 article on Lydney came to press, Dermot and Gwynnedd Casey had taken residence in Melbourne where their daughter was born in 1927 and a son was born in 1929 (Argus 19 September 1927; Argus 24 December 1929). In Melbourne Dermot seems to have kept active in archaeology with a specialisation in anthropology and ethnology and a bent to the public dissemination of these fields. He displayed a consciousness of the potential humanitarian benefits to the Australian working class of better understanding the history and heritage of Australian Aboriginal people. In 1933 he assisted the autodidact polymath A. S. Kenyon (1867-1943) in compiling and delivering a course on Australian Anthropology for the Workers Educational Association (WEA) (Age 25 March 1933). The WEA had its origins as a charitable organisation in the UK and it was invited to Australia in 1913 by the University of Melbourne (Price 1924).

In 1934 one year after his initial engagement with the WEA, Dermot Casey appears engaged at the National Museum of Victoria in a role variously described as 'honorary anthropologist' or 'honorary ethnologist'. In the same year he was responsible for bringing to display in Melbourne casts by the Leakeys of their recent African finds (Age 3 February 1934; Argus 3 February 1934). Casey would continue to hold this honorary role at the National Museum of Victoria for over 40 years. In a notable retrospective observation, John Mulvaney noted in his Obituary of Dermot Casey, that in the early twentieth century many

if not most persons interested in collecting Aboriginal stone tools simply kept these for themselves. Dermot Casey in contrast provided his finds to the National Museum of Victoria (Mulvaney 1977). The scale of this contrast between the norms of the time and Casey's diligence or sense of duty in depositing such artefacts with a museum is brought further to focus by the attention paid by Griffiths and Lever to the magnitude and destructive nature of amateur stone tool collecting in Victoria at the time (Griffiths 1996) (Lever 2016). It must however be noted that Casey too, removed these objects without permission of their traditional owners – an obligation that he would likely not have been aware of in an era when the Aboriginal past was seen as firmly the property of white men (McNiven & Russell 2005).

In the same year (1934) Dermot Casey also took his place as a founding member of the Anthropological Society of Victoria (ASV) along with A. S. Kenyon (1867-1943) and F. Wood-Jones (1879-1954) of the University of Melbourne (Age 29 March 1934). The ASV was not intended as a reclusive intellectual organisation secluded from public life. On the contrary, the meeting which led to the formation of the ASV was held at the Melbourne Town Hall chaired by the Lord Mayor with some 100 attendants. Explicit concerns of the association raised at this meeting were that ethnological and anthropological knowledge of past Aboriginal lifeways would be lost were efforts not made to study and preserve them, and that government should be lobbied for a department to be formed towards these aims. Newspaper reports of this meeting paternalistically termed its aims as 'Care of the Natives' and 'Helping the Aborigines' (Age 9 February 1934; Argus 9 February 1934).

Apart from engagement in museum and ASV affairs, Dermot had been busy on his own, travelling Australia and photographing Aboriginal rock art and archaeological sites (Casey 1932). He published unusual stone implements from Australia and New Guinea (Casey 1934) and again in 1934 Dermot Casey visited and recorded Aboriginal mounds in Sunbury Victoria that had not been previously identified as culturally made or as associated with Aboriginal ritual (Age 8 June 1934; Argus 18 April 1934). It is a demonstration of Casey's capacity that in 1936 he recognised the unique nature of pottery from Watom Island, Guinea (now Papua New Guinea) (Casey 1936). More than twenty years later this pottery type was to be termed 'Lapita' and interpreted as evidence of a previously unrecognised Pacific Islands culture.

The subsequent centrality of Dermot Casey to the first organised attempts at survey of Aboriginal archaeology in Victoria comes to light in a somewhat puzzling manner, but a manner which may bear relevance to the nature of Dermot Casey's later work with John Mulvaney. A newspaper report in the *Argus* dated to 8 May 1934, is titled *Items of Interest, Study of Aboriginal Relics*. It depicts that a two year survey and recording of Aboriginal occupation in Victoria had commenced under Dermot Casey, with a budget of £500. A subsequent article by the *Argus* dated 17 May 1934 is titled *Archaeological Research Offer to Melbourne Ethnologist* and states that on the recommendation of Professor F. Wood-Jones, the University of Melbourne had offered Dermot Casey £500 for a two-year program of archaeological work. It goes on to establish Dermot's archaeological credentials through mention of his association with Mortimer Wheeler. As sketched above, Dermot Casey was patently financially independent. The sum of £500, converted to current values (2021) is approximately \$52,000 Australian Dollars (Reserve Bank of Australia 2022). It seems unlikely that £250 or the equivalent of \$26,000 per annum would have represented a meaningful difference to Dermot Casey's ability to research and survey Victoria given his independent very considerable wealth. The issue of a university grant may however have been desirable to bring attention to the worthwhile nature of research into Australian Aboriginal archaeology, a discipline which was at the time barely in existence. Highlighting the worthiness of such study was also a central aim of the Anthropological Society of Victoria with which Dermot Casey was closely involved.

Possibly, insight lies in the detail of the second *Argus* article (17 May 1934) in which it is made very clear that the funding of £500 was from the University of Melbourne, at the recommendation of an eminent and senior British anthropologist (Wood-Jones) and to a qualified recipient (Dermot Casey). I would propose that the *Argus'* portrayed independence of these main actors and insistence on transparent process likely bore little resemblance to reality. Bearing in mind that Wood-Jones and Dermot Casey were fellow board members of the ASV, and that Dermot Casey had already taught for the Melbourne University backed WEA, it is highly likely that all actors here were long familiar to each other and, I propose, possibly something altogether different may have transpired to the process described in the *Argus*. It is worth considering (although no evidence has been recovered my me to date either way), whether Dermot Casey was not in fact himself the source of the £500 - an amount possibly strategically chosen as large enough to denote a serious commitment (by the University of Melbourne) to a discipline, yet not extravagant.

Despite the concern raised by Dermot Casey for preservation of the Aboriginal archaeological record, I have not found any evidence for his engagement with Aboriginal persons or communities at the time. Such engagement might have been extraordinary given the prejudices and perceptions of Aboriginality then prevalent.

Dermot Casey's intellectual work did enjoy some recognition. In 1938 he delivered an overview paper entitled 'The present state of our knowledge of the archaeology of Australia' to the Congress of the Prehistorians of the Far East in Singapore (Casey 1940). In this paper Casey displayed a depth of knowledge across the gamut of Australian anthropology and archaeology, and in keeping with thinking of the time he proposed that Aboriginal people had only inhabited Australia for a matter of centuries. In a letter to Dermot Casey dated 3 April 1949, N.W.G. Macintosh (1907-1977, the Professor of Anatomy at the University of Sydney) praised Casey's paper as an excellent review that still held good 11 years later (Casey Archive FR: A.a1).

With the outbreak of the Second World War, Dermot Casey enrolled and was appointed to the position of brigade major in the Royal Australian Artillery, Third Division. During this time, he nevertheless managed to maintain ethnological and archaeological activity, particularly while posted to New Guinea in 1944 (Spriggs 2021, p.4). His Artillery career however, ended in 1944 through the combined machinations of Mortimer Wheeler and Richard Gardiner Casey:

In 1944 Dermot Casey's one-time mentor Mortimer Wheeler was appointed Director-General of the Archaeological Survey of India. Wheeler sought to establish a six-month field school to train local Indian archaeologists. Wheeler had difficulty attracting his first choices of archaeological assistant teachers from Britain (Hawkes 1982). Richard Gardiner Casey junior had recently been appointed Governor of Bengal, serving in this position between 1944 and 1946. Mortimer Wheeler recollected connecting his memory of Dermot Casey as an expert excavator, with the local presence of his brother Richard Gardiner Casey junior in a position of authority. Wheeler approached Richard Gardiner Casey junior with the request as to whether it would be an imposition to have Dermot Casey abstracted from the Australian Army. Richard Gardiner Casey junior telegraphed the Indian Viceroy, Lord Wavell, who responded in favour immediately. Richard Gardiner Casey junior then telegraphed the Australian Prime Minister, John Curtin. This result of this chain of events

was that “a slightly puzzled Dermot Casey arrived at Simla with a small hand-bag after a precipitate journey in a bomber across five thousand miles of Indian Ocean” (Wheeler 1955, p.193).

Dermot Casey took the role of Wheeler’s assistant at the field school in Taxila (now Pakistan). We must take into account that travel between continents during the later parts of the Second World War was still a fraught undertaking, that six months life in Taxila in the remote far north of India (now Pakistan) would hardly have been comfortable by standards Dermot was accustomed to, and that as a 47 year old father of two, Dermot is unlikely to have been motivated to teach at Taxila by a spirit of youthful adventure. The Taxila field school was an exercise in duty which eventually lasted for nearly a year. Despite the likely hardships endured by Dermot, he was described by Mortimer Wheeler as having well satisfied the goals of the training institute (Wheeler 1955, p.197). This episode, together with the formation of the WEA course by Dermot seem to illustrate that teaching archaeology was a central concern to Dermot Casey. Casey left India shortly after the end of the war in Europe and returned to Australia and his work at the Museum of Victoria. Additionally, Casey held the prestigious position of president of the Royal Society of Victoria from 1947-1948 (Royal Society of Victoria nd).

Over the years to 1956, Casey appears to have been continually active in survey and recording of Aboriginal sites including leading student field trips with a notable expedition to Flinders Island (Age 20 July 1946; Grimwade 1951; Hobart Mercury 15 January 1946). Publication does not seem to have been a prominent priority for Dermot Casey – with the exception of an ongoing output of generally small single site or artefact type reports (Casey 1934; Casey 1936; Casey 1937; Casey 1938; Casey 1956). Over time, as indicated by multiple later items in the Casey archives (Casey Archives FR: A.f. Correspondence B. Edwards, 1964; N. Tindale 1974) Casey became a widely consulted authority on Australian stone tools, their typology, analysis and functional understanding. Dermot Casey’s professional-standard photographic skills are well displayed in images that he took for a work by his sister-in-law Maie Casey on the early architecture of Melbourne (Casey, 1953).

In summary then, we have in Richard Gardiner Casey senior a self-made man, taciturn, emotionally remote and authoritarian who was likely well aware that his low level of education, jackerooing and bush years had left him with little in the way of the level of

social finesse current among his social peers in Melbourne. Rather we see an individual who valued achievement for its own sake, perhaps as a way of compensating for real or perceived differences in social standing. His namesake, Richard Gardiner Casey junior appears to have borne the brunt of this projected ambition to the extent that for years Richard Gardiner Casey senior insisted that his eldest son maintain a constant flow of updates and a diary to be inspected by his father on a daily basis, with not infrequent criticism and excoriation for perceived minor infringements (Hudson, 1986, p.27). Richard Gardiner indeed went on to achieve phenomenal success within conventional frameworks of achievement, he stepped directly into his father's senior business role and achieved honour including knighthood and a term as Australian Governor General.

The question arising from this however, is where in this dynamic between father and sons do we place Dermot Casey? Perhaps the clearest indication regarding this comes from a surprising locus. In 1966 Richard Gardiner Casey junior published *Australian Father and Son* (Casey 1966). As the title indicates the work depicts the relationship between Richard Gardiner Casey senior and Richard Gardiner Casey junior – nevertheless one would expect some mention of Richard Gardiner Casey junior's only brother, Dermot. I have not identified any direct reference to Dermot (by name) in the work. He is at most indirectly referred to once or twice. It seems clear from the work's title and content that Richard Gardiner Casey junior was the inheritor of his father's role. Evelyn, their mother, is also an almost invisible figure, as if her youth and flamboyance were extinguished beneath this patriarchal edifice of dour manly duty and achievement.

Thirty years before *Australian Father and Son* (Casey 1966) came out, Richard Gardiner Casey junior had written regarding Dermot Casey to Lord Glendyne "he has not yet happened on any job or calling that promises to interest him permanently" (Hudson 1986, p.68). Was Richard Gardiner Casey junior, with his inherent drive and work ethic implicitly criticising Dermot here for his failure to work regardless of personal interest? Or is this an instance of the elder brother who once attempted to dissuade Dermot from the Army and who visited him regularly at Eton, again exercising a protective influence? These questions are unlikely to be answered. More pertinent is the picture of Dermot Casey that emerges, both from between the thumbnail summary sketches of his father and elder brother immediately above, and content throughout this chapter.

Born seven years after his elder brother, by the time Dermot would have been three or so and conscious of family roles, he likely regarded his older brother as almost adult and in a different sphere of achievement altogether – an intermediary between his father and himself. Unusually tall and with some slight degree of speech impediment (Mulvaney 1986), it is not difficult to envisage Dermot Casey as not only retiring (as described in adult life), but also content to allow his older brother to later take the leading role in the family's business affairs. Certainly, the Casey sense of duty is evident in Dermot's enlistment in the armed forces through two World Wars, the archaeological school in Taxila, and also in his honorary activities with the National Museum of Victoria, the Anthropological Society of Victoria and the Royal Society of Victoria. But perhaps more relevant is the apparent willingness with which Dermot took initiative in creating and delivering educational courses for the Workers Educational Association, and possibly funding research by the University of Melbourne. I would propose here it is plausible to see in Dermot Casey an individual who having himself been mentored and fostered by individuals including R.E.M. Wheeler and Richard Gardiner Casey junior, was in turn satisfied to take a background mentoring role towards others. Be they members of the Workers Educational Association, or as will be explored in the next chapter, one of the first 'professional' archaeologists in Australia, John Mulvaney.

Chapter 4: Dermot Casey and John Mulvaney

4.1 BIOGRAPHICAL INSIGHTS TO THEIR COLLABORATION

In the previous chapter I have outlined a fairly intimate sketch of Dermot Casey as an individual born to wealth and privilege, yet on whom a family and class ethos of duty and contribution to society was strongly brought to bear. Dermot Casey enjoyed considerable standing within the social, and financial elites of Australia having inherited considerable wealth and married into one of the leading squattocracy families of Victoria. Presented to the court of George V in 1929 (Kalgoorlie Miner, 1929), Dermot and his family were noteworthy topics of newspaper cover in their frequent travels e.g. to England and Europe (Telegraph (Brisbane), 1928), (Pahran Telegraph, 1929), and within Australia e.g. (Courier Mail (Brisbane), 1936), (Cairns Post, 1936), (Townsville Daily Bulletin, 1936). As the younger son of an almost overbearingly taciturn father, Dermot was likely shielded from his father's demands for achievement by the presence of his older brother Richard. This was a fortunate situation, as from what I have outlined, although Dermot was extremely competent in technical matters, he was a retiring individual with apparently a slight speech impediment. But all of this relates to Dermot Casey as a private individual, and I now consider his standing as an archaeologist and ethnologist by turning attention to framing the manner in which John Mulvaney first came to meet Dermot Casey.

In 1956 when John Mulvaney and Dermot Casey came into contact (apparently for the first time), Dermot Casey was a 59-year-old senior figure in the Victorian museum world with quite some standing in world and Australian archaeology. He had excavated with R. Wheeler in England, he had excavated and published independently on behalf of Wheeler in England too and had run a year-long archaeological field school for Wheeler in Bengal. He was the longstanding honorary ethnologist at Museum Victoria and was president of the Royal Society of Victoria between 1949-1950.

For a number of years prior to meeting Dermot Casey, and from the commencement of Mulvaney's employment at the University of Melbourne in 1953, Mulvaney had agitated for a greater place at the university for the subjects of archaeology and 'prehistory'.¹⁰

Mulvaney had some success in this campaign, gaining some support from the head of school, R. M. Crawford. In 1956 John Mulvaney had cobbled together barely sufficient funding (£200 from the University of Melbourne), staff and equipment to begin excavations at a rock shelter overlooking the Murray River at Fromm's Landing South Australia. These excavations would subsequently be widely regarded as the dawn of the modern era of Australian archaeology (Griffiths 2017).

In a retrospective piece, Mulvaney depicts both the cash strapped nature of the excavation, and the hesitance with which he permitted Dermot Casey to participate following Dermot's telephone contact requesting to volunteer. Mulvaney recalled Dermot Casey's request as having been made in 'a courteous voice with a slight stammer, requesting permission to visit the site' (Mulvaney 1986). Mulvaney only permitted Casey to join the excavation in its second half. Mulvaney gave his reasoning for this delay as that he had not previously met Casey and was concerned that such an elevated member of society, brother of Richard Casey junior, may not mix readily with the egalitarian dig staff (Mulvaney 1986).

4.2 SOCIAL DIFFERENCES

Matthew Spriggs (2021, p.9) proposes that this cautious attitude by Mulvaney towards Dermot Casey represented 'a sort of reverse class snobbery'. This is likely unfairly harsh towards Mulvaney and fails to take into account the very real class divisions that pervaded in Australian society at the time (Connell & Irving 1980). These divisions were based on a spectrum of factors, not least financial, but in particular they included differentiation based on ethnic and religious heritage – and I speculate that given his personal ethnic background, Mulvaney might have felt some personal unease at exposing himself to class

¹⁰ 'Prehistory' is a term that is not without denigratory implications towards the First Nation subjects of its study. It connotes that these peoples had no history due to their lack of written records. The term was frequently used including in titles of university departments, courses and degrees up until relatively recently, but is now largely fading from use. In Australia it was considered that prehistory ended with the arrival of the First Fleet in 1788, from which point history commenced.

judgement by Dermot Casey. For all that Irish Catholic and Jewish Australians have now achieved a greater degree of acceptance across the spectrum of Australian society than previously, this was certainly not always the case and both ethnic groups were once the subjects of significant discrimination and bias in Australia (Malcolm & Hall 2019) (Rutland 2005). Mulvaney's immediate heritage was a mix of both Irish and Jewish ethnicities.

Mulvaney's father Richard, was an Irish ex-seminarian fundamentalist Catholic, with a virulently anti-British brother (Mulvaney 2011, p.30). Despite his Catholic faith, Mulvaney's father had married a Jewish woman outside the church. Mulvaney's mother Frances Siegenberg was the daughter of a Jewish tobacconist who had died young. After her father's death, Frances was brought up by her maternal grandmother - a successful Jewish restaurateur in inner city Melbourne. Had one tried in Victoria of the time, it might have been difficult to find someone whose circumstances of birth, heritage, social status and acceptability to the Anglo-Australian elite differed more from Casey, yet with whom Casey would interact frequently on an apparent equal footing, than Derek John Mulvaney.

This contrast only becomes more apparent with the following brief outline of Mulvaney's upbringing, education and marriage - bearing in mind the contrast to Dermot Casey's experiences outlined in the previous chapter. Mulvaney's father eked a living as a teacher in a succession of small country towns, the heat and remoteness of some of which Mulvaney felt contributed directly to his mother's longstanding illness and early death at the age of 59 (Mulvaney 2011 pp.3, 12). Mulvaney apparently grew up in a family largely devoid of regular wider family or social interactions. His parents' marriage across religious lines (his mother only converted to Catholicism later in life) strained relations between his father and his father's family as well as with the Catholic Church. Mulvaney's mother's friends seem to have distanced themselves from her too – Mulvaney recalls only few occasions on which his mother's friends visited her and noted how different and difficult this must have been for a woman brought up in a highly sociable environment (Mulvaney 2011). In terms of the social stratification of the day, the Mulvaney's would have been quite outside the range of social interaction of the Casey family.

Mulvaney's father sacrificed greatly for young John's education, moving the family often long distances each time John graduated from the locally available schooling. Failing and taking no further maths from year nine, Mulvaney was by his own admission 'totally

unmechanically minded' (Mulvaney 2011, p.22) - an admission which takes on a greater significance when one later examines Mulvaney's excavation skills and management. In contrast the young John Mulvaney excelled at English, history and geography. Completing his schooling at the end of year ten, Mulvaney took up a teaching traineeship, with ambitions to become a secondary school teacher. After two years of traineeship in an apprenticeship model with little instruction, the so far uneventful course of Mulvaney's life was changed irrevocably through his call-up to the Royal Australian Air Force (RAAF) towards the end of the Second World War in November 1943 (Mulvaney 2011, p.20). While training in England as a navigator a chance billeting at Cambridge resulted in a cycling trip past the Rolleston's Stones - an ancient barrow or burial place in Gloucester. This generally sparked Mulvaney's interest in physical evidence of the ancient world (Mulvaney 2011, p.34). Exciting for Mulvaney as this period may have been, it was hardly a wild whirl or period of dashing adventure compared to Dermot Casey's experience in the First World War. The war ended before Mulvaney saw active service, and even off the field where young servicemen may have been expected to roam wild, Mulvaney's restraint and sobriety was remarkable. He claimed that during his postings in Sydney and Canada 'neither there nor in Canada did I ever enter a pub' (Mulvaney 2011, p. 23).

Back in Melbourne as a returned serviceman and now as a result of his service enjoying preferential access to university, Mulvaney studied history at the University of Melbourne and then took up a tutorship while studying at postgraduate level with an increasing interest in archaeology, still at the University of Melbourne and living in the same Catholic college for six years. Through this time Mulvaney appears to have engaged little with practical archaeology other than on annual and fairly desultorily described university trips to Phillip Island (Mulvaney 2011, p.39). Mulvaney distinguished himself in written work sufficiently to gain a postgraduate scholarship at the new Australian National University (ANU). Leveraging the fact that no Australian university at the time could provide him the training that he wanted in archaeology, he combined the ANU scholarship that he had been offered together with a Mannix scholarship for Catholic students in order to study undergraduate archaeology at Cambridge in 1951. During this time at Cambridge Mulvaney took part in excavations in Libya although his participation was limited to several weeks due to illness (Mulvaney 2011, p.80-85). He apparently ran a small student dig in Australia during 1952, and in 1953 was involved in excavations of a megalithic site in Tara, Ireland and also a Danish Neolithic site. This experience was limited by his self-admitted preference

for digging to the extent that he 'got no experience in surveying techniques which I regretted when directing digs in Australia' (Mulvaney 2011, p.88).

In 1953 and towards the end of his studies in England, Mulvaney met his wife Jean, an extraordinary individual of seemingly very different, adventurous character to him. Jean had recently completed a bicycle trip around Australia with a friend and met Mulvaney while she was in the UK to represent the Victorian Girl Guides at the coronation of Elizabeth II. Later that year they married and returned to Australia where John took up a position teaching history at the University of Melbourne. Their marriage celebration was a minimal affair, and John Mulvaney claims the couple 'were penniless upon our marriage. My worldly possessions amounted to a few books and a bookcase; Jean owned a chiffonier.' (Mulvaney 2011, p.93). This scenario stands in distinct contrast to that of Dermot and Gwynedd Casey on the occasion of their marriage.

One can also imagine effect that the reputation, height and likely patrician bearing of Dermot Casey may have had on those first meeting him. In particular one can understand the trepidation on meeting Dermot Casey held by young Derek John Mulvaney (1925-2016) in 1956. Mulvaney was at the start of his career, was 28 years younger and from quite a different social world than Dermot Casey.

4.3 A SUCCESSFUL START

Despite his concerns – and despite the deep social and class differences that I have described between Mulvaney and Casey, Mulvaney was explicit that his fears regarding Casey proved unfounded, and that Casey proved to be an invaluable and integral part of the Fromm's Landing excavation and of many subsequent field expeditions. In his obituary for Dermot Casey, Mulvaney describes Casey as possessed of 'boyish enthusiasm, unbounded energy and good humour' (Mulvaney 1977) and as 'a large, cheerful and hearty person, skilled in photography and surveying, who communicated freely with everyone...His sage advice and lavish provision of equipment, ranging from his four-wheel drive vehicle and trailer, a canoe and cameras, to Heath Robinson impedimenta proved essential during times of meagre finance' (Mulvaney 1977). Indeed, the cost of equipment that Casey is described by Mulvaney as having provided seems far in excess of the university funding provided to the excavation.

The likely trepidation experienced by Mulvaney in meeting Casey and in introducing Casey to the Fromm's excavation or 'dig' crew, provides one window into the world within which early academic archaeology was being practiced in Australia. A further such window is offered by the observation that it seems curious that a lecturer in archaeology at the University of Melbourne, particularly one with an interest in Australian archaeology, would not have previously encountered Dermot Casey in his role as the ethnologist of Museum Victoria, founding member of the Anthropological Society of Victoria president of the Royal Society of Victoria and widely regarded authority on Aboriginal stone tools. Further to this apparent lack of previous contact through relevant archaeological organisations it is also interesting that Mulvaney did not phrase his doubts surrounding Dermot Casey's potential to overwhelm the dig crew in terms of Dermot Casey's eminently demonstrated and published archaeological fieldwork expertise (with glowing recommendation by Wheeler in print by this time (Wheeler 1955) but solely in terms of his high-society links to Richard Casey junior.

It seems as if Dermot Casey, for all his 30 years of local and international archaeological activity may have been quite unknown as an archaeologist to John Mulvaney. As will be explored in greater detail at the end of this chapter, this disengagement likely reflects on the differentiation placed by universities of the time between archaeological work carried out within the university, and that outside it. For all his experience and publications, board positions and honorary expert capacity, to the best of my knowledge, apart from his voluntary association with the University of Melbourne in lecturing to the Workers Educational Association of Victoria, and some field survey for the University, Dermot Casey held no positions at any university and may have therefore operated quite outside the ambit of an early career academic such as Mulvaney.

4.4 MULVANEY AND CASEY IN THE FIELD – OR WAS IT CASEY AND MULVANEY?

Mulvaney's apprehension proved ill-founded, and Casey went on to accompany Mulvaney on many of Mulvaney's fieldwork across Australia (Mulvaney 2011, p.100). By shortly after their first collaboration Casey seems to also have allayed Mulvaney's trepidation around his connections with Richard Casey junior. In 1959 Richard Casey junior agreed to carry carbon samples from Fromms Landing when he next flew to the United States on government business, and he was able to arrange for Hallam Movius, Professor of Archaeology at

Harvard University, to carry out C14 dating on them – for free (Mulvaney 2011). At the time the 5,000 BP date recovered was among the earliest dates obtained for Aboriginal occupation in Australia. Beyond the few but revealing sentences by Mulvaney on the scale of Casey's practical and financial contribution, and the allusion to his provision of expertise such as surveying which Mulvaney honestly depicts himself as lacking (Mulvaney 2011, p.88), little other explicit recognition of Casey is found by Mulvaney. This does not cohere with the image gained so far of the huge contribution made by Casey, as is more explicitly stated in the writings of historian Greg Denning who like Mulvaney was a product of Catholic education and a Melbourne University history graduate. In his recollections of Fromms Landing and subsequent digs Denning recalled: 'Dermot Casey, wise, practical, a man of the land, is there. His sponsorship made much of the archaeological revolution happen' (Denning 1997, p.88).

Sponsorship is not a term I have found used by Mulvaney regarding Casey's contribution. Perhaps there was not sponsorship in cash terms, but the provision by Casey of time, expertise and effectively all equipment required to run a dig (as described by Mulvaney above) - all these would have constituted major quantitative subsidy in kind. Beyond this, quantitative subsidy there was an outstanding level of qualitative contribution too, that gave Mulvaney advantages likely beyond the reach of any other archaeologist in Australia at the time. This is the manner in which Dermot made available to Mulvaney services such as the international delivery of carbon samples by his brother Richard for free analysis by the pre-eminent anthropologist Hallam Movius at Harvard.

A full five years were to expire after the first season at Fromms Landing before Mulvaney sent himself to the University of London in 1961 to undertake fieldwork training to remedy the deficits he observed in his own practical capacity (Mulvaney 2011, p.107). One is left to wonder at the dynamics of task delegation and authority in the field at sites such as Fromms Landing. While Mulvaney was notionally in charge, it is likely that Dermot Casey would have taken the lead in surveying, photography, and likely most practical aspects of excavation. Mulvaney hints at this in his obituary for Dermot, who he stated had 'insisted on a tidy site' (Mulvaney 1977). Insistence on site tidiness is not generally the remit of anyone below the status of site or excavation director. Given the ongoing and cordial nature of the working relationship that seems to have endured over more than two decades between Casey and Mulvaney it is probable that this disparity in skills was

managed through an allocation of tasks and hierarchy which saw Mulvaney control overall direction, while consulting with and giving leeway to possibly the effective director of many activities - Dermot Casey.

Over the years, Casey seems to have remained Mulvaney's authority on surveying and practical aspects of excavation. When in 1968 John Mulvaney assembled the first edition of *Australian archaeology: a guide to field techniques* (Mulvaney (ed)1968), the first chapter following Mulvaney's introduction was a lengthy chapter by Dermot Casey titled *Elementary surveying for Australian archaeologists* (Casey 1968). Casey was an accomplished photographer, yet he seems to have relinquished the opportunity to write the next chapter in the field guide (on photography) in favour of someone he had himself coached in the subject – Robert Edwards (Edwards 1968). Over the years Dermot Casey had fostered a relationship with and had assisted the then young archaeologist and anthropologist Robert Edwards (1930-). Edwards had not come to Casey's attention as an accomplished academic, but in Casey's own words as an 'honorary part time worker' at the Museum of South Australia (Casey Archives FR: A.f, D. Casey to K. Oakley 1964). The Casey Archives contain several highly enthusiastic letters from Edwards to Casey describing Edward's fieldwork (Casey Archives FR: B.e. 10/5/1965, 22/07/1965, 22/11/1965), Casey appears to have frequently written to Edwards, notably in 1966 and 1967 providing advice on field photography (Casey Archives FR: E.a-b 31/07/1966, FR: B.e 14/10/1967).

Yet it is Edwards, not Casey who provided Mulvaney's fieldwork guide with a chapter on photography. It is quite evident that Mulvaney's fieldwork guide is reliant to a greater extent on Casey than any other source. Casey, with Ian Crawford and Richard Wright also contributed a brief final chapter on Australian stone tools (Casey, Crawford & Wright 1968). In total, of the fieldwork guide's 113 pages (discounting the introduction), Casey directly provided 22 pages (double the length of most other contributions) and when taking into account Casey's joint paper and the lengthy paper (22 pages) of his protégé Robert Edwards, Casey's hand can be directly seen in close to half of the work's content. In this and in descriptions of Casey at Fromms Landing above, I believe we see Dermot Casey as a relatively self-effacing character, who despite his considerable wealth and social standing was not only ready to foster scholarship among those divided from him by age and class, but was ready to assist them in their development, through ushering them to the fore in his place. The example of *Australian archaeology: a guide to field techniques* (Mulvaney 1968),

provides a published instance of a working relationship between Mulvaney and Casey in which Casey provided technical expertise that contributed considerably to the publication of a work bearing solely Mulvaney's name as author.

4.5 CASEY AS MENTOR

A striking example of the archaeological relationship between Mulvaney and Casey is one piece of correspondence in which John Mulvaney sought Dermot Casey's advice, and in which Casey calmly but firmly provided explanation. It is understandable that an established academic such as Mulvaney was by the 1970's might be circumspect as to with whom they may confide uncertainties around their own work. Yet such is the nature of correspondence between Mulvaney and Casey. Mulvaney sought Casey's advice in interpretation of basic understanding of stratigraphy as is evidenced by a fascinating exchange between the two regarding C14 dating anomalies in carbon samples from Kenniff Cave. On 18 May 1971 Mulvaney wrote to Casey in some disquiet over apparent inconsistencies in these C14 dates. He was disturbed that lower stratigraphic layers had produced C14 dates younger than upper layers, and was unable to make sense of the deposit (Casey Archives FR: A.a1). Within a matter of days, on 24 May 1971, Dermot Casey replied in length, addressing Mulvaney's concerns, reassuring him that 'all is not lost', and that such inversions could be explained by a variety of factors, including a close understanding of site formation - which he proceeded to explain (Casey Archives FR: A.a1).

4.5.1 The Papers of John Mulvaney – field books

A further rich source of information on the relationship between Casey and Mulvaney in the field - and on the likely capacity of Mulvaney to operate independently in the field - can be derived from archival material, both that body of material held at AIATSIS which has been mentioned in the previous chapter, and also material held at the National Library of Australia in the archive entitled 'Papers of John Mulvaney' (Libraries Australia ID 41218067). Items in this archive are subdivided into Manuscript (MS) reference numbers, within which items are individually numbered (e.g. MS 9615 / 8.10/6) This archive also contains numerous items of correspondence between Dermot Casey and John Mulvaney. Of interest here however is that the archive contains a number of John Mulvaney's field

notebooks spanning a period of decades and including notes from high profile excavations including those at Green Gully and Lake Mungo.

It is now, and I believe has long been a firm tenet of field excavation that the notes and field notebook of the site director constitute a crucial element in site recording. They frequently play an invaluable role in reconstructing events including the order and reason for excavation, and provide preliminary understanding of results. Given excavation seasons and publication may often be separated by many years, maintaining neat legible field notebooks is an essential aspect of fieldwork. If the notebooks in the Mulvaney archive are the final copy - and not rough notes, then, in my view, they are a troublingly untidy and disordered record. These notebooks are highly evocative, they are almost invariably hard covered students note books, at times with one face in graph paper, and often containing grains of sand, smears of dirt and flattened insects. They frequently start in Mulvaney's hand, sometimes in quite tidy manner but they soon devolve into scrawl and are often then taken up by other individuals, including, for example, archaeologist Wilfred Shawcross who worked with Mulvaney at Lake Mungo. Dermot Casey's hand is notably absent. Perhaps were he functioning as site surveyor and photographer, he would probably have kept his own notebook. Notably, where other individuals have taken up recording, their notes and writing are more comprehensive and considerably more legible than Mulvaney's. The images below show the opening page of the 1965 Keilor (Green Gully) notebook (MS 9615/8.7 /1) in Mulvaney's hand (Figure 5) first in pencil then in pen (Figure 6), followed by subsequent pages neatly entered in at least four different unidentified hands (Figure 7, Figure 8).

Figure 5: Green Gully notebook - John Mulvaney's handwriting in a mixture of blue biro and blunt pencil

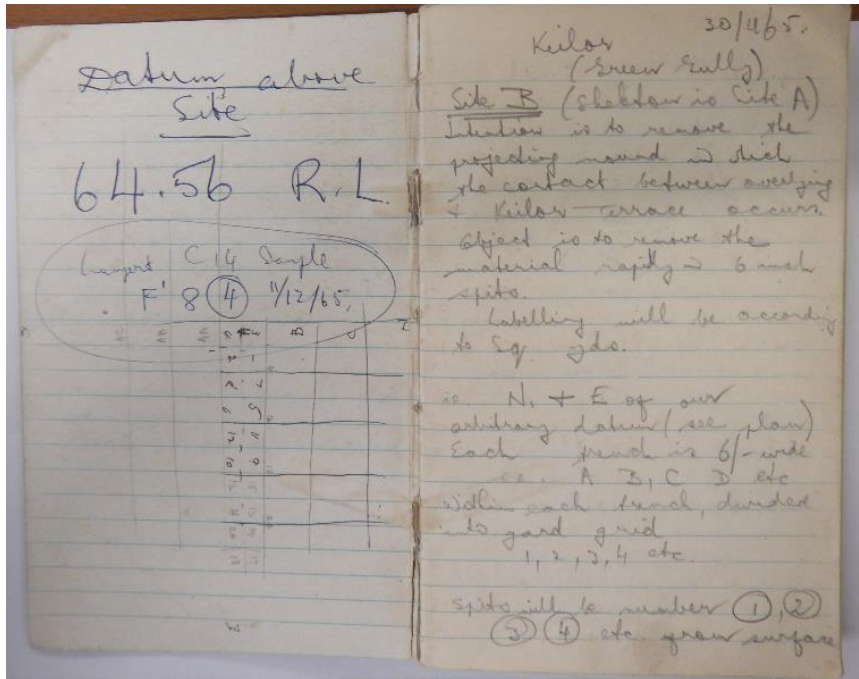


Figure 6: Green Gully notebook - John Mulvaney's handwriting in blue biro. Writing and content is considerably disordered.

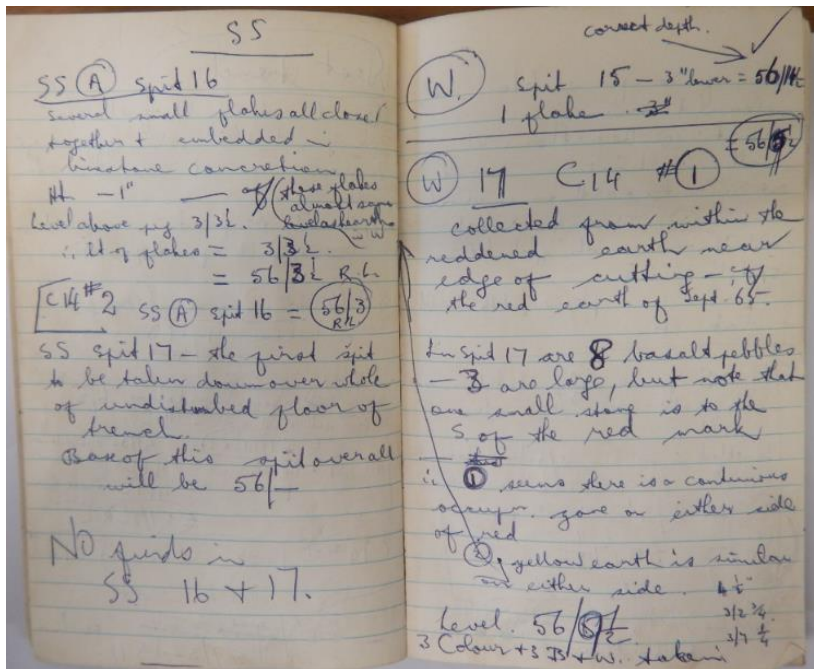


Figure 7: Green Gully notebook - unknown handwriting. Writing, contents, layout and organisation are notably more tidy and coherent than Mulvaney's examples above.

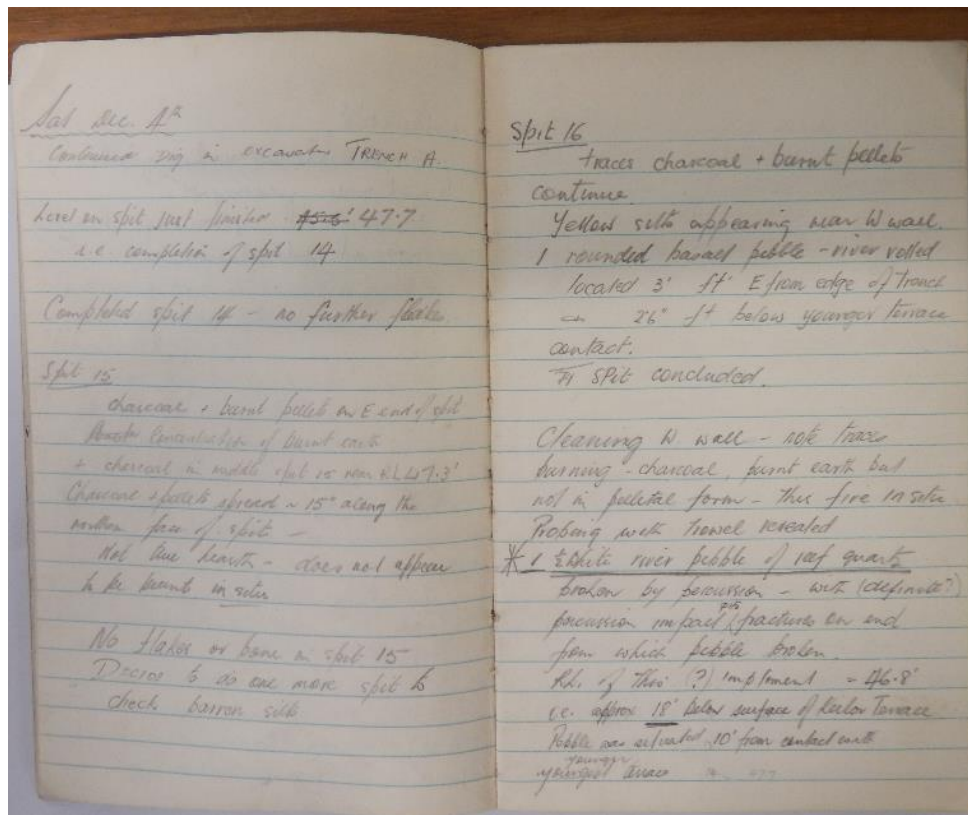
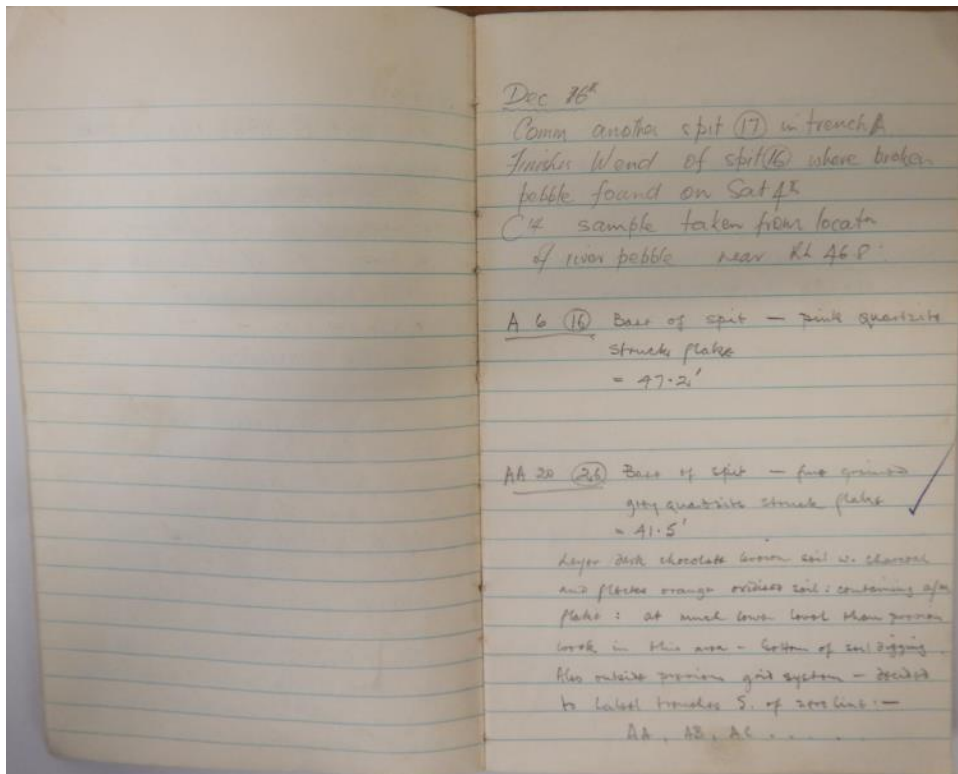


Figure 8: Green Gully notebook – unknown hands. Writing, contents, layout and organisation are notably more tidy and coherent than Mulvaney's examples above.



In these earlier notebooks from the Kenniff Cave excavations in 1960 (MS9615/8.4/2) and 1962 (MS9615/8.4/2) and from the Green Gully excavations in the mid to late 1960's (MS 9615/ 8.7 /1). Mulvaney at most lists his field staff - but there only very rare mentions in his notebooks of any on-site discussions that may have taken place between Mulvaney and other staff.

At Lake Mungo too, the 1973 season notebook has an opening page of general introduction by Mulvaney, without sketches after which pages alternate between a neat hand accompanied by technical drawings, and Mulvaney's generally broadly descriptive notes such as: "the deposit is sand, very moist. Sieving it on old sieves. Nothing in it except finely fragmented small shells – i.e. not food material. Looks very like a beach".

Figure 9: 1973 Lake Mungo notebook - Mulvaney's hand. Notes are very broad as described above

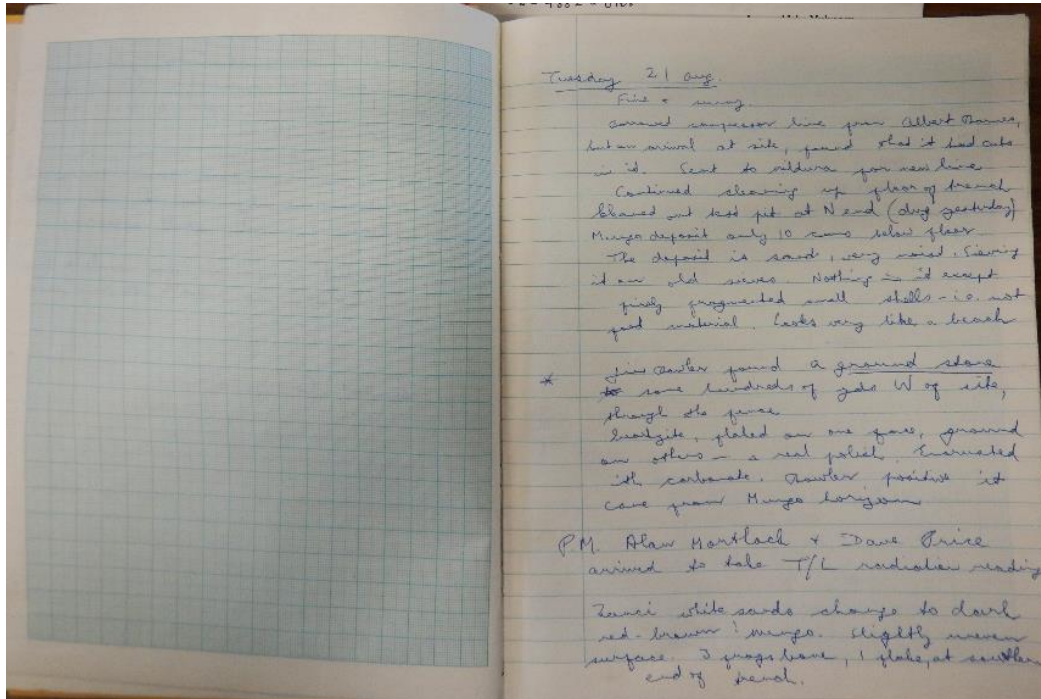
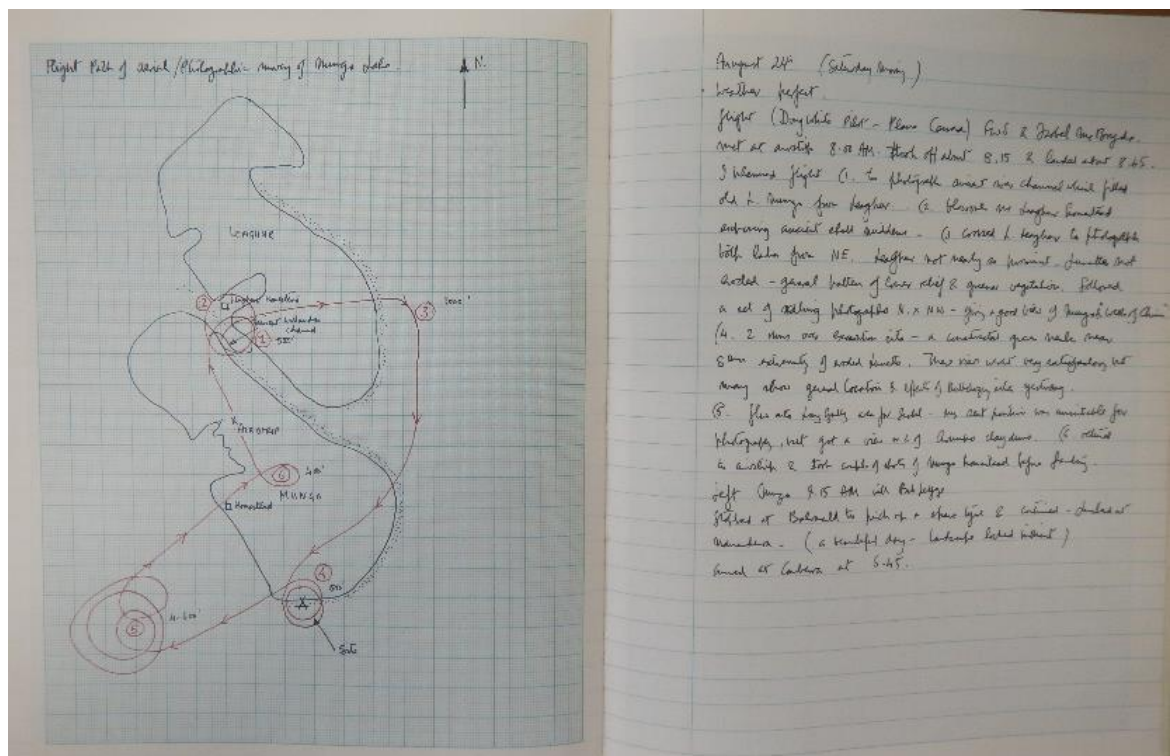


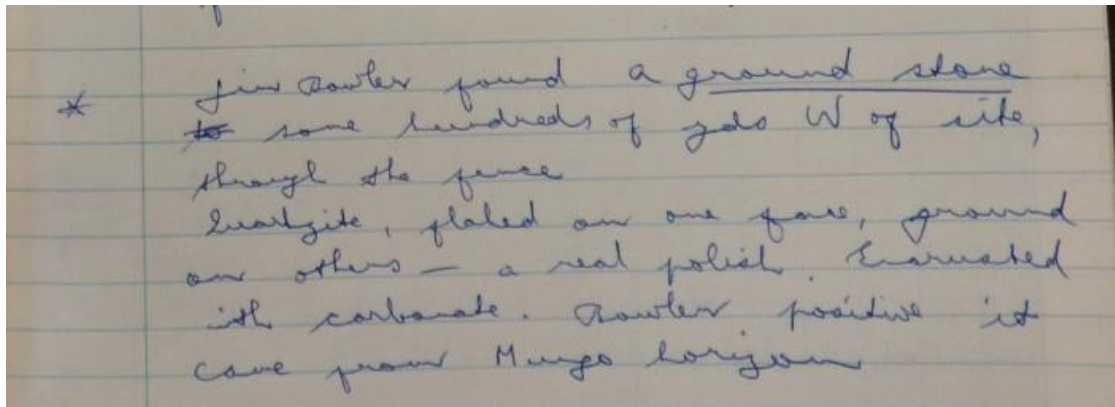
Figure 10: 1973 Lake Mungo notebook - unknown hand. Drawing is well executed for an overall site map, notes are far more technically informed than Mulvaney's



While not generally mentioning discussions with colleagues at Mungo however, John Mulvaney does mention in his notebooks his interactions with Jim Bowler, the geologist who was crucial to comprehension of the sites stratigraphy and relative dating. Mulvaney notes Bowler's interpretation, and on 21 August 1973, Mulvaney noted with some excitement that Bowler had found a ground stone axe,¹¹ which Bowler was certain had come from the Mungo horizon that corresponded with the Mungo burials. Mulvaney uncharacteristically marked the entry with an asterisk and underlined the find:

¹¹ Stone axe heads that had been formed through the grinding and polishing of a rough blank were often hafted and used for purposes including removal of bark for the construction of shelters. Beyond their practical functions they also almost certainly played a status role and possessed valued qualities that were based on the origin and appearance of the stone. Stone from certain locations such as Mount William in Victoria is known to have been traded over distances of many hundreds of kilometres. Isabel McBryde, mentioned later in this chapter played a central role in documenting not only the large exchange network of Mount William axe heads, but also in doing so demonstrated the extent of formal Aboriginal distribution networks that once existed.

Figure 11: Mungo notebook 21/8/1973 - Mulvaney's hand



Through Mulvaney's Mungo notes it is obvious that Jim Bowler was the prime operator, with Mulvaney passively noting his understanding of how Bowler was implementing machine excavation to reduce sand levels to those strata that were of interest. Mulvaney's notes are almost without exception less organised, insightful and methodical than those of the other writers whose handwriting appears in his notebooks. Not only in regard to observation and noting of fieldwork, but as has been noted above regarding C14 dating and as will be noted below regarding Lake Mungo, Mulvaney seems to have been challenged in interpreting archaeological data in the field.

4.6 CASEY AS A STEADY HAND

Unfortunately for Mulvaney's excitement at the potential find by Jim Bowler of a stratigraphically embedded stone axe, Dermot Casey visited the excavations by Mulvaney at Lake Mungo in 1973. Casey was sceptical regarding the stratigraphic depth of the axe in his letter to John Mulvaney, dated 30 August 1973 (MS 9615/8.10/2):

Dear John,

Thank you for having me at your place as they say on the radio...as to the 'ground' axe, myself, I think that you would be wise to think twice before you commit yourself to this. It seems to me that it can only be accepted as being on the surface...one of the rules of the game is not to count anything as being in a layer unless it is well below the surface.

Dermot Casey's language here is cordial but certain, potentially seeking to defuse any perception of a patronising attitude through use of the term 'rules of the game' rather than

stipulating principles of stratigraphic deposition or making reference to relevant texts on the topic. We also see probable evidence of a likely divide between the cultural world of Dermot Casey and the university archaeology students who made up the 'egalitarian workplace' at Fromms's Landing and who were the cause of Mulvaney's original concern at engaging Dermot Casey to participate there. Dermot Casey notes the term 'at your place' as a novelty phrase 'as they say on the radio'. The apparent lack of intimate contact by Dermot Casey with common vernacular might be a further indication of just how far removed he was from the world of Mulvaney's staff at Fromm's Landing.

This willingness by Casey to caution Mulvaney was not restricted to site interpretation, but extended to authorship and editing of papers for academic publishing. Yet even where Casey played an important role behind the scenes, his place remained secondary to Mulvaney. This is apparent in the write-up of the Green Gully Burial, in which the authors were listed in the following order: J.M. Bowler, D.J. Mulvaney Australian National University. D.A. Casey, T. A. Darragh, National Museum of Victoria (Bowler, Mulvaney, Casey, & Darragh 1967).

In a letter dated 31 December 1966 in which Dermot Casey provides edits to the text of this article (Bowler, Mulvaney, Casey, & Darragh 1967), Casey was firm but definite in reversing changes made by John Mulvaney: (AIATSIS MS 1325):

Dear John, I have trimmed the Casey / Darragh screed much as you suggested. Apart from changing words here and there – of no real consequence – I have not entirely followed your editing, and I have put back some of the things that you scratched out, and altered the wording of some of your amendments.

Here too, Casey is politely firm in insisting on his changes, even though the recipient of his remarks had by that time been a prominent academic for over 20 years.

It would not seem to generally be the case that Mulvaney was an individual who readily accepted criticism or correction. In the two archives examined, that at AIATSIS and the National Library of Australia, the only individual from whom he seems to accept regular correction and caution is Dermot Casey. A marked contrast to this acceptance of correction is the following episode in which Mulvaney quite evidently felt that his prestige as a senior archaeologist had been impinged upon by government officials of lesser expertise.

4.7 MULVANEY AND THE NSW NATIONAL PARKS AND WILDLIFE SERVICE

In 1974 after a season at Lake Mungo in which Mulvaney and Jim Bowler had used a bulldozer to reduce soils to the strata of interest to them, Mulvaney had written a preliminary letter to the NSW National Parks and Wildlife Service (NPWS) to obtain a permit to carry out further works for a follow-up excavation season. It seems from Mulvaney's response to queries for clarification by NPWS, that his application had been somewhat cursory and did not include details such as the proposed location and depth of the proposed excavation. In a letter to Mr D.A. Holmes of the NPWS dated 26 February 1974, John Mulvaney responded sharply to NPWS requests for clarification. In the following excerpt it would seem to me that Mulvaney perceived NPWS oversight as an affront to his personal status as a professional of repute.

I regret that the specific points arise from a misreading of my initial report. I find it very disappointing to say the least, that my comments were subjected to such detailed scrutiny, even before I applied for a renewal of permit. I suggest that this is not the best way to gain the confidence of field workers. Certainly I will write future reports with greater circumspection.... My point is, therefore, that if an archaeologist is a professional (and other professionals of repute are associated in the project), the permit to excavate should assume that he is responsible, and not delimit his scope of action too finely. In cases such as the above (or in a cremation burial erosion) it is surely better to excavate at once and obtain 'permission' after rather than to risk its destruction. (MS 9615/8.10/2).

The background to this exchange needs to be set somewhat – the *NSW National Parks and Wildlife Service Act 1969* had been subject to review with widespread changes implemented in the subsequent *NSW National Parks and Wildlife Service Act 1974*. These changes included new authority vested in the government to oversee and require permits for excavation from individuals and organisations who had previously been free to excavate almost at will (Organ 1994).

From the perspective of NPWS it would be hard not to notice the almost certain ironic use by Mulvaney of quotation marks around the word 'permission'. It would similarly be moot from the perspective of NPWS to consider whether the likely destructive nature of archaeologists using a bulldozer (for the first time in Australian archaeology), without

specified locations and depths of excavation would have constituted a more destructive process to archaeology than it would constitute part of a claimed capacity to save archaeology.

4.8 THE ABSENCE OF ABORIGINAL VOICE

A perspective that is quite absent from correspondence between Casey and Mulvaney, or in any of Mulvaney's field note books, is the attitude of Aboriginal people to the excavation of their past in the form of stone tools - but at Mungo, in the form of ancestral remains. When interviewed for the film *Message From Mungo* (Pike & McGrath 2014) Mulvaney claimed his lack of engagement with Aboriginal people was due to the lack of any Aboriginal people in the surrounding area. The 'Papers of John Mulvaney' archive suggest that casual racism on Mulvaney's part would almost certainly have contributed to this lack of engagement.

Casual racism has been defined by the Australian Human Rights Commission (AHRC) as: "not necessarily a deliberate act of discrimination, and casual racism often isn't meant to cause offence or harm..." but is an act which nevertheless has an impact on its recipient (AHRC, 2021). In Mulvaney's 1963 fieldnotes from Ingalanddi and Kintare (MS9615/8.5/5), Mulvaney makes reference on 31/ 7/ 1963 to a 'half-caste'- a term which was likely to have been at the end of its acceptable use period then. On the same page, Mulvaney makes reference to information 'reported by the old xxx¹²' - a three-letter term which would not have been acceptable to the individual being referred to nor to academic society or even polite society at the time.

To give some context to social attitudes towards the term over time, an extensive search of newspaper archives at the National Library of Australia online Trove reveals a significant phasing out of use of the term xxx through the mid twentieth century, showing that the term xxx was used in Australian newspapers 6,000 times between 1930 to 1939, 4,000 times between 1940 to 1949, and 2,000 times between 1950 to 1959. Between 1960 and

¹² The three letter term which has not been reproduced here is now unacceptable in polite usage, but for many years was used as a denigratory term towards Aboriginal people

1969 when we have evidence of Mulvaney not only using it but committing it to writing, Australian newspapers only used the term 281 times, among which a fair proportion of its use was by authors decrying how unacceptable a term it was. In the previous chapter I had hypothesised as to whether the experience of the Casey family had brought them into conflict with Aboriginal people, or resulted in negative attitudes towards them. There is still insufficient evidence to hand to reach a position on this. However it is obvious that John Mulvaney felt comfortable in using the term 'xxx' in correspondence with Dermot Casey, when on 29 / 8 1965 (A. a1) Mulvaney wrote to inform Casey that " I have xxx. Institute Executive & Council meetings" (in his position as a council member of the Australian Institute of Aboriginal Studies).

4.9 PUBLICATION AND RETROSPECT OF FIELDWORK RESULTS

Despite Casey's ability to write to an academic standard is evidenced by his chapter in Mulvaney's field guide (Mulvaney (ed)1968), and his not insubstantial corpus of peer-reviewed articles (Casey 1931, 1934, 1936, 1937, 1938, 1940, 1957,1967,1970, 1971, 1973). Yet, Casey is notably absent as an author or an accredited contributor to the publication of several excavations to which his assistance seems to have been indispensable. Casey is absent as an author of the Fromms Landing excavations, publications of which were in the *Proceedings of the Royal Society of Victoria* (Mulvaney 1960, Mulvaney, Lawton and Twidale 1964). According to Mulvaney, this absence was at Casey's insistence, who 'refused to have his name as co-author on any of the reports, although it was very appropriate' (Mulvaney1977).

The extraordinary assistance afforded to Mulvaney by the Casey brothers, in flying Fromms Landing material to the United States, and arranging for the pre-eminent American Anthropologist Hallam Movius to carry out C14 dating on them at Harvard for free, is unmentioned in Mulvaney's publication of these landmark findings. Mulvaney's only expression of gratitude reads: 'Thanks to the assistance of Professor Hallam L. Movius, jnr., and Dr T. A. Rafter, the writer has obtained an important series of five dates for the one stratified deposit' (Mulvaney 1961).

Many years later in nostalgic reminiscence Mulvaney published some correspondence and pictures from the early stages of his Australian fieldwork career, including one photograph

(Figure 12 below) that he had taken at Fromms Landing in 1958 showing a shirtless Dermot Casey taking a 'watermelon break' along with the senior archaeological figures of Aldo Massola, Felix Raab and Norman Tindale (Mulvaney 2000). Tindale was cutting the watermelon. The impression from this photograph is clearly that Casey was considered a hard working (and unpretentious) member of the core academic team at Fromms; none of the other visible site workers are named.

Dermot Casey appears to have spent the last decade of his life in something of a retirement. Although still honourarily involved at the Museum of Victoria, he had taken up residence at his rural property *Willimi* in Mount Macedon, approximately 75km north west of the Melbourne CBD. Through this time as evidenced by the Casey archives he corresponded on familiar terms, often alluding to shared personal experiences, with some of the most prominent names of Australian and international archaeology, including a number of students embarking on their careers. Correspondents include Grahame Clarke of Peterhouse Cambridge, Isabel McBryde, Charles Dortch, Norman Tindale, Harry Allen, Anne Bickford, Peter White, and Jim Specht.

Figure 12: Figure 1 from Mulvaney (2000) showing a watermelon break at Fromm's Landing shelter 2, 1958. Left to right: Dermot Casey seated and shirtless, Aldo Massola, Felix Raab, Norman Tindale slicing melon.



4.10 NO NEED TO ACKNOWLEDGE

In July 1974, only three years before Dermot Casey's death, Isabel McBryde wrote in sincere thanks to him for assistance he had given her in arranging access to the Mt William Aboriginal quarry (Casey Archive FR: A.ai 20/7/1974.). The Mt William site was focal to McBryde's studies on the distribution of Aboriginal ground greenstone axes (McBryde 1984). McBryde wrote her thanks while quoting Dermot Casey's insistence that there was 'no need to acknowledge' him. It is in the tension and to-and-fro between larger narratives such as Casey's career and institutional positions and small interpersonal insights such as this exchange with Isabel McBryde that allow greater insight to both the broader and smaller brushstrokes of historical evidence that have been used here.

The picture painted here of Dermot Casey has provided a richer and nuanced understanding of why and how his contribution to Australian archaeology is as little recognised as it currently is. Put simply, Dermot Casey would not have had it otherwise. As a product of a social elite which valued equally self-effacement and service in direct proportion to privilege, the willing acceptance of credit was not part of Casey's worldview. In John Mulvaney, Casey found a fitting recipient for assistance. Mulvaney's position as an academic 'new man' meant that it was quite acceptable from Casey's perspective for Mulvaney to accept assistance from him. But beyond this, it was congruent with his worldview that credit for their joint efforts should be solely accorded to Mulvaney. From Dermot Casey's perspective Mulvaney was a member of the lower social orders whose advancement as part of the grander plan of Empire for social improvement was an overriding concern of the imperial vision and of its key Australian actors such as the Casey family. Mulvaney therefore provided a means by which Dermot Casey could pursue his own archaeological passion and invest great energy and means and largesse into archaeology - without having to take credit for the results of this contribution. I doubt Dermot Casey would be pleased at the recognition that has been drawn to his actions in this thesis, but I would hope he could accept it as part of an academic endeavour to provide acknowledgement.

These conclusions largely speak to the personal motivations of Dermot Casey. It is however the aim of this thesis to apply an analytical approach to biographical factors in the formation of the community of scholars in the discipline of Archaeology in Australia. As

outlined in the literature review section (Chapter 2) of this thesis considerable academic research has been carried out on the manner in which communities of scholars come into being. The seminal work by Goodman on the topic (Goodman 1964) defines critical criteria of any scholastic community. These include the observation that the community of scholars is and has been a self-regulating entity since the 12th century when the current model of western universities commenced taking shape (Goodman 1964, p.187). Bourdieu's thesis that education, particularly tertiary education serves to further and reproduce the structures of power that frame it can be applied here with an interesting clarification (Bourdieu & Passeron, 1990) that in this case it seems that the internally generated and self-referential structures of power within the university community allowed for an individual of as considerable social power as Dermot Casey, to be considered an outsider.

The manners in which I have interrogated texts and primary documents here draws on established methods of critical historical and literary analysis (Oates (ed) 1986, Russell 2009, Richetti 2006, Denzin 1989). They include a critical approach to all sources of information and most robustly to autobiographical information, a detailed cross-comparison of sources to examine coherence and correspondence, the active search for sources of any type that may support or contradict existing narratives, identification of key life events, an active weighing of the stated and unstated motivations of all actors, and the understanding of individuals and events in as wide a context as possible. With specific reference to critical biography, in addition to these techniques, there is the need to apply a perspective that allows one to see what the biographer Leon Edel metaphorically referred to as 'the figure under the carpet (Edel 1986). The figure under the carpet is the image, invisible from the carpet's face, of the knots and weave that make up the structural fabric of the carpet. In this case it seems evident that beneath the face of the carpet that was the 'father of Australian archaeology' (Mulvaney) lay a considerable lack of archaeological field experience and expertise. This was a lack considerably compensated for by collaboration with individuals who were more practically oriented such as Dermot Casey.

The notion of a community of scholars (Goodman 1964) is not one that has been explicitly noted in any of Mulvaney's writings. Yet, the distinction between those inside and those outside the university, and the emphasis on the universities' hegemony on knowledge - and the need for this hegemony to be enforced was one that Mulvaney overtly drew. Indeed, he voiced marked disquiet at the fact that none of the archaeological work done up to the

1950's in Australia had been carried out by university trained archaeologists (Mulvaney 1961). Years later in reflection on his taking the presidency of the Anthropological Society of Victoria in 1958, Mulvaney recalled having been driven by a sense of moral duty to archaeology: 'morality demanded that I become holier-than-thou... educate these amateurs' (Mulvaney 1990, p.249) The invocation of a moral imperative by Mulvaney perhaps draws attention to an understanding by him albeit possibly ill-defined that, the coalescence and formation of what he considered to be 'professional archaeology', was more a matter of drawing lines around the community of university trained scholars, and creating a cohesive 'family' as it was a matter of actually emphasising archaeological capability. For as we have seen in some detail, John Mulvaney was firmly inside the university community of scholars - but appears to have been patently less qualified in terms of real archaeological field and interpretive skills than was Dermot Casey, and likely other similar individuals who had trained and gained greater levels of experience overseas.

This retrospective piece by Mulvaney (1990) is unusual in that presumably Mulvaney includes in the class of 'amateurs', his friend and mentor Dermot Casey. But perhaps this distinction existed in Mulvaney's mind from the outset. I noted that Mulvaney had never met Dermot Casey prior to Fromm's Landing and noted that this seemed unusual given Dermot Casey's prominence in local archaeological organisations including his role as a founding member of the Anthropological Society of Victoria. Consistent with this delineation, it seems likely from this that Mulvaney solely introduced himself to the field of Australian archaeology from within the university and its community. I would propose that the ease with which Mulvaney accepted Dermot Casey's advice and assistance - including corrections and apparently fairly patronising comment at times - all this was facilitated by the fact that Mulvaney perceived Dermot Casey as an outsider to the community of archaeological scholars, or professionals as Mulvaney termed it. Being outside the community of archaeological scholars, Dermot Casey may have been a technical expert and a source of aid and advice – but he would not likely have constituted a challenge to Mulvaney's sense of expertise, in the same manner that would likely have been the case had Casey been an employed academic.

In summary, the outsider, Dermot Casey, was empowered to integrate with and contribute to Australian archaeology largely as a result of his status outside the archaeological

community of scholars and his technical expertise, which too could be considered outside university domain. Dermot Casey's cultural ethos not to accept credit for his contributions merged conveniently with his collaboration with John Mulvaney – who at the time sought to define professional Australian archaeology as that carried out within the university, by university qualified archaeologists.

Chapter 5: Alexander Gallus

5.1 . ALEXANDER GALLUS AND THE AUSTRALIAN ACADEMY

In the previous chapters I argued that Dermot Casey, an individual well accepted by Australian society, did not integrate with the community of Australian academic archaeologists. In this chapter I examine the causal factors and the manner in which a highly qualified, but non-Anglo Saxon academic archaeologist was effectively excluded from the professional community of Australian academic archaeologists. This individual is Alexander (Sandor) Gallus (1907-1996) seen below in Figure 13.¹³

¹³ There are a variety of ways that different authors have referred to Gallus, at times listing Sandor as his first or middle name. Given that Sandor is a Hungarian diminutive form of Alexander, I will use Alexander Gallus or Gallus.

Figure 13: Dr Alexander Gallus in approximately 1970. By kind permission of his son Dr Alexander Gallus (Jr)



Before embarking on a detailed historical and biographical study of Gallus and his relationship with the Australian archaeological community, there is one piece of highly evocative evidence which seems to so well encapsulate the perception of Gallus by his Australian peers, that it is worth sacrificing chronological coherence to present this item at the outset. This evidence comes not from documentary or published information, but from bodily actions by archaeologists captured on film and the subsequent manipulation of these actions by archaeologists in editing and production of this film.

Some minimal background is required to contextualise the production of this film *Flint Miners of the Nullabor* (Edwards & Wright 1967), that was shot at the location of archaeological excavations at Koonalda Cave. Koonalda Cave is a large cave on the Nullarbor Plain in South Australia. It is over a kilometer in length and is accessed via a sinkhole some 25 metres deep. The cave contains significant flint deposits which Alexander Gallus determined as having been mined and quarried by Aboriginal people. In 1956 Gallus controversially dated these Aboriginal flint mining activities at Koonalda Cave to before 20,000 BP. At the time the oldest accepted archaeological date in Australia was 8,700 BP. Gallus' dating was not the only controversial aspect of his practice in the eyes of the Australian archaeological establishment. Gallus' interpretation of stone tool morphology and also his interpretation of artwork at Koonalda Cave were also vigorously contested. In retrospect, Gallus' interpretation of dating was corroborated through radiocarbon analysis, and the unique nature of Koonalda Cave - including the presence of art as first documented by Gallus - was listed on Australia's National Heritage List in 2014 (National Heritage List 2021). At the time however, and as will be explored in greater detail later, Gallus' claims were treated with the barely polite dismissal that the professional community of Australian academic archaeologists of the time gave to most of his publications and propositions.

It was effectively to check and counter such perceived erroneous claims by Gallus, that in 1967 an expedition was sent to Koonalda by the Australian Institute of Aboriginal Studies, led by R.V.S. Wright of the University of Sydney who travelled to Koonalda Cave with a crew of specialists and student volunteers. Specialists included D. Symon who contributed a chapter to the site's publication on vegetation, R. Frank on sedimentology, A. Thorne on faunal remains and L. Maynard and R. Edwards on wall markings. No mention is made of any consultation or involvement with Traditional Aboriginal owners of the location. Two main publicly available pieces of evidence survive from this trip. The first to be examined here is the short black and white film (26 minutes) mentioned, with copies held at the University of Sydney Rare Books Collection, the Museum of South Australia, and at the Australian Institute of Aboriginal and Torres Strait Islander Studies (AIATSIS). Titled *Flintminers of the Nullabor* (1967), it was directed and filmed by Bob Edwards – the same person noted previously as having contributed the photography section to John Mulvaney's *Australian archaeology: a guide to field techniques* (Mulvaney 1968).

For a film that apparently sets out to document scientific methodology, the production is remarkably emotive. The opening scenes are of dark subterranean chambers with young archaeologists working in small pools of dim light. Throughout, atmospheric piping flute music sets an evocative sense of impending mystery and danger. The crisp British or upper-class Australian accent of Barry Pitman commences narration. Pitman was a film producer for the Australian Broadcasting Commission and a weather presenter on Channel 2 and later Channel 9. The narration first focusses on the technological challenges that the team had faced in excavation, and the various methods used to overcome these challenges. We are informed that the investigations here are into 'Primitive man' (sic) and 'Prehistoric man' (sic). There is no connection made between Aboriginal people of the time and the archaeological remains being excavated. With an up-tempo shift in musical tone, the camera pans to the ten University of Sydney student volunteers involved in sieving and sorting extracted sediments. Little or minimal clothing is noted on these volunteers within or outside the cave, as male volunteers at times work barefoot, bareheaded and in only shorts. The film now takes a bolder and wider gaze as the music track changes again to more heroic strains to accompany scenes of men abseiling down nearby South Australian cliff tops in search of similar caves and flint deposits. Then, there is a retreat in the musical setting to bucolic tones as the camera moves to apparently connected activities of female volunteers gathering plants, and then the camp eating together.

Through all this, one could be forgiven for having missed a fleeting moment, but a moment which I have to suspect was intentionally manipulated by the film's producer. Casting back to the scenes of student volunteers sieving in the Koonalda sinkhole while clad so casually in their shorts and often little else, there are a few short moments when the camera turns to the ground surrounding the top of the sinkhole and focuses on a figure that approaches slowly in the unrelenting Nullarbor sun - a figure that seems almost tragic-comic given the setting of casual dress described so far. As the camera turns, this somewhat slouched individual appears and approaches the camera wearing a formal suit and shoes and makes his way slowly to the sinkhole. No eye contact with the camera is made while the individual then turns his back to the camera to slowly take the ladderway step by step by step down to the base of the sinkhole. The camera holds its focus on this descent until all that can be seen is the balding retreating head - of Alexander Gallus. No individual team members of the excavation are named during the film - including Gallus, yet it is clear whom the

outsider is, in Gallus's perception, in that of the film producer and almost certainly all others onsite.

The specifically Australian cultural mores of academic archaeological practice are brought to the fore yet again in the films closing scenes. Alan Thorne (1939-2012), specialist in anatomy and human evolution (Curnoe 2012) is shown in a University of Sydney Laboratory, replete with white laboratory coat as he studies faunal remains from Koonalda. R.V.S. Wright, the director of the Koonalda field season, appears in a dark tie and crisply ironed white shirt, as he carries out lithic analysis, demonstrating the way he proposed a lithic tool had been shaped to fit the hand. Archaeology it appears, is safely once more in the hands of empiricists, who are doing science and measuring things with scientific instruments in an appropriately sterile-looking environment.

5.2 AN ACADEMIC BIOGRAPHY - HUNGARY

Who was Alexander Gallus and why was he presented in this film as a forlorn figure – a figure whose marginalisation by the Australian academic community will be more firmly evidenced in the sections to come? My knowledge of the early life and career of Alexander Gallus is drawn from an obituary and a retrospective piece written by Vincent Megaw - an academic working in Australia who respected Gallus and his work (Megaw 1983, 1997). A further source of information into Gallus' personal life results from generous and frank email correspondence with Professor Alexander Gallus Junior (1940-) in response to my email enquiry regarding his late father (Gallus-Jr 2021).

Alexander Gallus was born in 1907 in Sopron, Hungary – a town near the Austrian border. He obtained a Doctorate of Law from the University of Szeged in 1931 and a Doctorate in Philosophy from the University of Budapest in 1943. From 1931 to 1945 he worked in the National Museum in Budapest, first as Curator of Prehistory between 1931 and 1935 and later as head of the Prehistory section (Megaw 1983). Gallus published prolifically, although often only in brief reports on Museum holdings or on preliminary results of trial (or exploratory) excavations. These publications were generally of two to five pages length and have been briefly tabulated below:

Table 1: Early publications by A. Gallus

Year	Topic
1934	Preliminary results of excavation at Fonyod, Hungary
1936A	Report on excavations at Sangvar, Hungary
1936B	Report on a large tomb at Nagyteteny, Hungary
1936C	Report on trial excavations at a neolithic site in Tallya, Hungary
1937	Report on a fortified settlement near Sopron, Hungary
1937A	A summary of new palaeolithic finds in Hungary
1938- 1943	Report on on trial excavations at Jaszdozsa, Hungary
1939	Report on holdings in the Hungarian National Museum

The brief and descriptive nature of these pieces likely reflects the administrative role in which Gallus would have increasingly found himself as he rose to a directorship role at the Hungarian National Museum. Gallus kept up this publication output throughout the Second World War and completed his second doctorate through this period. The context needs to be understood, that Hungary changed its allegiances from Axis to Allied powers several times during the war. This cannot have contributed to a secure environ for those such as Gallus who held positions in state-run institutions. Nevertheless, these brief publications by Gallus continued through the war. These items too are tabulated below for easy reference:

Table 2: Publications by A. Gallus during World War 2

Year	Topic
1940	Kokori Magyar pottery
1940A	National Museum of Hungary holdings
1940B	Regional museum holdings
1940	Local archaeological typology & chronology (with T Horvath)
1940D	On the find of a Celtic kiln
1940E	Pre/Historical international relations
1941	Discovery of a Magyar (early Hungarian) cemetery
1942C	Celtic Jewellery

Year	Topic
1942A	Excavation reports
1942B	Regional museum holdings
1943	Ancient cremation methods

Two instances of significantly longer pieces by Gallus from this period include his typological paper on iron age funerary lamps of central Europe (Gallus 1938A), and his study into the laws of typological seriation (Gallus 1942A).

Gallus' longer publications are more informative as to his intellectual interests away from his museum employment. His study on Iron Age Celts first brought him to the attention of the then-student Vincent Megaw, as Gallus' work on the topic (Gallus 1935) was considered authoritative. The theme of international relations and the movement of peoples is one that Gallus explored in one of his longer journal articles on the Balkans (Gallus, 1938) and later on the topic of Italy and the Carpathian Basin (Gallus 1944-45).

One of Gallus' most notable contributions to Central European archaeology came in his two volume work, co-authored with T. Horvath on pre-Scythian horse-riding people in Hungary – continuing his work on the theme of population mobility and change (Gallus & Horvath 1940) Megaw 1983). It is of note that even at this relatively early stage in his academic career, Gallus published on the methodology and theory of archaeology to a length greater than most of his other journal archaeological publications. For example, Gallus (1936) 19-page article contains an introduction to the principles of contemporary science. In 1942 he published a 45 page piece on principles of typology (Gallus 1942D), followed in 1944 by a 20 page piece on methodological principles in history (Gallus 1944). Gallus was from an early stage a person with a well-developed interest in theory, rather than a strict empiricist concerned only with cataloguing museum holdings.

Gallus' intellectual breadth is likely reflected in correspondence by his son, who in response to my email enquiry, provided evidence that he seems to have absorbed a broad range of intellectual understanding from his father. In his response to my enquiries, Gallus Junior depicted his father as:

...a person with wide interests outside his specialty (evolution, pre-history, theories of history and cultural evolution, 'Volkerwanderung'¹⁴, Carl Jung, Teilhard de Chardin, Spengler, even quantum physics, and so on). He loved the poetry of Rainer Maria Rilke. In many ways a 'polymath'. (A. Gallus Jr. email correspondence to the author, 8 June 2021).

Professor Alexander Gallus Junior is Emeritus Professor of Haematology at Flinders University School of Medicine (Flinders University 2021). To the best of my knowledge, haematology is not a profession for which an awareness of 'Volkerwanderung' and Spengler are prerequisites, and I suspect strongly that the household in which Gallus Junior grew up was a richly intellectually infused environment, reflecting the personal characteristics of his eminently qualified father and university qualified mother.

5.3 THE SUSPENSION OF GRAND ARCHAEOLOGICAL NARRATIVES

The emergence of culture-historical archaeology was intimately tied to what would ultimately prove to be horrific historical outcomes. Gustaf Kossina (1858-1931) published among other works his *Die Herkunft der Germanen (The Origin of the Germans)* (1911). Kossina was a passionate nationalist who considered archaeology to be the most nationalistic discipline, particularly for the study of ancient Germans. In his search for the origins of the German people, Kossina proposed that the archaeology of central Europe from Mesolithic times onwards could be drawn as a mosaic of cultures. He developed a concept of identifying characteristic artefactual and settlement archaeological signatures *Siedlungsarchaeology*, through which he proposed superior German antecedents could be distinguished from their 'inferior' Slav neighbours. Further, Kossina proposed that progress in material culture was a direct result of human biological superiority. This ideology was eagerly absorbed by the Third Reich and its propaganda mechanisms (Trigger 2006, pp.235-239). In particular, the Nazi party *Ahnenerbe* was an organisation within the SS for the research and promulgation of racial superiority based on archaeological evidence (Arnold

¹⁴ Population movement

2008). This mode of thinking, that cultural advancement was linked to racial superiority, had penetrated the English-speaking world too, notably through three foundational works by Gordon Childe. In *The Dawn of European Civilisation* (Childe 1925), *The Aryans* (Childe 1926) and *The Danube in Prehistory* (Childe 1929). Childe similarly to Kossina equated material culture with racial or ethnic grouping, connecting advances in material culture with racial types and also ethnic properties such as language. Childe enshrined Kossina's racist perspectives at the close of *The Aryans*, noting that it was the 'superiority in physique' of Nordic peoples that had made them appropriate bearers of elite culture (Childe 1926, 211). Childe would later repudiate these remarks as shameful, particularly in light of his eventual Marxist positions (Trigger 2006, 248).

Largely resulting from the recognition of the horrific manner in which archaeology had been utilised in fomenting nationalistic and racist social forces in the Third Reich, there was after World War Two a notable turn in English-speaking archaeology away from grand archaeological narratives, such as the search for the diffused origins of elite peoples. Instead an emphasis was placed on empirical site-specific studies that stressed local functional analyses. Prominent among these empirical English practitioners was Grahame Clarke, who commenced as a lecturer in 1935 and trained large numbers of Cambridge graduates. Clarke was dismissive of Childe's attempts to apply overarching explanatory concepts such as Marxism, to archaeology.

Gallus in contrast, had a long interest in grand narratives of humanity, which he had published in work on the Celts (Gallus 1935) on the Balkans (Gallus 1938) Italy and the Carpathian Basin (Gallus 1944-45) and in his two-volume work, co-authored with T. Horvath on pre-Scythian horse-riding people in Hungary – (Gallus & Horvath 1940C). In these works, Gallus did not engage in estimation of superiority, or ranking of racial and ethnic types. Rather, he displayed a genuine interest in the formation of human groups in and of their own standing, highlighted in his enquiry to their geographic origins and interrelationship with other cultural groups. As will be seen later, considerable friction resulted in Australia from Gallus' ongoing willingness to engage in grand narrative archaeology, rather than solely empirical archaeology as taught by Clarke at Cambridge.

5.4 FLIGHT TO AUSTRIA

Gallus' phase of life at the Hungarian National Museum came to a dramatic end with the looming Russian occupation of Hungary in 1944 and the end of the Second World War. It is extraordinary that Gallus had continued publishing up to this time. After the war, prospects were not good under the communist occupation for those who had held public office under the at-times Axis-allied Hungarian government. Alexander Gallus Junior expressed the view that simply being of bourgeois class and an academic archaeologist would have counted against his father – as it did for those of his family who remained in Hungary and who subsequently experienced significant discrimination. Gallus fled Hungary with his young family to Graz in Austria in 1945, where they were “hosted by amazingly generous distant relatives who saved us from refugee camps” (Gallus-Jr 2021).

Although safe from the prospects of a refugee camp, Gallus found that there were no solid prospects for displaced academics in a region that had been devastated by war. Gallus' language skills provided him a useful resource. He had been raised near the Austrian border and was a native bilingual speaker of Hungarian and German, he could also read and speak French, could make basic conversation in English, and as was standard for middle-class education of the time, was familiar with Latin (Gallus-Jr 2021). His son noted that his father also certainly thought in all these modern languages, as the margins of his books were covered by notes in Hungarian, English, German and French. As a result of these language skills, Gallus was able to gain work as a translator for the English-speaking occupation forces in Austria (Gallus-Jr 2021), to which Megaw adds that he found employment at the Language School of Graz University (Megaw 1983), the second oldest and still one of the largest universities in Austria.

In a further demonstration of his capacity for publication even under straitened circumstances, Gallus published one known piece during his time in Austria. This is a nearly 50-page article in German, published in what was then a relatively new Argentinian archaeology journal (Gallus 1947). In this 1947 piece '*Ueber die grundlagen der urgeschichtlichen methodik*' [Regarding the foundations of method for the study of prehistory], Gallus emphasised the concepts of historical landscape, relationships between artefact typologies, concepts of culture and ethnic identity, and different scales and modes of human organisation and existence. It is notable in this lengthy piece and considering the

wide-ranging nature of his analysis that Gallus cites only twelve sources in his 50 pages. It is possible that we are witnessing here a maturation of Gallus' thinking into that of a more assured and confident intellectual, more willing to engage in wide-ranging and philosophical discussion on archaeological and anthropological topics without a need to provide detailed citation for each and every point raised. As will be noted later with reference to perceptions of his work in Australia, this tendency may not have been without its disadvantages as it could be viewed locally by Australian scholars as intuition or speculation, irreconcilable with an empirical approach.

5.5 AUSTRALIA - EMIGRATION AND DENIGRATION

Despite Gallus' employment positions as a translator and an employee of Graz University, the situation in Austria remained considerably less than ideal. Austria, as much of Europe, continued to struggle with the economic and social aftermath and ruptures of the Second World War. The tenuous and adjunct positions of employment that Gallus had obtained may have satisfied his families immediate economic needs but would have done little to ensure the long terms prospects of his children's future. Emigration seemed to be the Gallus' families best option. Although English was only Gallus' fourth language, the family migrated to Australia in 1949 as much for their children's sake as for the family as a whole (Gallus Jr-2021). This story of post-war migration is not unique in Australia, nor was it unusual that immigrants to Australia had difficulty in getting their qualifications recognised. Australia at the time was described as 'a xenophobic nightmare' by fellow immigrant Vincent Megaw (Megaw 1997). Or, as put by Anne Coombs in her history of social movements in 1950's Australia:

Australia was a narrow place; narrow-minded in the perception of what was wrong and what was right – morally, politically, socially – narrow in its lack of appreciation of diversity, narrow in a national pride that was coupled with xenophobic arrogance. New migrants were welcomed because their labour was needed, but were simultaneously required to shed their foreignness as quickly as possible. (Coombs 1996, pp.16-17)

As much as it would be inappropriate to allege without specific evidence that Australian academics at the time gave Gallus short shrift due to xenophobic motivations, it would also

be naïve to consider that the prevalent social xenophobia of the time did not penetrate attitudes of Australian academics.

It is my view that the general intellectual malaise of the time regarding archaeology would have counted against Gallus' recognition as a world authority on his arriving in Australia. As Megaw notes, archaeology in Australia was subject to an almost total lack of interest:

When the visit of Vere Gordon Childe, Australia's greatest archaeologist, to Melbourne in 1957 was met with almost total lack of interest, it was perhaps hardly surprising that the only other expert in Central European prehistory then in Melbourne, Sandor Gallus, should also have been ignored. (Megaw 1983, p.3)

One could add that at the time of his visit to Australia in 1957, Gordon Childe was almost certainly the preeminent archaeologist in the world - and is still one of the most prolifically published archaeological authors. Yet the Australian government not only had no idea who the Australian Childe was but had to cable MI5 in London for his details (Lever 2015). If such was the state of Australian ignorance and disinterest around archaeology, then the ignoble situation that Gallus faced in Australia is more understandable.

At this time, university degrees from non-English speaking countries were not recognised in Australia almost as a matter of principle (Megaw 1983). It is likely that Gallus had some forewarning of this, but I suggest that Gallus could not have fully prepared himself for the reality and magnitude of the impending changes in his workplace, lifestyle, self-perception and social position. Gallus had fled a position as a section director of his home nations' most prestigious museum, to work in the language school of a prestigious Austrian university. On arrival in Australia and for the six years until 1955 he had only menial employment including as a cleaner and a clerk (Megaw 1983). After some effort he was able to obtain a position as a temporary teacher with the Victorian Education Department. He then trained as a teacher, graduating in 1962 at the age of 55 years after which he received a position with the Victorian Education Department Correspondence School (Megaw 1983). His son reflects: "I recall his English as competent (if accented and not wholly idiomatic; and people weren't as tolerant of non-native English then as now)" (Gallus-Jr 2021). It is probably reasonable to assume that his initial difficulties in finding

less-than menial occupation were at least partly due to his level of spoken English. It may also be that his obtaining a position in the state correspondence school was a wise choice for a 55-year-old starting teacher with a foreign accent and perhaps less than colloquial spoken English.

Figure 14: Alexander Gallus in approximately 1953. By kind permission of Dr Alexander Gallus Jr



Alexander Gallus was not the only one in his family making such sacrifices. Alexander Gallus Junior recalls that his university educated mother worked as a seamstress for the Melbourne rag-trade. Having had my first career in the Melbourne rag-trade some 30 years after the Gallus family arrived, I can attest that even with the passing of time into the 1980's the glamour of the fashion industry did not extend beyond the sewing room door. Sewing rooms were often crowded spaces with women packed close together, hunched over industrial sewing machines and subject to loud noise in an environ laden with floating threads, dust and scraps.

Seamstresses were closely supervised for speed and quality of work with minimal breaks and with their attendance closely monitored through time-clock punch cards (Bundy machines). This would certainly have been a drastically different environment for Mrs

Gallus – and the fact that both parents now worked menial jobs will also certainly have impacted significantly on the social and economic lives of the family as a whole. These changes may have also had emotional impacts on the family unit, with frustrations at their work situations suppressed at work in the face of the need to maintain income.

In concluding this section, the tragic folorn figure descending the ladder at Koonalda Cave in 1967 is now seen as far from one of non-achievement or irrelevance as the camera would have us believe. On the contrary, Gallus was an individual who moved rapidly, almost in stellar fashion to the head of his profession in Hungary, a country which had and continues to have a very high standard of archaeological investigation and recording (Megaw 1983). His 1935 work on Celtic culture (a wide ranging and complex field), published when he was 28 (and almost certainly written well before that), was considered an authoritative work of the time. Despite the challenges of managing a state museum section, and through the difficulties of the Second World War, Gallus continued publishing both short pieces, but also more substantial pieces on a range of topics encompassing the philosophy, theory and methods of archaeology and anthropology.

Leaving Hungary for Austria, Gallus found employment in Austria through the application of his language skills and language aptitude. Finally, having arrived in Australia at the age of 42, and finding himself faced with a lack of recognition of his skills or qualification, Gallus undertook menial work for six years before obtaining work as a temporary teacher. I suspect that finding such temporary teaching work was the outcome of a process of many demoralising and unsuccessful employment applications by Gallus.

The weight of these six years of menial work is likely reflected in the longest hiatus in Gallus' publication record. Previously, the two year gap between papers published as Gallus (1944-45) and Gallus (1947) had been the longest break between articles by Gallus - notwithstanding the Second World War and his refugee status. After Gallus (1947) however, I have not been able to locate articles by Gallus until seven years later with the publication of Gallus (1954) - a German language report on his excavation at a site in Keilor, Victoria (discussed below). Many factors could lie behind this hiatus in publication; perhaps a lack of access to Australian University archaeology collections and literature, certainly in his native languages, lack of access to excavations and museum collections and possibly a preoccupation with work and raising his family.

However, I will argue that something deeper was taking place. Many of these factors were at play during the Gallus family's stay in Austria, yet Gallus managed to produce a significant theoretical work, written in German, during his time there (Gallus 1947). As will be seen later, Gallus's reputation and work remained highly regarded internationally after his arrival in Australia; and it seems unlikely he would have faced difficulty in having his works published outside Australia had he chosen to do so. What we see here in this hiatus is the nadir of Gallus's academic career, a low point from which he then made significant, dogged and concerted efforts to rebound. These efforts include the bemusing (likely humbling) experience of returning to university as someone of 54 years of age, with multiple publications and holding two doctorates, to study education alongside recent high school graduates.

As we will shortly see, Gallus worked hard to become recognised as an archaeologist in Australia, as well as to build local archaeology. As alluded to in the opening section of this chapter, Gallus was never accepted by the Australian archaeological community and never obtained an academic position in Australia. In 1961 Gallus unsuccessfully applied for a position in European Prehistory at the University of Sydney (Megaw 1983, Mulvaney, 1998). The position went to his academic admirer, Vincent Megaw (Jackson 2014) Gallus was unsuccessful in this application, but as we will see, by this time his general prospects had improved. His inability to find academic employment in Australia was not due to any deficiencies in the nature of his work - twenty years after Gallus left Europe, whenever Megaw met specialists in European archaeology overseas, an almost inevitable first question would be the welfare of Alexander Gallus (Megaw 1983).

Gallus' name had not faded in international circles as an authoritative figure in European archaeology. He provided a lengthy chapter to the work on *Form in indigenous art* edited by Peter Ucko in 1977 (Gallus 1977). This includes Gallus' theories on what he considered to have been art at Koonalda Cave. Why then, given Gallus' international reputation and credentials was his name not accepted in the Australian academic archaeological community? Gallus did achieve membership of the Aboriginal Institute of Aboriginal Studies - but this too was not without local resistance (Megaw 1983, Megaw 1997).

In the remainder of this chapter I will explore several threads of evidence relevant to this question. Firstly, I will explore his excavation - the manner in which Gallus became involved

with the Archaeological Society of Victoria (ASV) – an amateur group, and his excavation and publication of a site at Keilor Victoria with the ASV which provided him with a platform for engagement in local journals and conferences. Further, I will consider his engagement at Koonalda Cave and the manner in which his interpretation of its archaeology was treated by mainstream archaeologists of the time. Secondly, I discuss the demonstrable quantitative disparity in education levels between Gallus and Australian archaeological practitioners of the time. Thirdly, I speculate on certain differences in cognitive modes of thought that existed between Gallus and his Australian peers.

5.6 GALLUS AND THE ARCHAEOLOGICAL SOCIETY OF VICTORIA

Gary Presland, the eminent local historian of Melbourne, was involved in the formative years of the Archaeological Society of Victoria (ASV) including participation in early archaeological excavations that the ASV organised. Presland has provided a brief account of Gallus' "significant and crucial role in the early development of the society" (Presland 1998, p.9), which included Gallus taking a role on the ASV committee from its founding year of 1965. The society took seriously its role to provide public education and practical training in archaeology. Academics such as Bill Cullican of the University of Melbourne delivered free lectures – and Gallus presented a lecture series in 1966 on ancient Greece. Equally significant is that Gallus had been excavating at Dry Creek near Keilor outside Melbourne since the mid-1950's (hence his German language preliminary report noted above) (Gallus 1954).

Gallus had also been working at Koonalda Cave since 1958, having made four privately sponsored field trips there with enthusiasts, and one trip in 1965-66 funded by the Australian Institute of Aboriginal Studies (Gallus 1971). Gallus's work at Keilor was one of only a few archaeological projects being undertaken in Victoria at the time. The others were being undertaken through institutions: Mulvaney and Casey (and to a far lesser extent RVS Wright) operated within the aegis of university and museum archaeology departments, and Aldo Massola through Museum Victoria. With the formation of the ASV and the transfer of archaeological studies at the Keilor site to the ASV under Gallus, a far wider vista of opportunity was available to people wanting to train in archaeology at a time when no formal degrees in the subject yet existed. Gallus regularly published the results of his findings at Keilor - in at least sixteen articles that ranged from general results reporting, to

detailed stratigraphic and site formation analysis, and also items that tied into wider themes.

This intensive publication regime was carried out at rapid pace, particularly considering that Gallus' archaeological work was a part-time affair undertaken in his own time. Gallus' publication record on Keilor spanned nearly 15 years and included initial site reporting and ongoing almost annual, sometimes twice-yearly, updates. Of particular note is that, from early on, Gallus attempted to tie his Keilor results into the global story of human evolution and migration, presenting a conference paper at an ANZAAS¹⁵ conference in 1969 on *Palaeolithic Stratigraphy of the Maribyrnong River Terraces and its Bearing on the Problem of the Presence of Homo Sapiens and Middle Pleistocene Man in Australia* (Gallus, 1969). Two significant reports were issued by Gallus in 1971 and 1972 on his excavations at Keilor (Gallus 1971A) (Gallus 1972A). Gallus also delivered excavation updates at conferences - such as his paper on Keilor to ANZAAS in 1975 (Gallus, 1975). It is noteworthy, and will be further noted regarding Koonalda Cave, that Gallus adhered to European modes of categorising lithic implements, rather than adopt a scheme after then locally widely accepted morphological categories such as based on work by earlier Australian researchers, notable McCarthy, Bramell, and Noone (1946) (Gallus 1971). Gallus' last conference paper on Keilor, in 1978 (Gallus, Orshiston, Patrick, & Ferguson 1978), was presented four years after excavation at Keilor had ended following major flooding that had irreparably damaged the site (Presland 1998).

This level of excavation and publication by Gallus constitutes a significant workload for any archaeologist, let alone a part time 'amateur'. But this renaissance in Gallus' archaeological activity - certainly a renaissance in comparison to his years immediately following migration to Australia, is heightened when one takes into account that at the same time that he was excavating and publishing Keilor, he was also involved in five field seasons to Koonalda Cave. Gallus also published these Koonalda Cave field results in *The Artefact*. These publications were not limited to excavation reports but covered wider topics including archaeological method and theory. Gallus had also commenced engagement with English speaking local media and international academic journals – in particular *Current*

¹⁵ Australasian Association for the Advancement of Science

Anthropology. Gallus had become an Associate of *Current Anthropology* in 1963 (Megaw 1997) and he contributed papers on a wide range of topics and becoming a frequent commentator on pieces that touched on his specific interest in Cognitive Archaeology. It is worth noting that while one form of Cognitive Archaeology enjoyed some popularity from the 1970's through the advocacy of prominent practitioners such as Colin Renfrew (Trigger 2006), Gallus' interest in the field and publication on it predates the development of it in the English speaking world by an apparent span of at least two decades. Gallus' articles mentioned earlier, including Gallus (1947) illustrate considerable exploration of approaches that could be termed cognitive avenues of investigation. Yet it must be noted that Gallus' approach differed significantly from the British Cognitive Archaeology which took hold from the late 1970's onwards e.g. Renfrew (1982), Renfrew & Zubrow (1994). Gallus made humanitarian, philosophical and psychological explorations of the manner in which humans in the past had experienced their world – utilising empathetic understandings from the present to better comprehend the past. In contrast, Renfrew and his peers sought a far more scientific perspective through which to understand the neurological interaction between humans and the physical world – or what Renfrew and his co-author Malafouris termed 'Neuroarchaeology' Renfrew and Malafouris (2008).

Select examples of the breadth of Gallus' interest include his piece on *A Biofunctional Theory of Religion* (Gallus 1972) and later *Comments on Alexander Marshack: Upper palaeolithic Symbols Systems of the Russian Plain: Cognitive and Comparative Analysis* (Gallus 1978A). His comments illustrated a wide ranging, philosophically and anthropologically informed reading such as seen in his *Comments on K E Rosengren: Malinovski's Magic: The Riddle of the Empty Cell* (Gallus 1976A) and *Comments on P.D. Sheets: Behavioural Analysis and the structure of a Prehistoric Industry* (Gallus 1975A).

An example of a select item of Gallus' Australian publications that illustrates the depth of scholarship he brought to Australia is his 1968 piece on art at Koonalda Cave published in the *Helictite* (Gallus 1968). The paper is a theoretically informed conceptual piece which shows Gallus thinking globally in comparing Koonalda, Australian archaeology and Aboriginal peoples to sites and cultures worldwide. His conceptual palette is similarly broad as evidenced in his turn to Jung to interpret the meaning and causes behind the creation of the 'finger-groove art' at Koonalda. At Koonalda the soft limestone walls of the cave had

frequently been shallowly scored with curvilinear finger grooves, which Gallus proposed could be interpreted as:

...according to Jung whenever man faces the unknown, such a background is peopled with formations of the projective activity of the subconscious mind (Jung 1953). In the abstract maze covering the walls of the cave, we might see the representation of emotional states and stresses of early men, who released psychic tensions in this way. But what tensions? We have no way of knowing" (Gallus 1968, p.48).

Over the past 40 years of global archaeology, and likely in Australian archaeology of the last 20 years, such a wide ranging and philosophically analytic interpretative mode may well have been widely accepted. In Australian archaeology at the time however, under the firm hand of strict empiricism that will be depicted in the following section, such interpretations were seen at the very least as being as excessively specious.

At the same time that Gallus had been excavating at Keilor, public interest was also kept engaged in local archaeology by the excavation of an Aboriginal burial at Green Gully by R.V.S Wright of the University of Sydney. As one of the few active ongoing excavations in Victoria, Gallus' Keilor site was under the eye of prominent local scholars - not least E.D Gill (1908-1986), palaeontologist, geomorphologist and museum administrator, and Jim Bowler, geomorphologist. Presland (1998) argues that, given the scale of their activity, output of reporting and oversight by prestigious individuals such as E.D. Gill, "Dr Gallus and members of the ASV were at the forefront of Australian Archaeology of the time" (Presland 1998, p.11). This is a powerful statement of acknowledgement by Presland - an eminently qualified individual who was well aware of the contemporary activities of Mulvaney, Massola and R.V.S. Wright.

By the mid-1970's, the Victorian Archaeological and Aboriginal Office had been established (later to become the Victorian Archaeological Survey), and sites of archaeological potential were increasingly deemed not suitable for excavation by 'amateurs', thus resulting in a scaling down of excavation by Gallus and the ASV.

5.7 THE THREAT OF THE AMATEUR ARCHAEOLOGIST

I have shown that after Gallus arrived in Australia in 1949, he did not publish for several years until his 1954 Keilor publication. A noticeable upturn in publishing is then evident, as Gallus regularly provided progress reports in *The Artefact*. But perhaps it was precisely this close engagement with an amateur organisation – at all levels from excavation to publication and committee membership, which contributed to the manner in which Gallus was regarded askance by the small archaeological academic community of the time - chiefly John Mulvaney, R.V.S. Wright Alan Thorne, Rhys Jones, Jim Allen and Isabel McBryde.

When Mulvaney took on the presidency of the Anthropological Society of Victoria in 1958, he was driven by a sense of moral duty to archaeology: ‘morality demanded that I become holier-than-thou... educate these amateurs’ (Mulvaney 1990, p.249). The very fact that a second and competing society - the Archaeological Society of Victoria- was founded in 1965, reflected on the moribund nature of Mulvaney’s Anthropological Society of Victoria - which by 1976 had dwindled to ten members and was forced to merge with (Gallus’) Archaeological Society of Victoria in order to continue its existence. Gallus was successful in enthusing participants and generating funding from an amateur organisation for excavations at the same time that the Anthropological society with which Mulvaney had been involved was effectively defunct This is more noteworthy given Mulvaney’s stance in depicting himself as having a moral obligation, not merely an intellectual investment, to rescue archaeology from amateurs. Gallus has been depicted as holding an almost larger-than-life presence and charismatic hold over the members of the ASV who attended his lectures and excavations eagerly and referred to him as ‘the Doc’. Megaw observed that the younger generation of archaeologists at the time “in a real sense worshipped the Doc” (Megaw 1983, p.4). That Gallus handed archaeology back to enthusiastic amateur engagement cannot have gelled with the professional disciplinary trajectory that Mulvaney had hoped for.

In the environment of the time - before the tighter regulatory schemes in Victoria and NSW that were implemented in the mid 1970’s, the prospect of competition for sites, funding and personnel would have been a serious consideration, and the prospect that an amateur band of archaeologists could descend on a site with private funding and excavate and publish the site outside of the university system – this would have been grounds for great

concern to a professional university archaeologist such as Mulvaney. It is my view that academic archaeologists resented, and quite likely felt threatened at the prospect of a world-renowned scholar such as Gallus lending his imprimatur to the activities of a group of enthusiastic and philanthropic volunteers.

This was because Gallus worked outside the university system and therefore independent of the checks, balances and reviews inherent to that world. This threat was perhaps more real than it appears in hindsight. Not only did Gallus attract attention in terms of attracting volunteers, raising funding for excavation and publishing prodigiously, he also started to speak on behalf of Australian archaeology, both inside and outside Australia, in wide ranging and popular terms. Alexander Gallus wrote in broad brushstrokes that matched the thematic scope of contemporary archaeologists such as Rhys Jones' *Firestick Farming* (Jones 1969), or the slightly later thesis of *Intensification* by Harry Lourandos (Lourandos 1977). His ten-page piece in the Australian periodical magazine *Twentieth Century* entitled 'Expanding Horizons in Australian Prehistory' Gallus (1970), is, in my view, the first such 'public relations' piece by an Australian archaeologist (certainly of this length). The title and content of it make it evident that Gallus positioned himself to speak on behalf of Australia, its archaeology, and the way in which it would go forward. The xenophobes who in the 1940s and 1950s welcomed post-war migrants with cries of 'bloody reffo' (Megaw 1983, p.3) had not simply disappeared by 1970 nor had their children universally turned their backs on their parents' ways - and it is not unreasonable to suppose that xenophobia existed within academic archaeology too.

This then, the willingness of Gallus' to speak, in his 'non-idiomatic' (Gallus-Jr 2021) English and foreign Hungarian accent ¹⁶ on behalf of Australian archaeology would likely have rankled some Australian archaeologists. On this note of academic competitiveness let us turn back to the topic that opened this chapter - the joint 1967 season of excavation at

¹⁶ The State Library of Victoria holds two cassette tapes of interviews with A Gallus. Unfortunately these have deteriorated to the point where they cannot be accessed and I therefore cannot verify the nature of Gallus' in English:

State Library of Victoria 2021. Oral history interview with Dr. A. Gallus, 1978 Oct. 31 [sound recording].
http://search.slv.vic.gov.au/permalink/f/1cl35st/SLV_VOYAGER892266 (Accessed 20 May 2022)

Koonalda Cave. I have previously described the film representation of Gallus at Koonalda in 1967, and in the next section I examine academic publications on the archaeology of that site.

5.8 MEASURES TO CORRECT: KOONALDA

The slim volume documenting the 1967 University of Sydney expedition to Koonalda Cave, edited by R.V.S. Wright, (Wright 1971) gives an impression of a harmonious enterprise, undertaken in conjunction with Gallus. The title of the work and site name are given as “The Gallus Site” and a brief mention in the preface by Wright notes:

Special thanks are due to Dr. A. Gallus, the discoverer of the cave’s archaeology. Faced with disagreement he has held firmly to his interpretations of the site, but in a spirit of true scientific altruism he has generously let others investigate. (Wright 1971 p.iii).

Yet, the depth of division between Gallus and Wright’s team becomes evident on scanning the index of the work which is clearly divided into input by Wright’s team in Part One, and by Gallus in Part Two. Wright depicts this division as due to a total incompatibility of underlying assumptions, terminology and interpretations stating: “No hybrid could have been produced without mutilating surgery that would have been unacceptable to the authors of both parts.”

I question that this was the case. Rather it seems that had good will existed, the results of at least four of the seven chapters by Wright’s team could have been integrated with Gallus’ contribution. These chapters include the Ethnographic Background (Chapter One), Vegetation surrounding the cave (Chapter Two), The Sediments (Chapter Four) and The Fauna (Chapter Five).

What I believe is at play here is the attempt by R.V.S. Wright’s team to frame the expedition as one of applying scientific method in contrast to Gallus’ intuition and use of European terminology, reserving for themselves the right to document the cave in as an a-theoretical and scientific (scientistic) manner as possible. This then left Gallus to take up his part of the publication from Chapter Eight onwards, after the reader has already processed and presumably accepted the depiction of the cave by scientists in Part One. For, if Gallus truly was owed gratitude for discovering and first excavating the site, I would expect that it would be his interpretation placed first. Further, that disagreement between Gallus’

account and that of the University of Sydney team could be left for the reader to observe during their reading without being explicitly voiced. However, R.V.S. Wright does not leave this disagreement for the reader to deduce. On the contrary, Wright quite explicitly seeks out open contradiction with Gallus, in Wright's Chapter Six - The Flints. Without entering into close lithic analysis, I have mentioned above that at Keilor and Koonalda Gallus used terminology characteristic of European archaeology to describe Australian lithic artefacts (e.g. "Lame Levallois", "beaked burins" or "Narrow-nosed high scraper" (Gallus 1971, pp.98-102)). Wright states that he had seen the artefacts from Koonalda that Gallus defined as 'man-made' (sic) and that Gallus had described in European terminology. Wright bluntly refutes that many of these items are artefacts at all. In short, Wright, in a work undertaken with Gallus and which plays overt homage to Gallus' contribution, effectively calls Gallus incompetent or disingenuous, in print (Wright 1971, pp.56-57). Gallus in turn makes no mention of any of the chapters by other authors.

The larger issue at play here is not who was a superior analyst of stone tools. The issue is one of gatekeeping the perceived scientific values in archaeology - who may and who may not enter the Australian archaeological academic community. In the same volume, the chapter by Lesley Maynard and Robert Edwards (Chapter Seven, Wall Markings) mentions and negotiates a difference in interpretation with Gallus regarding finger-groove markings in the Koonalda Cave walls without forcing confrontation (Wright 1971, p.79). Perhaps the difference in relative antagonistic attitudes to Gallus can be explained biographically. R.V.S. Wright was at the time a Senior Lecturer in the Department of Anthropology, University of Sydney.¹⁷

Lesley Maynard was then preparing a masters thesis in anthropology, and Robert Edwards was Curator at the South Australian Museum. Wright had excavated near Keilor where he almost certainly would have become aware not only of Gallus' successes but also his applications of European interpretive schemes – Koonalda may have offered Wright an opportunity to set straight old standing grievances. Edwards seems to have primarily contributed to the chapter through his photography with little call for entry to intellectual

¹⁷ Aboriginal and colonial archaeology were not taught in the Department of Archaeology at the University of Sydney until 1978, prior to which they were taught (if at all) in the Department of Anthropology

affray. Lesley Maynard, as a student, may not have seen it as beneficial to openly contradict Gallus - an archaeologist of over 40 years experience at the time. Further, as someone who went on to complete her masters thesis on Australian rock art (Maynard 1976), Lesley Maynard may well have been more disposed to Alexander Gallus' ideas, than were other participants in the field season. This seems evident in the opening pages of Maynard's masters thesis, where Maynard indicated dissatisfaction with the dominant empirical approach to Australian archaeology, noting with specific citation from Mulvaney's first synthetic work on Australian Prehistory (Mulvaney 1969) that John Mulvaney was generally dismissive towards the potential of rock art studies as a valid field of archaeological research. Maynard cited Mulvaney:

"A prehistorian may infer methods of application or techniques of engraving, from observation, but comments concerning motivation and meaning is beyond the scope of normal archaeological activities."
(Mulvaney 1969 p. 174 in Maynard 1976 p.2).

Maynard summarises the rest of Mulvaney's chapter on the topic: "The rest of this discussion "Prehistory and Art", is a resume of all the things that cannot be inferred about prehistoric Aboriginal art" (Maynard 1976.2).

5.9 QUANTITATIVE AVE EDUCATIONAL DISPARITY

It is hardly contentious to state that, although change has come about in the last decades, for long periods most Australian archaeology proceeded in an empirical manner with little emphasis on theory or theoretical interpretation. Up to the turn of the 21st century there had been no major theoretical practitioners working long-term in Australia. The closest the discipline has ever come to a theoretical divide was during the "intensification" debate (Lourandos & Ross 1994), and as much as some of its participants may wish to differ, this was not a debate between alternative theoretical stances – each informed by alternate ontological or epistemological stances, but was rather a methodological argument as to how the same form of strict empiricism could be applied in different ways to the same archaeological data.

Whatever the underlying cause for the general historical disregard towards theory by Australian archaeologists, I believe a comparison of the educational backgrounds held by Gallus and Mulvaney will highlight the potential incommensurability of their positions.

Perhaps the simplest means of demonstrating clearly the very significant difference in education between Gallus and his peers in Australia is to examine, compare and contrast his own education with that of the senior Australian prehistorian of the time, the 'Father of Australian Archaeology' - John Mulvaney (Griffiths 2016). I will look at their educational records and then undertake a critical reading of a piece by Mulvaney portraying Gallus (Mulvaney 1998). I believe the understanding gained from examination of this piece will illustrate that Mulvaney could not have comprehended sufficient of Gallus' thinking and motivations to formulate a coherent critique of Gallus's works.

Gallus had two doctorates and many years of excavation and publication experience prior to arriving in Australia in 1949, at which time Mulvaney had not yet formally studied archaeology at all. Gallus also had spent significant time running the prehistory department of the National Museum in a country with a deep knowledge of its prehistory. By the time he reached Australia in 1949, Gallus' publications evidenced a wide ranging and deeply informed education. In contrast, Mulvaney held a master's degree in history from the University of Melbourne, but Mulvaney did not study archaeology at postgraduate level then or at any later time in his career. Mulvaney's time at Cambridge, after completing his master's in history, was spent completing only the first two years of a three year undergraduate course. There is no evidence I have found in any of Mulvaney's writings from any time in his career to suggest that he engaged actively with archaeological theory or philosophy of any nature, whether while an enrolled student or a practicing academic. The closest Mulvaney would appear to have come to such topics would be an article 'Human cognition: the Australian evidence' (Mulvaney 2012). This was a workman like and empirical call for the application of works along the lines of Renfrew (Renfrew, 2007) and Steven Mithen (Mithen 1996) to be carried out in regard to Australian rock art. Mulvaney's PhD was granted by Cambridge University in 1971 for works already published. He did not at any time carry out under supervision an independent extended archaeological thesis at honours, masters or PhD level. It may be that Mulvaney was a competent autodidact – but nevertheless the existence of post-graduate faculties worldwide owe their existence to the

reality that enrolled study under instruction and supervision is not something that self-directed study can generally substitute for.

Although he had not spent much time studying archaeology, Mulvaney had however been at the right place at the right time – or perhaps the wrong time, to start his academic career in archaeology before any other Australian. In 1954, immediately after his two-year undergraduate stint at Cambridge ended, Mulvaney's wife was injured in a car crash in England forcing their return to Australia where he took up a teaching position in the Department of History at the University of Melbourne and was able to co-opt funding from that university for limited archaeological excavation programs. In 1965, Mulvaney joined the staff of the Australian National University (ANU) and in 1971 was appointed the Foundation Chair in Prehistory there.

5.10 DISPARITY AND VENALITY

I found it an unpleasant task to turn to Mulvaney's article concerning Gallus; *Dr Gallus and Australian Archaeology* (Mulvaney, 1998). This piece opens a commemorative edition of *The Artefact*, the journal in which Gallus had so often published, and the organ of the ASV in which he was regarded with great affection. Mulvaney's accounts in this article of his encounters with Gallus are, in my reading, unfair to Gallus. Mulvaney seems incapable of mentioning even a potential positive side to Gallus without immediately turning that side to attack Gallus with. Yet, these attacks are made in such an un-self-consciously venal manner that two outcomes eventuate. Firstly, the reader is set aback and turned against Mulvaney. Secondly the reader is led to wonder what the motivations could be that would lead a senior academic to write in this incredibly insensitive manner. Mulvaney wrote this piece effectively as an obituary to Gallus – it was published in 1998, barely one year after Gallus' passing.

I have described above how Mulvaney's attempts to reform the Anthropological Society of Victoria were a failure, with the organisation needing to merge with Gallus' ASV to continue. Mulvaney has painted his efforts at the Anthropological Society there in self-sanctimonious terms, that he needed to be 'Holier-than-thou' to educate the amateurs (Mulvaney 1998). Mulvaney's notion of an attempted salvation of the ASV too - to educate ASV amateurs by denigrating their recently deceased leader? It is not impossible that this is

in fact the case - that having perceived the locus at the ASV in the absence of the departed Gallus, Mulvaney saw it as his role to step in and save the amateur members of the ASV from committing what he perceived as further crime against the archaeological record.

Mulvaney had in the 1950s and 1960s carried out a series of what he himself described as 'numerous sermon-like lectures' which he further described as being of 'missionary purpose' (Mulvaney 1990, p.249). Hilary du Cros takes this further to describe Mulvaney as engaged in a 'private crusade to push out amateurs' (duCros 1996, p.31). When the situation is regarded in these terms, as one of a missionary and crusading endeavour by Mulvaney, it would make sense but still seem of poor taste for Mulvaney to seize an opportunity to address the ASV directly, now without their quasi-professional and academic figurehead of Alexander Gallus and to robustly criticise their recently departed and warmly regarded figurehead who had encouraged them in their previous 'amateur' excavations.

Regardless of any positive motivations that Mulvaney may have had towards the state of archaeology in authoring this article, a prime motivation that seems apparent as suggested above, is a considerably less than generous attitude towards Gallus. Similar trenchant attitudes by Mulvaney are apparent in his article on the return of the Kow Swamp skeletal remains to their traditional Aboriginal owners for reburial (Mulvaney 1991). The Kow Swamp remains consisted of some 40 Aboriginal ancestral individuals excavated by Alan Thorne in the late 1960's prior to the existence of excavation permits and controls. The Victorian Government's decision to return these remains to Aboriginal groups triggered further repatriation of Aboriginal ancestral remains held at Museum Victoria, that had been acquired from Mr George Murray Black. G M Black was a plunderer of many thousands of Aboriginal graves in the 19th century. Mulvaney (1991) protested the return of all these remains in highly self-righteous terms. More extraordinarily, Mulvaney went to the extent of publicly and in academic print labelling the actions of the Victorian Government as "crass ignorance or deliberate misinformation" (Mulvaney 1991, p.20). Several points must be borne in mind in evaluating this statement. Firstly, that the Victorian Government's decision to repatriate these and many future remains was carried out on the basis of consultation with Aboriginal Affairs Victoria, with Museum Victoria including representatives from the Victorian Archaeological Survey, and most importantly, local relevant Aboriginal communities. The government decision to repatriate could be called many things, but not ignorant or misinformed. Secondly, that this statement by Mulvaney

is not a throwaway emotive spoken remark made in the heat of the moment – on the contrary it was made in a peer reviewed journal (*Antiquity*) and will have been subject to several stages of draft, editing, consideration and review before making it to print. Thirdly, that Mulvaney chose to commit himself to such a heated and apparently un-substantiable remark almost certainly reflects that his chief affront was a personal one at having his status publicly challenged. That is, his status as one of the select “keepers of archaeological knowledge” (duCros, *Committing Archaeology in Australia*, 1996, p. 31) (paraphrasing (Clarke 1993)). By the time he wrote this article, Mulvaney had recently retired. This allegation by him of “crass ignorance or deliberate misinformation” is far from the sort of tempered argument one would expect from a senior academic, let alone an emeritus and founding chair of Australian prehistory. It would certainly not seem intended to engage in dialogue with disparate views on the topic or to seek an outcome that reflected the balance of opinion of all parties involved. However, the phrase does well encapsulate what emerges from the rest of the article (Mulvaney 1991) as evidence of Mulvaney’s sense of entitled authority and his infuriated response at having this authority rebutted. Bearing this in mind, we can perhaps better understand why and how some six years later, Mulvaney (1998) may have felt quite self-justified in attempting the literary demolition of the archaeological career of Alexander Gallus, in an item addressed to those who admired Gallus the most (the membership of the ASV).

One of the most telling indicators in this article (Mulvaney 1998) is Mulvaney’s incapability to express an argument around the philosophy of science in precise, let alone nuanced terms. Certainly, he does not display any capacity for diagnostic self-reflexivity or a de-centred understanding of his own intellectual position in anything other than the most basic partisan terms. Having introduced Gallus as perhaps “Australia’s first post-modern archaeologist”, throughout the article Mulvaney refers to Gallus’ work as ‘intuitive’, ‘inference and theory’ ‘deductions’ or ‘spiritual’ and devoid of objective evidence. Of himself however Mulvaney reassures the reader “I was surely the first prehistorian to exemplify the data-gathering approach, so opposed to intuitive methods”. In both depicting his understanding of Gallus’ approach and his own, Mulvaney has in an utterly inarticulate manner lumped together entire worlds of often contradictory theory, thus illustrating his apparent inability to identify the epistemological place of Gallus’ work and his own on a philosophical spectrum. At best, Mulvaney has created an utterly fictitious binary

opposition such as between his “data gathering” and Gallus’ “inference and theory” in order to better set up Gallus as a straw man.

Let us tackle Mulvaney’s self-depiction as a “data gatherer” first. In his eminent and often reprinted Trevelyan lectures, E. H. Carr noted that “The nineteenth century was a great age for facts” providing the often quoted example of the German historian von Ranke who insisted in 1830 that the role of the historian was to write *wie es eigentlich gewesen war* / “simply how it really was” (Carr 1961, p.3). Since von Ranke, and assisted by theoreticians of archaeology and history such as Carr and many others, it has been increasingly obvious that in history and archaeology, facts, data and objects, do not simply exist, waiting for discovery by the archaeologist or historian, independent of framing theories and philosophies of value. More so, that these facts, data and objects do not transfer themselves onto the page without going through significant filters of further framing theories and philosophies of value that exist in the minds of the archaeologist or historian. One may adopt a strict realist, empiricist and positivist position regarding interpretation of such data - which is what Mulvaney appears to be claiming. However, one would expect a senior academic to be capable of enunciating this in more articulate terms than “data gathering”.

In speaking of Gallus’ theoretical propositions, there is of course the issue that much of Gallus’ thinking would have been informed by foreign language authors unavailable to Mulvaney. But even so there were sufficient English language works available at the time, including those in archaeology, that displayed direct parallels to Gallus’ propositions enough that it would be obvious to an informed reader what Gallus’ ontological and epistemological foundations were. Mulvaney’s use of the terms ‘intuitive’ and ‘spiritual’ regarding Gallus simply informs us to the limited range of Mulvaney’s philosophical lexicon, for there were (certainly by the 1990’s, but during Mulvaney’s student years too) more than sufficient English language archaeologists and historians utilising methods similar to those Mulvaney depicts as invalid when used by Gallus. The eminent Oxford don, R.G. Collingwood (Collingwood 1939, 1946), archaeologist and philosopher, was far more of a radical relativist and idealist than I have detected in Gallus’ writings - yet I have seen no allegations in criticism of Collingwood that he was indulging in spirituality. Mulvaney was certainly familiar with Collingwood’s ideas having studied them during his undergraduate at Melbourne University (Mulvaney 1986).

But this is an unnecessarily weak argument – and a stronger one can be made: let us accept that for whatever reason Mulvaney did not read history and philosophy of science and theory in general. Nevertheless, this article (Mulvaney 1998) was published in 1998 - more than 30 years after the ‘New / Processual Archaeology’ emerged in the United States a movement which brought theory to the fore of almost any archaeological conference worldwide (Trigger 2006). All the concepts raised above – including the invalidity of conceiving of archaeology as simply “data gathering” had been thrashed out in archaeological literature for over 30 years. Similarly, Mulvaney’s (1998) article was published 15 years after the emergence of the counter-movement ‘Post-Processual Archaeology’, which heightened theoretical debate in rebutting the strong positivism of the “New Archaeology” and arguing for precisely the sort of relativist and idealist notions advanced by Collingwood nearly 50 years previously - and in Gallus’ work too (Trigger 2006). Rather than perhaps formulating his critique of Gallus in the narrative of Processual vs Post-Processual archaeologies, Mulvaney flounders to define either his own or Gallus’ theoretical position and simply labels Gallus as “Post-Modern” (Mulvaney 1998, p4).¹⁸

Mulvaney depicts that in an uncomfortable circumstance Gallus enrolled at the University of Melbourne for a master’s in history, and Mulvaney found himself allocated to supervise Gallus. Gallus did not continue his enrolment, which Mulvaney considers was due to Gallus finding “my emphasis on objective evidence a barren ideology” (Mulvaney 1998, p5). The notion that one academic (Mulvaney) was in possession of the sole means of determining “objective evidence” is in itself a troubling philosophical statement. A more likely scenario is that encountered with a junior academic with a narrow intellectual and academic scope compared to his own, Gallus chose not to frustrate himself with submission to Mulvaney’s supervision.

I will close this section with the analysis of a quote from the opening paragraph by Mulvaney (Mulvaney 1998) regarding Gallus: “Some of his conjectures and interest in cognition forty years ago possess current relevance, even though most of his evidence must

¹⁸ In empiricist Australia labelling something post-modern is as good as calling it nonsense - it’s a dog whistle for chortles from “common sense” people.

be rejected". The venality of this statement lies in that it wipes away the worth of over forty years work carried out by Gallus in Australia - fieldwork at Keilor and sixteen careful publications of this work and additional excavation and publication of Koonalda cave. Even where Gallus had been proven after the fact to be correct regarding the age of Koonalda – Mulvaney points only to the credibility of his work on cognition from forty years ago. Because, after all, what threat could an individual pose (even after death), if their "data gathering" was deficient and all that remained to them was specious theory?

It is almost certain that Gallus was not only well aware of the disregard in which he was held by Australian archaeologists, particularly Mulvaney, but also the effect this regard had on his career. There seems to be considerable projection at play in Gallus' article in an Australian Carpathian studies collection of papers (Gallus 1976)- a publication outlet that he could probably safely consider his Australian archaeological peers would not read. Gallus opens this paper with the idealistic observation that given potential human development of "wider awareness" (Gallus 1976, p.130) the possibility of non-competitive existence is a valuable guide for any attempt on a global scale to arrive at a non-competitive cooperation of all mankind. Gallus here is firstly engaging in ideas that would have sounded to the politically minded suspiciously like communism – which is a surprising turn from an academic who fled Hungary to evade communist rule. Secondly, if the potential political overtones are removed, Gallus sounds distinctly like the proponents of utopian future who held sway in popular culture at the time. I sincerely doubt Gallus intended to be regarded in either of these ways, however, they are I believe demonstrative of his flexibility and idealistic nature of thought. As to his possible projection into this article of his likely perceived regard in the eyes of his Australian colleagues, I believe the following extract is very telling, overtly written regarding the work of English speaking historians work on the Carpathians:

"The source of their judgement is subliminal and the assumptions behind them are so deeply embedded in the culture and ethnocentric structure of the historian's Ego, that he cannot gain critical distance from hisethnocentric bias" (Gallus 1976, p.133).

Is Gallus here demonstrating and projecting that he is well aware of the attitudes of his Australian academic colleagues towards him as expressed in Mulvaney's judgemental article on him 20 years later? It is tempting to think so.

5.11 QUALITATIVE COGNITIVE DISPARITY

Gallus' son noted that his father wrote notes and almost certainly thought in four modern languages (Hungarian, German, English and French), from his citations in publications it is evident that he read Spanish too. It is interesting to analyse the frequency of cited works by language in Gallus' works. A selective sample is offered here from his Australian English language publications, which would indicate that apart from a predominance of English sources as would be expected for publication in works in English, Gallus was able to select the sources he desired from across language boundaries. For example, when publishing on stone tool technology his French citation rate is far higher due to his selection of works on the topic by Francois Bordes, than it is in Gallus 1976 where French literature is less relevant to his purposes (see Table 3 and Table 4 below).

Table 3: Citation frequency by language in Gallus 1976

Language	# cited works	% of total
English	42	62
German	10	16
Hungarian	5	8
Spanish	2	3

Table 4: Citation frequency by language in 1976A

Language	# cited works	% of total
English	32	58
German	9	16
French	13	24
Hungarian	1	2

It is evident from this that Gallus was not only competent but fluent in up to 5 modern languages. Gallus came of course from a part of Europe that was particularly predisposed to plurilingualism. Hungary had been part of the Austro-Hungarian Empire between 1867 and 1918, with lasting effect on the culture of Hungary including the prevalence of German

speaking ability within it. Having been born in Sopron near the Austrian border it is understandable that Gallus was fluent in German - but such plurilingualism was hardly unusual in countries with multiple international borders such as Hungary had and still has. Hungary currently shares borders with Austria, Slovakia, the Ukraine, Romania, Serbia, Croatia and Slovenia – the languages of many of which differ substantially. Further, Hungary measures only slightly over 200 kilometres from north to south - Hungarians sharing borders with one foreign language neighbour are never that far from compatriots sharing a border with a different foreign language, and as a result, the Hungarian population overall was historically polyglot (Arendas 2016). This is of course in strong distinction to the culture of monolingualism that prevailed then and still does prevail in island England and its colonial outgrowth, island Australia (Clyne 2008). Not only is monolingualism a symptomatic feature of society in these countries, but it has taken on the aspect of a defensive social bulwark against the encroachment of foreign culture and social values (Clyne 2008).

Recent research in the fields of psychology and psycholinguistics indicates that the inherent effects of bilingualism and certainly multilingual capability go well beyond the obvious abilities to read write and speak in these languages. There is almost certainly also an observable increase in cognitive capacity, particularly around long-term neural plasticity but also more relevant to this chapter, in the capability of the multilingual person to negotiate complex conceptual fields and to innovate in these complex concepts (Bialystok, Craik, & Luk 2012, pp.1-2). In his work on the prevalence of monolingualism, Clyne (2008) sets out in bullet points the three items that he perceives as the prime differentiating factors between the intellectual capacity of monolingual and plurilingual counterparts:

As they only have one set of representational symbols, monolingual children are less likely to understand the arbitrary nature of language than their plurilingual counterparts.

Monolingual children are less likely to think divergently than plurilingual children.

Monolingual children require more neural activity to perform the same operations as plurilingual children.

Clyne's article is a synthesis of works by prominent scholars in the field, notably Bialystok (Bialystok 1999, Bialystok, Craik, & Luk, 2012 Ricciardelli, 1993. Bialystok's studies had found that bilingual children outscored monolingual children in tests of symbol manipulation and conceptual problems that engaged higher level thought processes including "different forms of problem solving than previously used" (Bialystok 1999, p.637). That is to say, not only different solutions to problems, but entirely different approaches to how one goes about solving problems. Interestingly, Bialystok also found that the bilingual or plurilingual mind did not operate by switching off the inactive language / languages during conversation, reading or thought. Rather, at each point that the plurilingual mind sought a word, it had to actively suppress the constant awareness of equivalent terms in the language / languages not being used (Bialystok, Craik, & Luk 2012, p.9). The effect of this is that the multilingual mind lives in a constantly bubbling world of polyvalent alternative meanings, concepts and symbols - not only resulting from each of the languages that mind possesses, but also from the interplay, pun and mixed meanings that obtain from thinking across languages. This all sounds very much like a comparison between Gallus and his Australian counterparts. Between Gallus with his wide-ranging conceptual and innovating analyses that tended not to place huge emphasis on what he likely perceived as arbitrary empirical observations and categories, and on the other hand monolingual Anglo-archaeologists to whom there was one way, the empirical way, to approach archaeology- and anything else was 'intuitive' and specious.

5.12 HISTORY IS OFTEN PREDICTABLE IN RETROSPECT

Gallus's lack of recognition by the Australian archaeological academic mainstream, as exemplified by Mulvaney's mean-spirited retrospective of him (Mulvaney 1998) was in retrospect almost inevitable. Arriving in Australia as an already established and renowned scholar with two doctorates and an impressive multilingual publication record, Gallus had long been thinking and publishing along lines that were incompatible with the empiricist regime then in control at Cambridge, where most of the then current Australian archaeologists were trained. In 1983 Vincent Megaw could still claim that Graeme Pretty at the South Australian Museum was "one of the few Australian archaeologists to have an extensive knowledge of the historical and theoretical bases of European prehistory" (Megaw 1983, p.4). Perhaps these few such as Pretty and Megaw may have understood Gallus, whose views were almost certainly incompatible with the archaeological thinking of

someone such as Mulvaney, who, educated only to partial undergraduate level in archaeology, does not appear to ever have acquired a wider understanding of the philosophical and anthropological context within which much global archaeology (including postgraduate archaeology at Cambridge) was often practiced and could be interpreted and discussed.

It is worth observing that while Mulvaney has been described as some as the father of Australian archaeology, Mulvaney himself tended to refer to himself as a prehistorian - indeed this was the title of his position and then chair at the Australian National University. A prehistorian may engage in archaeology, but is more accurately described as someone who uses the techniques of history to interrogate the past for which no written records exist. Or as the distinction between the two drawn by David Clarke (Clarke 1978) is paraphrased by Taylor, 'archaeology is a study of artifacts and prehistory is a form of history made possible by archaeology' (Taylor 2008). Gallus' potential divergent thinking resulting from his multilingual abilities would only have added to the misalignment of understandings between himself and other Australian archaeologists. When one adds to this the enthusiastic amateur following and participation that Gallus engendered at the Archaeological Society of Victoria and at the same time the resistance to amateur archaeology from Australian academic archaeologists, we can well see in how many ways Gallus would not have fitted the community of Australian archaeology at all. This was after all the period of "Professionalisation of Australian archaeology" (Moser 1995) in which disciplinary boundaries became established.

I would differ with Moser (1995) in that I do not believe that the formation processes that Moser (1995) depicts are those of professionalisation - rather they are processes of self-definition and exclusion that could occur in any social group. I would contend that the amateur-professional divide between university employed archaeologists and others, is largely not a useful distinction. Rather, that professionalisation of the discipline of archaeology really only commenced with the development of professional (commercial) archaeology and the engagement by archaeologists with government to draft acts of state heritage legislation that would result in the capability of the commercial archaeology industry and government heritage offices to support increasingly greater numbers of archaeologists, working under legislated and enforced common standards of practice.

These numbers of commercial archaeologists have long been greater than have ever been employed in the comparatively miniscule academic archaeological world.

However, I would agree with Moser (1995) regarding the processes of group formation and inclusion and exclusion that we can observe at play in the treatment of Gallus by the Australian archaeological community. Moser goes to some length in depicting the attributes and formation processes likely to contribute to formation of a community of archaeologists. With particular regard to Australia, Moser notes that historically the Australian archaeological community acted to heighten the proportion of its members that were “white, male, middle-upper class and heterosexual” (Moser 1995, p.51). Gallus adds a further layer of differentiation to this so that the discriminant could now read “white, male, middle-upper class, heterosexual, Anglo and empiricist, elitist towards amateurs”

In the long run, Australian archaeology has increasingly become enriched through the contribution to it of archaeologists who do not fit any or all of the discriminant factors listed above. Women and LGBTQI archaeologists can and do now contribute to Australian archaeology from the highest levels down – although this is not to say that they do so free of obstacles and ongoing prejudice against them.

It is something of a tragedy for the discipline then that a scholar of the experience, brilliance and wide-ranging analytical capacity of Alexander Gallus was never able to transmit his knowledge and understanding within an Australian tertiary institution. In the end then, the loss experienced by Australian archaeology due to the strictures inflicted on it by its gatekeepers has been greater than just the inequity to Gallus – it has almost certainly resulted in a generation of Australian archaeology students not being exposed to a thinker well outside the ambit of the empirical doyens of the discipline. I mentioned earlier that Australian archaeology up to the early 21st century was largely devoid of theory. Perhaps had Gallus been given a “fair go”, Australian archaeology would be in a very different place now.

Chapter 6: Elsie Bramell

6.1 A FIRST AMONG FIRSTS

In the previous chapters I have discussed two individuals whose involvement with mainstream academic archaeology were limited. These limitations were due to very specific combinations of class, social standing or of foreign status and ideas that were unfamiliar to the gatekeepers of the emerging Australian archaeological community. The topic of this current chapter is Elsie Bramell (1909 – 1985) an Australian anthropologist and archaeologist who was at no disadvantage of class or migrant status. Her disadvantage was far more wide reaching, having been born a woman in an era when male privilege was accepted without question to an even greater extent than it is currently.

Elsie Bramell was forced to resign her role and relinquish her career as an archaeologist and anthropologist in 1940, when she married Frederick McCarthy (1905-1997) her less qualified colleague at the Australian Museum, Sydney. Under the Public Service Regulations of the time, married couples could not work together (Bowdler & Clune 2000). The little evidence available on Elsie makes it apparent that she would be a rich and rewarding subject of lengthy study. Unfortunately, what is available to me on her life and career is scant. Her remembrance has been primarily enshrined (if only briefly) in works by current female archaeologists.

I would not have been aware of Elsie were it not for prompting by my supervisor Annie Clarke. Annie reminded me that the monograph *Women in Archaeology A Feminist Critique* (duCros & Smith 1993), had been dedicated to Elsie, and that Dr Hilary du Cros (duCros 2002) made several mentions of Elsie in her overview of Australian archaeology. My own gleanings that I carried out through newspaper and journal archives for evidence relating to Elsie would not have resulted in the understanding of Elsie's life and place in archaeology that I have obtained, without the kind generosity of Hilary du Cros and the Australian Museum. Dr du Cros kindly lent me a copy of her honours thesis *Skeletons in the Closet. A history of the prehistoric archaeology of N.S.W. (c1890-1940)* (duCros 1983). As part of her Honours thesis research, Dr du Cros had interviewed Elsie Bramell and her husband Fred

McCarthy at length. The tapes of these interviews were deposited with the Australian Museum. Under then current (2021) coronavirus travel restrictions it was not possible for me to listen to these recordings at the Australian Museum. The Museum and Dr du Cros kindly cooperated to transfer these tapes to electronic format and to provide these recordings to me. The historical content contained in these recordings may not have greatly and directly informed my understanding of the trajectory of Elsie's career. Nevertheless, they provided me with approximately one and a half hours of listening to voices, interpersonal dynamics, understanding of relationships and an ambience which has contributed greatly to this chapter. I refer to these recordings as (duCros 1983a).

6.2 FAMILY BACKGROUND AND SCHOOLING

Elsie Bramell was born in 1909 in Port Moresby New Guinea (Oakes, Meehan, & Bolton 1985). It is worth considering here whether, and at what points, her life could be considered one of privilege. Certainly, during her early childhood in New Guinea, her life would have been privileged compared to that of the native New Guinean. But the question arises whether Elsie and her family would ever have measured themselves by a common socio-economic yardstick to those Indigenous peoples around them.

When Elsie was born, her father Bertram Bramell (1867-1938) had been an officer in the British New Guinea and Papua Services since 1897 (Lewis 1996). In 1910, the year after Elsie's birth, Bertram Bramell was appointed as Commissioner for Native Affairs with responsibility for policing the indentured labour system. His position was not popular with plantation owners as he and his officers were entitled to enter all places where Papuans were employed and to prosecute employers for breaches to regulations of employment conditions.

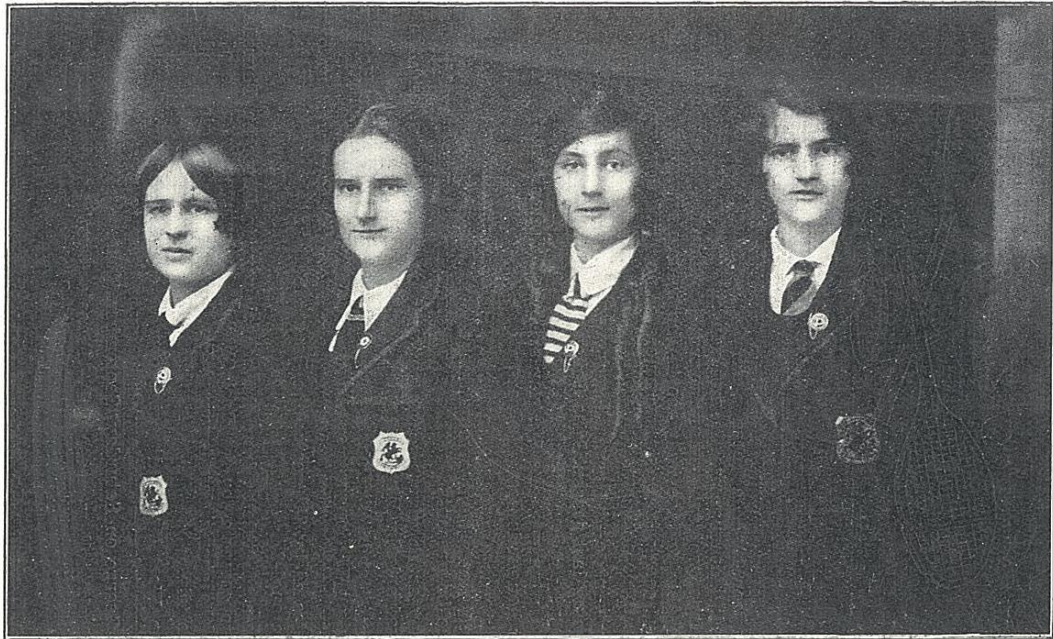
Bertrand Bramell and his superior Sir Hubert Murray held progressive views regarding the governance of the local population. They were both of the opinion that punitive regulation of the Papuan workforce was counterproductive and unwarranted. This attitude was ridiculed by plantation holders as misguided and overly 'soft' towards the labourers (Lewis, 1996 p.278). Bertram eventually resigned his position in 1922. I bring up this background of Bertram Bramell, as I intend to later explore the extent to which Bertram may have been a powerful role model for his daughter Elsie who went on to carry out an MA on the topic of

governance in New Guinea, adopting what seems to have been similar attitudes to those of her father.

Elsie's father then was a highly placed but disliked public official in a world in which much real power and wealth lay in the hands of rich plantation holders. Seen in this light, the Bramell's position in New Guinea could hardly be seen as privileged relative to plantation society. This tension between her father and the empowered elements of white New Guinean society does not seem to have tainted Elsie's childhood recollections of New Guinea - she is described as remembering her time there 'with great affection' (Oakes, Meehan, & Bolton 2020). Perhaps at the time Elsie left New Guinea for school in Australia, she may have been too young to have sensed tensions around her father's standing in local colonial society.

It is uncertain at what age Elsie left New Guinea and her father for schooling in Australia. It is similarly uncertain whether Elsie was accompanied to Australia by her mother Ada Blanche Bramell (nee Skelly) (1886-1970) (of whom I have not been able to identify more information than her name, and that she was nearly 20 years younger than her husband Bertram). Elsie first attended Morven College, Moss Vale, and then St George High School in Kogara (Oakes, Meehan, & Bolton 1985). I have not found other evidence to support my speculation that Elsie's transfer from a private school in the north shore of Sydney (Morven College) to a selective government school in south west Sydney (St George High School) may have resulted from a reduction in the family finances. The timing of events might cohere with this notion, given that Bertram Bramell had retired his public service position in 1922, the same year that Elsie would have commenced high school. Regardless, Elsie seems to have flourished, and was elected St George High School Captain for her final year of studies in 1927. Her end of school portrait shows Elsie second from left below:

Figure 15: St George Prefects and School Captain. St George Yearbook 1927. Elsie Bramell is second from left (Courtesy of the St George High School archives)



PREFECTS, 1927.
Left to Right:—A. Thomas, E. Bramell (Captain), D. Moore, M. Corish.

6.3 ELSIE BRAMELL AND ANTHROPOLOGY AT THE UNIVERSITY OF SYDNEY

After matriculation, Elsie enrolled at the University of Sydney. While her eventual degree resulting from this study was a B.A. Dip Ed (Oakes, Meehan, & Bolton 1985) Elsie also studied anthropology during this period. At this time the Department of Anthropology at the University of Sydney was young and its future was uncertain. Some context is necessary here not only to provide background to the studies of the undergraduate Elsie Bramell, but also to sketch the nature and calibre of teaching staff and fellow students to whom she was exposed.

The key figure in the foundation of anthropology at the University of Sydney was Grafton Elliot Smith (1871-1937). He travelled to Australia on behalf of the Rockefeller Foundation of the USA to advocate for the establishment of a chair in anthropology at the University of Sydney. The proposal that Grafton Elliot Smith put forward was that the Rockefeller Foundation would provide five years funding to the university, with the aim of training fieldworkers and administrators capable of operating in New Guinea. Grafton Elliot Smith's proposal was successful and in 1925 Alfred Reginald (A.R.) Radcliffe-Brown (1881-1955)

took the first position as head of department, assisted by Raymond Firth (1901-2002) as a lecturer. From its earliest then, anthropology at the University of Sydney was focused on New Guinea, with a particular emphasis on governance (Elkin 1970, p.252).

Elsie Bramell commenced her undergraduate studies at the University of Sydney in 1928, under Radcliff-Brown and Raymond Firth. In that year Radcliffe-Brown appointed an independently wealthy young British woman with a PhD from Cambridge as a temporary lecturer in anthropology at Sydney University. This was Camilla Wedgewood (1901-1955) of the Wedgewood crockery dynasty (Wetherill 2006)¹⁹. By 1930, the first five-year Rockefeller grant had expired and uncertainty returned to the future of anthropology at the university. Radcliffe-Brown left Australia in 1931, and his assistant Raymond Firth was appointed Acting Professor (Elkin 1970, p.257). The lack of clarity surrounding the future of anthropology at the University of Sydney, in particular as evidenced in the position of 'Acting' rather than full Professor discouraged Raymond Firth, and he left Australia in 1932 (Elkin 1970, p.260). Adolphus Peter (A.P.) Elkin (1891-1979) was subsequently invited to the position of 'Lecturer in charge' for a one-year period (1933). Fortunately, in that year (1933) Commonwealth government commitment to ongoing funding was received. In the following year (1934) Elkin was appointed Professor for a five-year term and in 1936 was granted a full-time lecturer to assist with the teaching load.

That fortunate year of 1933, in which ongoing funding for the anthropology department was secured, is the opening scene for Elkin's obituary of one of his former postgraduate students, Phyllis Kaberry (1910-1977) (Elkin 1978), which commences:

¹⁹ Camilla Wedgewood was notably of independent mind. Her career has been summarised in the Australian Dictionary of Biography "In 1928 Professor A. R. Radcliffe-Brown appointed Wedgewood temporary lecturer in anthropology at the University of Sydney, to replace Bernard Deacon, who had died at Malekula, New Hebrides. Instead of pursuing her own research, she accepted the self-effacing task of editing Raymond Firth's *Primitive Economics of the New Zealand Maori* (London, 1929) and *Deacon's Malekula* (London, 1934). Wedgewood lectured (1930) at the University of Capetown, South Africa, and was assistant lecturer (1931-32) under Bronislaw Malinowski at the London School of Economics and Political Science. She was a fellow (1924) and council-member (1931-32) of the Royal Anthropological Institute of Great Britain and Ireland". (<https://adb.anu.edu.au/biography/wedgewood-camilla-hildegard-11992>).

My post-graduate seminar in 1933 included three M.A. students: Phyllis Kaberry, Elsie Bramell and W.E.H. Stanner, and among visitor-participants (in second term) Gregory Bateson, Margaret Mead and Reo Fortune.

6.4 ELSIE'S LECTURERS AND COHORT

As evidenced by the list of her faculty, and also her peers listed by A.P.Elkin, Elsie Bramell studied under and among some of the giants of Australian anthropology, and indeed anthropology world-wide. Her undergraduate professor was Radcliffe-Brown. Radcliffe-Brown and Bronislaw Malinowski are considered the two founding figures of modern anthropology, and Radcliffe-Brown went on to take the chair of Anthropology at Oxford University (Hogbin 2006). Lecturing Elsie Bramell under Radcliffe-Brown was Raymond Firth, who after leaving Australia in 1932 had a long tenure as Professor of Anthropology at the London School of Economics, succeeding Bronislaw Malinowski. Raymond Firth was knighted in 1973 for his contribution to anthropology, which included effectively creating British economic anthropology as a discipline (Young 2003).

A.P.Elkin's star may not have shone as brightly as his predecessors at the department, however his dedication to his students and to the discipline along with his consistent high level of output make it evident that the department remained in highly capable hands. Elkin not only worked at the university, but he also died there too at a function at International House in 1979 (Wise 2006).

Elsie's Australian enrolled colleagues were of brilliant calibre too. Probably her classmate best known for his work in Australia in the same field as Elsie's eventual speciality of Aboriginal archaeology and anthropology, was William Edward Hanley (W.E.H.) Stanner (1905-1981). W.E.H. Stanner was four years older than Elsie and had come to university through personal and financial struggle. W.E.H. Stanner's father was a cook who had died when Stanner was three years old, and W.E.H. Stanner could not afford to stay at school in order to matriculate. He took up employment as a bank clerk and later as a fairly successful journalist, while studying privately for his matriculation. In 1926 W.E.H. Stanner was captivated by a Sydney Arts Society lecture by Radcliffe-Browne. W.E.H. Stanner entered university in 1928, won prizes in anthropology in 1929 and 1930. He graduated with a BA in 1932 and MA 1934 with first class honours, followed by a PhD at the London School of

Economics. Although military duties intervened in his career, W.E.H. Stanner went on to become a leading figure in Australian Aboriginal anthropology, and an advocate for Aboriginal rights with his 1968 Boyer lectures on the topic being frequently republished (Mulvaney 2012).

Phyllis Kaberry (1910-1977) was slightly younger than Elsie Bramell, highly independent and adventuresome from childhood. Phyllis graduated with a BA in anthropology in 1933 and won first class honours for her masters thesis. At university she joined the radical philosophical Freethought Society spearheaded by Professor John Anderson. Phyllis Kaberry took out her PhD from the London School of Economics on a gender topic, published as *Aboriginal Woman : Sacred and Profane* (Kaberry 1939) in which she explored the independent contribution of Aboriginal women to communal physical and ritual spheres of achievement, dispelling the notion that Aboriginal women largely played a tangential role in these aspects of traditional Aboriginal life. Much of her later research was in Africa, and by 1950 Phyllis Kaberry was reader in anthropology at University College London where she remained until her death due to 'acute alcohol poisoning by misadventure' in 1977 (Cheater 2006).

Understanding the level of effort and dedication that is evident, in addition to inherent talent and intelligence displayed by Elsie Bramell's role models- her instructors and peers, is particularly relevant in evaluating the drive and initiative that Elsie brought to her short but brilliant career. This context does not seek to diminish the extraordinary achievements of Elsie Brammell by positioning them as ordinary or normative among her peers of the time. On the contrary this context recognizes that Elsie not only held her place but excelled in a cohort of colleagues who went on to become world renowned scholars, while studying under professors and lecturers who were foundational figures in their anthropological fields.

Having just introduced several of Elsie's peers, it is worth examining here to what extent the decision to abandon her career in anthropology and archaeology might have been a deeply personally upsetting one to Elsie, or to what extent on the other hand, it may have been a pragmatic acceptance of the actions required to fulfil the then-normative social expectations of marriage, parenting and domesticity – in that order. I find it unlikely that someone of the mental acuity of Elsie Bramell would not have clearly foreseen and

evaluated the alternative life trajectories available to her, of companionship and family life on the one hand or single 'spinsterhood' on the other. Even where public service rules did not apply, such as in academic settings in separate institutions or perhaps overseas, prejudices against women continued and certainly pregnant women were not deemed suitable participants in the workforce generally. Perhaps a brief overview of the career paths of Elsie's peers may provide illumination to some of the considerations which might have weighed on her thinking.

6.5 WOMEN, ACADEMIA AND RELATIONSHIPS IN THE EARLY TWENTIETH CENTURY

An illustration of the difficulty facing women in academia during the early-mid 20th century can be gained from a brief overview of the careers of several of Elsie Bramell's contemporaries and their apparent difficulties in forming lasting relationships. It has to be stated at outset however, that the historical record almost certainly occludes whether some of these women did find satisfying personal relationships, in that it is only legally enshrined heterosexual relationships that tend to be recorded.

Phyllis Kaberry has been briefly mentioned above. Her life and career is explored at greater length in a short book by Sandy Toussaint (Toussaint 1999). While Toussaint adds little information on the time that Kaberry spent with Elsie Bramell at university, Toussaint does add personal insight, particularly to the sexual dynamics of academic and field work as perceived by Phyllis Kaberry. Kaberry is depicted as "determined and high spirited" (Toussaint 1999, p.9) an irreverent heavy smoker with little interest in presentation or pretention, and someone who in later years suffered from increasing bouts of depression and heavy drinking (Toussaint 1999, p.20). Toussaint speculates whether this decline in mental health was due to a combination of successive failed relationships and looming retirement. Certainly, retirement weighed heavily on Phyllis Kaberry's mind, and it seems likely that the prospect of a lonely and unproductive existence was one that bore significantly on her mental health. Her loneliness is reflected in a long-distance phone call that she placed on 30 October 1977, the night before her death, to one of her few remaining friends, Mary Durack, in Perth, Australia. Despite Mary Durack's protest at the cost of the call, Phyllis implored Mary to simply keep talking (Toussaint 1999, p.26). Phyllis had already described her despair at the thought of retirement to Mary Durack in a 1972 letter: *The thought of retirement and not being able to write has filled me with gloom, really despair* (Toussaint 1999, p.27).

Perhaps it is the case that the sacrifices made by an enthusiastic young Phyllis Kaberry had come home to haunt her in later life. Phyllis Kaberry had ventured into anthropological and academic practice in an era when despite the achievements of notable individual women, significant challenges faced women generally in academic disciplines. This again is frankly disclosed by Phyllis Kaberry in a letter to Mary Durack dated to 1945, and in her early decades of professional practice:

My own make-up is particularly suited to fieldwork. Fundamentally I am of course feminine and I don't go round looking as though I have a spanner in my hip pocket. But I have the faculty of being able, as it were, to put my sex into neutral gear and to conduct my relations on that basis in the majority of instances. Perhaps it is a bit abnormal, but when I meet men its only occasionally I am aware of them physically. (Toussaint, 1999 p.23)

It is noteworthy here, that even as an anthropologist Phyllis Kaberry equated femininity with heterosexuality. Such may have been the predominant view of the time. While such views are no longer contemporary it is still the case however that professional conduct between peers requires that sexual drives not be expressed in the workplace. I believe however that Phyllis Kaberry is expressing here a far more powerful perspective, namely the view that it was for her an advantageous personality trait, to be capable of suppressing her own sexual identity and awareness while in the field. It is not as if Phyllis Kaberry was a cold and emotionally detached person. She is quoted by Raymond Firth as having stated:

Anyone who engaged in practical fieldwork was a fool; it was a constant wear on one's emotions. But 'if one puts emotions into cold storage then it means also an absence of a really vital relationship with the people' (Firth 1978)

To be aware then of the potential for relationship, and to suppress such potential at the same time may have been a process which led Phyllis Kaberry to great academic success yet saw her sacrificing potential emotional relationships to the cause of her research and career.

Camilla Wedgewood was an honorary lecturer at the University of Sydney at the time that Elsie Bramell and Phyllis Kaberry were students there. A P Elkin described her as 'unsurpassed as a lecturer and teacher' (Elkin 1956). Less than a decade older than her students, Camilla Wedgewood likely provided a powerful role model of potential achievement in this early era of women's entry to academia. Wedgewood was not able to secure permanent academic positions, lecturing at the University of Capetown and later the London School of Economics. Her longest tenured position in a quasi-academic role was her eight years as principal of Women's College at the University of Sydney. A P Elkin noted, that 'behind her apparent self-confidence however, she was somewhat retiring and lonely' (Elkin 1956, p.177). Through her life Camilla Wedgewood was not known to have formed romantic relationships (Wetherell & Carr-Gregg 1990).

The last of the women who studied with Elsie Bramell at the University of Sydney, and on whom there is some detail relating to personal life and career trajectory is Margaret Mead (1901-1978). Although only a visiting student in 1933, Mead's contact and contemporary status with Elsie Bramell also allows for some comparison. Margaret Mead graduated her bachelors from Barnard College, New York and went on to complete masters and doctoral degrees at Columbia University. She came from an academic background – her father was a professor of finance at the University of Pennsylvania, her mother had studied sociology and Mead could claim of herself that she grew up in a 'social science' family (Fortes 1979). At the time of her death Margaret Mead was undisputedly the most influential living anthropologist. Astonishingly prolific, her publication list ran to over 4,000 items. She worked at the American Museum of Natural History from 1946 to 1969. She was an adjunct professor at Columbia University from 1954 to 1978. At the same time, she was founding professor of anthropology and chair of the Division of Social Sciences at Fordham University from 1968 to 1970. In 1970 she took the position of Distinguished Professor of Sociology and Anthropology at the University of Rhode Island. Margaret Mead served as president of the Society for Applied Anthropology in 1950 and of the American Anthropological Association in 1960.

Through this time Margaret married several times. At first (1923-1928) to her high school sweetheart Luther Cressman (Cressman 1983), subsequently (1928-1936) to Reo Fortune, a New Zealander whom Margaret had met in 1926 while returning from fieldwork in Samoa (Library of Congress 2001), and lastly (1936-1950) to Gregory Bateson with whom she had a

daughter, Mary Catherine Bateson (Howard 1984). Mead's third husband Gregory Bateson left Mead in 1947 before their divorce in 1950. Mead was devastated and apparently kept Bateson's photograph at her bedside, to her death. Margaret Mead lived and worked with the anthropologist Rhoda Metraux from 1955 to 1978. Margaret Mead's daughter Mary Catherine Bateson allowed publication of correspondence between Margaret Mead and Rhoda Metraux, from which it is fairly obvious that a romantic relationship existed between them (Caffey & Francis 2006). I do not mean to impute that Margaret Mead did not live a full and happy life as a result of these broken or short-term relationships - or even that they were not fulfilling in themselves. Nevertheless, given the social mores of the early to mid-20th century, wider society would almost certainly have regarded askance the multiple marriages of Margaret Mead. Her individual fortitude and strength of character may have been such that she was able to ignore public opinion. Regardless, some taint of public judgment is almost certain to have affected her inner equilibrium.

In summary then, we have four young women, almost of an age, who studied or were together at the University of Sydney in the late 1920s and early 1930's. Elsie Bramell went on to a short lived but brilliant anthropological and archaeological career which we will shortly explore, followed by an enduring and apparently happy marriage. Her contemporaries however, Phyllis Kaberry, Camilla Wedgewood and Margaret Mead continued their stellar academic careers with only limited if any lasting romantic personal relationships being formed in their lives within the social norms of marriage predominant at the time. The longest lasting of such relationships identified here is the 11 year relationship of Margaret Mead and Gregory Bateson (married 1936, separated 1947, divorced 1950). I must emphasise again that the point here is not to attempt to judge any of these women by the quality or durability of their romantic connections. Rather, the point is to attempt to gain better insight to Elsie Bramell's thinking in her leaving academe altogether for what proved to be an enduring marriage.

I can only presume that Elsie would have been aware to some extent of the earlier generations and first women of Australian archaeology - the majority of whom were not university trained, or certainly not trained in the field of anthropology itself. Reading Julie Marcus' work on the topic *First in their Field. Women and Australian Anthropology* (Marcus 1993), there is an almost monotonous regularity to which Marcus' preface biographical summary of each female anthropologist reads along the lines of 'Born - xx; Died - xx;

Married - never; Children – none.’ If we overlook the tragic four-month marriage of the widowed Mary Ellen Bundock, then of the six case studies in Marcus’ work, five women never married or had children. This statistic is somewhat lessened by the fifth case study - that of Daisy Bates who was bigamously married to two men and who had one child. These six women - Mary Ellen Bundock, Daisy Bates, Jane Ada Fletcher, Ursula McConnel, Olive Pink and Phyllis Kaberry may not have overtly engaged in longstanding and conventional romantic relations with men, nevertheless their professional lives were almost totally dependent on subservient relationships with men, in their need to obtain patronage and approval from the men who dominated the discipline so totally (Marcus 1993, p.7) and through their need to negotiate their every movement relative to this self-entitled ruling ‘brotherhood’ of male academics (Marcus 1993, pp.4-5).

Despite the evidence I have provided here, of my own observations and those by Julie Marcus, I may well have the cart before the horse. Given the small statistical sample pool available to me, it may not be the case that involvement in academia was the chief factor which worked against the abilities of these young women to form the sorts of heteronormative relationships that could be publicly acknowledged at the time. It could equally well be the opposite - that academia was a powerful attractant to the sort of person who may have struggled to form relationships that were of the sort then deemed socially normative - or who may not have been attracted to the notion of such relationships in the first place. The historical sources available to me at this point simply do not provide sufficient information for a potential queering of early female Australian anthropological and archaeological history - yet it would be unreasonable to assume that this was not at least partially the case, or that such a history could not be researched and written.

6.6 THE AUSTRALIAN MUSEUM

In 1933 Elsie Bramell was at something of a loose end, approaching her graduation with a B.A. Dip Ed, specialising in anthropology (McKinnon 2021). She had been offered a lecturing position at the University of Sydney however, confirmation of this role was considerably delayed. Her previous supervisor at the University of Sydney, Raymond Firth, suggested that she apply for the position of Assistant Ethnologist at the Australian Museum which had become vacant with the passing of W.W. Thorpe (duCros 1983a). Elsie Bramell was evidently a highly competent and motivated young woman - the previous year, in August 1932, during her undergraduate degree she had delivered a paper to the Australian and

New Zealand Association for the Advancement of Science (ANZAAS) on *Government and Justice in New Guinea* (Anon, 1932). Elsie was successful in obtaining the role of Assistant Ethnologist at the Australian Museum, commencing on 7 November 1933, a position which fatefully came with the legacy of a colleague Frederick McCarthy who was three years older than Elsie, but who for all his enthusiasm and experience cataloguing books and objects in the museum collection, had no formal tertiary education at all. Elsie went on to complete her masters degree in Anthropology in 1935 while working at the museum. At the same time her assistant Frederick McCarthy attended university for the first time himself to eventually graduate in 1935 with a Diploma in Anthropology.

In a moment I will turn back to Elsie Bramell and her achievements at the Australian Museum, but first it would be proper to spend a few words briefly introducing her husband-to-be, Frederick David (Fred) McCarthy, who preceded Elsie at the Australian Museum:

Fred McCarthy (1906-1997) was born to working class parents in Petersham, Sydney. A neighbour of the McCarthys worked at the Australian Museum as a carpenter and he advised Fred McCarthy that a position for a Library Clerk had come available in the Museum Library. Fred McCarthy applied for the position successfully, and commenced work on 23 March 1920 as a fourteen year old. In 1930, after 10 years in the library, Fred McCarthy transferred to the Department of Birds and Reptiles for 18 months, adding taxonomy to his existing cataloguing skills (Khan 1993). The Museum appeared to operate with a fair proportion of unqualified staff.

The Curator of Ethnology was W.W. Thorpe who had been appointed Ethnologist in 1906 with little education and no formal training at all. In 1932, Fred McCarthy was offered the position of Assistant Curator of Ethnology to W.W. Thorpe (Bowdler & Clune 2000). Fred McCarthy accepted this offer and only a few months later W.W. Thorpe died leaving not only Fred McCarthy without leadership, but also leaving the Museum Ethnology collection in a parlous state. W.W. Thorpe had resisted any efforts at all to catalogue the Ethnology collection, insisting that he knew the whereabouts of all collection items (Khan 1993). It is perhaps unsurprising that Thorpe's opinion not to catalogue items carried the day and the museum ethnology collection remained largely uncatalogued to the time of Thorpe's death.

It is at this point that Elsie Bramell enters the Australian Museum, quite obviously from a very different background and with different interests to those of her predecessor W. W. Thorpe, and also those of her colleague Fred McCarthy. Elsie's interests lay in anthropology, specifically social anthropology and the analysis of the manner in which societies structured and governed themselves. This had been the field of specialisation of her lecturers at the University of Sydney and formed the basis of her undergraduate and later masters study on the nature of Indigenous self-government in New Guinea. In distinct contrast, W.W. Thorpe had pursued a cabinet of curiosities approach at the museum, and his assistant Fred McCarthy, fresh out of ten years cataloguing books in the museum library, appears to have been most concerned with cataloguing and creating typologies for the objects in the museum collection. Such is certainly the impression gained from the numerous times to which Fred McCarthy returns in his taped interview, to the theme of cataloguing and typology and the primary need for such activities (duCros 1983a).

In distinct contrast, from her first taking a position in the Ethnology Department at the Australian Museum, Elsie Bramell embarked on a public speaking campaign on behalf of the museum, and was one of the first to give radio talks on the Museum's anthropology interests (McKinnon 2021). This campaign was unmatched by her predecessor, and certainly by Fred McCarthy her successor. To provide some idea of the publicly noted nature of this speaking campaign I have summarised those speeches that I have identified in Table 5 below. I have no doubt that other talks that I have not been able to identify were delivered by Elsie Bramell in this period. It is noteworthy that Elsie targeted a very wide range of audiences, from the David Jones business girls' lunch to talks at the Australian Museum itself. This certainly seems a period in which Elsie Bramell not only was the voice of the Australian Museum but also sought to extend and redefine its audience and clientele.

It would, for example, be fascinating to gain further insight to Elsie's addressing the Feminist Club, and on the subject which she chose – the lack of any feminist rights among New Guinean women. The Feminist Club of NSW (1914-1970) was formed to work for 'equality of status, opportunity and payment between men and women in all spheres.' The group concerned itself with a broad range of issues, including child welfare, adoption, divorce laws, women's influence in politics and 'Aborigines' (Griffith 1988). Was Elsie herself a feminist who in this speech portrayed the plight of New Guinean women to her

sympathetic sorority? Or was she perhaps quite the opposite - not a feminist and someone who portrayed New Guinean gender relations as simply another form of life which needed to be respected as it was, and not interfered with by Western reformers? I have not found evidence that would solidly support either proposition. On the one hand Elsie Bramell was certainly a highly motivated, independent and capable young woman who might be considered a likely feminist. On the other hand, Elsie was the only one of her cohort to leave anthropology for a stable married life – and perhaps saw traditional marital and patriarchal values as, if not predominant, then perhaps inevitable.

It would be greatly beneficial were we able to consult Elsie's master's thesis to gain greater insight to her thinking on such topics of society and gender as I have raised above.

Unfortunately, her thesis is not to hand, despite queries with university libraries, the Australian Museum, and Elsie's descendants and I am only aware that her thesis was on the topic of 'Native Law in New Guinea' (Bowdler & Clune 2000, p.29). Some inkling of Elsie's general egalitarian and anthropological attitudes can be gained from the topics of her public talks listed below in Table 5. Further evidence is available from a selection of the short articles published by her in the *Australian Museum Magazine*. The first (Bramell 1933) depicts in popular but empathetic terms, life in Papua, while in the second (Bramell 1935) examines 'Law and Order Among the Australian Aborigines', reaching the forceful conclusion that Aboriginal society was highly ordered and lawful.

Table 5: Identified public speeches by Elsie Bramell 1933-1940

Date	Location	Topic / Title	Advertising entity
26/10/1933	David Jones business girls' lunch	GRISLY Headhunters of New Guinea	<i>Sun</i> (Sydney) p28
30/1/1934	The Sun Newspaper offices	Lecture on New Guinea	<i>Sun</i> (Sydney) p6
28/5/1934	Australian Museum	Magic among Primitive peoples	<i>Education</i> Vol. 15. No. 7, p3
8/8/1935	Australian Museum	Talk on Australian Aboriginal dueling	Sydney Morning Herald, p10
9/8/1935	Australian Museum	Law and Logic	Daily Telegraph (Sydney), p6
11/12/1935	Feminist club	NOT FEMINISTS. Island Women HAVE NO STATUS	<i>Sun</i> (Sydney), p23
21/5/1937	Australian Museum	Native dance and dreams	<i>Education</i> Vol. 18. No. 7, p3
24/8/1937	Sydney YMCA	The story of money	Sydney Morning Herald, p5
1/10/1937	Radio talk	A Papuan club house	<i>Wireless Weekly</i> Vol. 30. No, 14
24/5/1938	Australian Museum	Seafarers of the South Seas	<i>Education</i> . Vol.19. No. 7, p631
20/5/1939	Australian Museum	Warfare in the Western Pacific	<i>Education</i> . Vol.20. No. 7, piii
21/5/1940	Australian Museum	Primitive Dress and Ornament	<i>Education</i> . Vol.21. No. 7, p224

It is notable from the items tabulated above, that a fair degree of attention was paid by the tabloid press to Elsie and her public speaking. Elsie's appointment was certainly noteworthy - at the young age of 24 she had been appointed to a senior position in the oldest museum in Australia. Further, her specialties included intimate and at times personal knowledge of topics that appealed greatly to sensationalist press - the world of head hunting and otherwise exotic New Guinean 'primitive' activities. The fact that Elsie was a young and attractive woman only enhanced the sensationalist value that one senses these tabloid papers sought to extract from articles on her.

These articles were frequently inherently patriarchal and patronising as in the example below (Figure 16) which refers to Elsie as a 'mere woman' and sensationalises the fact that someone so young and female should have knowledge of 'Native Culture'. Despite this, the articles' author does clearly convey their impression that Elsie Bramell was eminently capable of fulfilling the position to which she had been appointed. I would have to suspect that any negative prejudices held by the articles' author may not have survived the realities of an interview with Miss Bramell, whose determined gaze meets us from across the decades, preserved in these newspaper articles.

Figure 16: *Sun (Sydney) 21 May 1933 P 26*

<p>UNIQUE JOB HELD BY WOMAN</p> <p>She Knows About Native Culture</p> <p>THIS distinction of being the only woman applicant for the position of assistant ethnologist at the Australian Museum belongs to Miss Elsie Bramell, who was duly appointed to her unusual post by the Museum authorities, who had never considered a mere female in such a position before. She is the only woman ethnologist in an Australian museum to-day.</p> <p>This youthful scientist, who has a B.A. degree and a Diploma of Education from the Sydney University, with her M.A. looming close, fills the position made vacant by the death of Mr. W. W. Thorpe last year, who has been senior ethnologist for many years.</p>	<p>A working knowledge of objects from bushrangers' pistols to Egyptian Scarabs is an unusual enough possession for anyone, but the most remote "antique" holds no terrors for Miss Bramell, who efficiently copes with all inquiries on such matters and thousands of others as part of her daily routine.</p>  <p>Miss Bramell</p> <p>and extends to archaeology of ancient Egypt, and the physical history of man, not to mention numismatics, most of which subjects are closed books to the average person.</p>	<p>Apart from her busy routine work of cataloguing, and the hundred and one tasks that come her way each day, Miss Bramell has to arrange and give lectures to University and College students when called upon, and has also to write articles on scientific matters for publication.</p> <p>Her early interests in social anthropology commenced in Papua, where she was born, and where her father, Mr. B. W. Bramell, was Commissioner for Native Affairs for many years.</p>
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Much the same surprise and patronising attitude was voiced in news columns patently written by women and for women. The section *A Woman's Letter* in *The Bulletin* of March 21, 1934, makes much of Elsie's youth and describes archaeology as a subject with which 'the young scientist toys'.

Figure 17: *A Woman's Letter* in *The Bulletin* of March 21 1934, p 36



In 1935, Elsie Bramell graduated with a masters in anthropology from the University of Sydney on the topic of Native Law in New Guinea (Bowdler & Clune 2000, p.29) and in the same year became a member of the Anthropological Society of NSW (McKinnon 2021). In 1935, too her colleague Fred McCarthy graduated with a diploma in anthropology from the University of Sydney.

Throughout the time that Elsie Bramell continued her speaking on her intellectual interests of social anthropology and New Guinea - the area of her masters degree, her employment at the Australian Museum required an involvement with and awareness of Australian archaeology well before it became established as an academic discipline. Together with Fred McCarthy, she took the burden of cataloguing and establishing typologies for the large existing collections that were to that period largely amorously dumped in storage or in poorly labelled displays (duCros 1983a). In addition to this, amateur excavations and collectors required management, as did the mounting of displays and exhibitions.

I have previously mentioned that Elsie's father Bertram Bramell held what were at the time progressive views regarding the governance of Indigenous New Guinean and had expressed the need for them to be allowed self-governance to some extent (Lewis 1996). Certainly, as illustrated in her choice of masters topic and her frequent talks on New Guinea, it is evident that the region of New Guinea formed an ongoing central motif in Elsie's mind. Little information is available on the content of Elsie's talks on New Guinea, most newspaper advertisements simply provide notice of upcoming engagements or provide salaciously worded titles relating to her proposed content.

One exception to this is an article in the *Yass Tribune-Courier* (28 October 1935, p1) entitled 'Primitive Peoples. Ways of Meeting (sic) Out Justice'. The article reports on a speech given by Elsie Bramell at the Australian Museum and describes Elsie's depiction of "Native Law" as a valid code of governance that 'had been evolved to suit the requirements of the society it served and was extremely effective within its own sphere'. On reflection it is unlikely that a trained anthropologist would have expressed opinions drastically variant to these- and perhaps Elsie Bramell is speaking here less under the direct influence of her father, than under that of her anthropological instructors. Nevertheless, Bertram Bramell's attitudes were unpopularity progressive at the time that he voiced them from 1897-1920 and it is likely that his interest in New Guinea's Indigenous affairs and the wellbeing of Indigenous New Guineans formed a foundation for his daughter's later study of New Guinean anthropology in the first place.

6.7 ELSIE BRAMELL AND FRED MCCARTHY

Little evidence is available to me on the nature of the working relationship between Elsie Bramell and Fred McCarthy at the Australian Museum. Presumably their relationship was sufficiently cordial that their marriage resulted from it. Nevertheless, the image below (Figure 18) poses some questions to the understanding of the relative positions of Bramell and McCarthy within the museum. This image was taken in 1933, the first year of Elsie Bramell's employment at the museum as assistant curator of Ethnology, possessed of a bachelors degree, enrolled in a masters degree and in the company of her as-yet totally unqualified colleague Fred McCarthy.

The body language evident in this photograph does not reflect this set of relationships, of Elsie as significantly better qualified and therefore in formal terms of the times higher

ranked than Fred. McCarthy lounges casually reading perched on the corner of the desk resplendent in waistcoat. Elsie Bramell on the other hand wears a coat. Was it a dustcoat to protect her white dress? Was she simply cold? Or perhaps was her dress sleeveless or otherwise unsuitable for a formal photograph? Elsie is perched on what looks like a stool and is ready to take notes on what appears to be an accession card from the stack next to her. McCarthy is positioned physically above Elsie Bramell and it is difficult not to read into this an imputed statement as to who held real authority in the room. Needless to say, this photograph is staged, but even so, there are limits to the extent to which the realities of professional and personal power relationships could be stretched in asking individuals to pose in manners which contradict the real power dynamics at hand.

Figure 18: Elsie Bramell and Fred McCarthy in the Australian Museum Anthropology Department 1933. Photographer Anthony Musgrave. Australian Museum Archives AMS514_VA180_8. Reproduction Rights Australian Museum



It may well be the case that in encountering the massive backlog of material to be catalogued that had accumulated at the museum during W.W. Thorpe's tenure, the skills of cataloguing and typology came to the fore in daily practice at the ethnology department in the Museum. While Elsie Bramell was undoubtedly better educated in social anthropology

than Fred McCarthy, the opposite would have been the case for cataloguing and typological skillsets. Although he held no degree in anthropology or librarianship, Fred McCarthy had been working at cataloguing library material and later zoological and lastly archaeological material for over 13 years by the time Elsie Bramell joined the museum. Looking at the image in Figure 18 through this lens then, I have to ask where the balance of power lay between these two individuals. I doubt the answer lay wholly in the personality of either. Fred McCarthy may here have been sitting on a desk, slightly higher than Elsie Bramell, but in Australia of the time he also stood on a massif of inherited male privilege which made it inevitable that it would be Elsie Bramell who would retire to allow the career of Fred McCarthy to continue.

The only insight I directly have to the relationship between Elsie Bramell and Fred McCarthy comes from the recordings by Hilary duCros (duCros 1983a). In these recordings duCros commences by asking both Elsie (nee Bramell) and Fred McCarthy to reflect on their earliest years together at the Australian Museum. Despite Elsie having been senior to Fred McCarthy at the time, McCarthy is frequently insistent on his recollection of events as correct, voiced by simply speaking loudly over Elsie - to the often, quite emphatic, but steely and quiet contradiction by Elsie. As the interviews progressed and covered time after 1941, when Elsie had retired from the Museum, Elsie's voice was barely heard at all other than politely pottering in the background to prepare and offer tea.

My first impression of these recordings was that McCarthy was somewhat deaf and his frequent resort to loud almost bellowing tones was a result of this deafness. Subsequent listening leads me to propose that this deafness and emphatic volume may have been selective, and a technique by which McCarthy could continue to dominate conversation and rotate it around himself. Elsie on the other hand had obviously developed techniques to quietly deal with this tactic. At one point in the recording (duCros 1983a) (Tape 1 side B) McCarthy complains dramatically and in grandiose terms about the sacrifice he had made in carrying out gruelling fieldwork "I was walking up to 12 miles a day with those men, in tropic heat. Hunting kangaroos and fishing in waterholes". Elsie quietly slipped in the line "Try chasing three kids Fred" (referring to her domestic duties at the time). Elsie's line was obviously either not heard by Fred McCarthy or was heard and selectively ignored - his only response was a bellowed "Ey What?!" while the interviewer (duCros) and Elsie enjoyed a quiet laugh at his expense.

Elsie engaged energetically with the early phases of anthropological research in NSW. Not only did she join the Anthropological Society of NSW, but she was also a member of the Society's committee between 1935 and 1939, including a period as the organisation's secretary (Ramsden & Bramell 1937). In addition to this, Elsie Bramell spent the period 1940-1946 as an editorial member of the journal *Mankind* which was published by the Anthropological Society of NSW (Oakes, Meehan, & Bolton 1985).

In 1938, Elsie again attended the Australian and New Zealand Conference for the Advancement of Science - this time held in Auckland, New Zealand. There Elsie raised the need to protect Aboriginal Australian artefacts and places. It is noteworthy that Elsie's call was made long before consciousness of this issue became foregrounded in Australian anthropology and certainly long before legislation was passed for the protection of Aboriginal places and sites (Oakes, Meehan, & Bolton 1985).

It may be that the dearth of funding at the Australian Museum lay partially behind the romantic relationship between Elsie Bramell and Fred McCarthy. In the absence of official funding for archaeological surveying or exploration, the pair carried out a significant amount of independent fieldwork together for the museum on weekends. They undertook an archaeological survey of 17 rockshelters in the Wollombombi area of NSW, excavated at Emu Cave, and collected stone artefacts and recorded rock carvings at Maroota, Port Kembla, Dee Why and Wondabyne (Oakes, Meehan, & Bolton 1985).

In 1940 Elsie Bramell and Fred McCarthy married, with Elsie forced to take retirement under the Public Service Regulations (Bowdler & Clune, 2000). Elsie evidently remained interested in the field of archaeology. Her best known publication reached press some six years after her 'retirement' and was a joint publication under her maiden name, with Fred McCarthy and H.V. Noone on *The stone implements of Australia* (McCarthy, Bramell, & Noone 1946). This forced retirement and estrangement from the field must surely have been galling. Only two years after McCarthy, Bramell & Noone (1946) went to press, Fred McCarthy joined the Australian-American scientific expedition to Arnhem Land. This was the largest such expedition undertaken in Australian history to that time. Included in the expedition team was Margaret McArthur. Margaret (1919-2002) was ten years younger than Elsie and was permitted to accompany Fred McCarthy and the expedition by virtue of

(among other factors) not being married to him. Margaret McArthur went on to become the first woman to gain tenure as an anthropologist in Australia - in 1965.

In common with the other female anthropologists we examined previously in this chapter, Margaret McArthur remained single through almost the entirety of her career, towards the end of which she married a retired professor of anthropology in 1976. It is hard to imagine that Elsie would not have known and experienced at least some anguish that another female anthropologist could join the expedition that her husband was on - but that she could not.

It would be remiss however to focus solely on Elsie Bramell's need to leave anthropology and archaeology as the sole forced decision of her life. There is too the movement away from social anthropology to archaeology which came about through her need to grapple with the sheer weight of *things* that had accumulated at the Australian Museum, and the requirement to catalogue and create typologies for these objects. This is an inherently archaeological rather than anthropological activity. There is too the possibility that Elsie's acceptance of this shift in career may have resulted from her not wanting to leave Australia at this point in time to pursue field work or further studies as her peers did. Elsie's father Bertram Bramell passed away in Sydney 1938 at the age of 71 and it is worth considering whether Elsie may not have wished to leave Sydney in the years immediately prior to her marriage.

Elsie's time in New Guinea (and possibly memories of time with her father there) continued to play a role in her thinking well after she left the Australian Museum. The last public speaking role that I have been able to identify by Elsie is a talk entitled 'Splendid Photographs of Papua' given at the Pacific Islands Society and listed in the *Pacific Islands Monthly* (Anon 1943). The existence of these photographs raises questions to their origins - Elsie Bramell was only at the end of her primary school years when her father Bertram retired and joined his family in Australia and one doubts whether she could have taken these photographs herself at such an early age. Presumably these photographs were either taken by Bertram Bramell in New Guinea and shared after his death by his daughter Elsie, or were taken by Elsie during a return visit to New Guinea, perhaps during her masters' research. I have not found any evidence for such travel by Elsie during her masters' and given that she was employed at the Australian Museum during the period of her studies I think it more likely that she was sharing her father's images.

There evidently existed a powerful sense of belonging among Australians who had once served or lived in New Guinea. Not only did Elsie Bramell deliver the above mentioned talk to a Pacific Islands Society in Sydney - the existence of which speaks plainly of an expatriate sense of identity, but the listing of this talk, along with that of her father's death in 1938 and that of her mothers in 1970 both appeared in the expatriate publications *Monthly (Papuan Courier, 1938, 11 November p., Pacific Islands Monthly, 1970 vol.11, p.135)*. The headline of the section containing the announcement of Elsie's mother's death reads 'Deaths of islands people'. Apparently, despite having lived in Australia for most of her life and having left New Guinea some 40 years earlier, Elsie's mother still qualified as an 'islands person'. So too it would seem, Elsie Bramell continued her sense of connection with the Pacific Islands.

Elsie Bramell had already then largely given up at least two forms of identity, yet had experience maintaining connection to both, albeit in greatly diminished form. For all her expertise, knowledge and qualifications relating to New Guinea, and her emotional connection to the place as evidenced in her sharing of not only academic but also aesthetic images of the land in public talks – despite this, she was no longer of New Guinea and had to accept an identity as an anthropologist and archaeologist of and in the Australian mainland. Similarly, despite having studied under and among the most brilliant minds of the anthropological world at the time, it seems that circumstances demanded she remain close to home and accept employment at the Australian Museum.

This does not downplay the historically unprecedented achievement of Elsie's obtaining employment at the museum, nevertheless the role and its associated object-related tasks were a far cry from the social anthropological insights and study that formed the basis of Elsie Bramell's anthropological masters thesis. It seems to me that Elsie had displayed a considerably adept hand at dealing with whatever life dealt her. She could not be in New Guinea and could not find local work as an anthropologist - but she did make an extraordinarily competent fist of adapting to the opportunities which were available to her.

And then there was Fred. Frederick McCarthy, young, handsome, well dressed, sporting and chipper (Khan 1993), with whom Elsie shared an office and also, on field trips, a car and time in the outdoors. I can only speculate, but hope it is informed speculation, that here too, the pragmatically minded Elsie Bramell made her mind as to what she wanted in life,

understanding the sacrifices entailed, identifying the injustice of the situation with an anthropologist's eye, but accepting such was the unfortunate lot of women at the time. After all too, if one wanted a life partner with whom one could discuss a passion for anthropology and archaeology, the market at the time was significantly constrained.

I have no direct evidence to support the argument in the paragraph above. Yet having listened several times to the one and a half hours of interview tape with the elderly Elsie and Fred McCarthy, I am struck by the absence of rancour or openly expressed resentment by Elsie, at any time. Certainly, as cited above, Elsie was not beyond taking a friendly poke at Fred when he waxed overly pompous – but it felt as if this was the whole extent of the exchange, there was no attempt by Elsie to drive the point home, to reiterate it for Fred's (apparent) difficulty in hearing, or to collude bitterly with the interviewer against Fred. There were equally many occasions in these tapes on which Fred McCarthy accepted Elsie's firmly but quietly voiced corrections (usually stated to him several times). There was simply an acceptance that Fred had pursued a career in anthropology and Elsie had pursued a career in parenting. Perhaps the harmony I allude to was not always present in the relationship. This harmony may be the outcome of many decades of settled emotional resolution and retrospective acceptance. Or perhaps, Elsie Bramell was for the many years of her marriage as stellar an individual as a partner, as she was for the few years of her archaeological and anthropological career.

Chapter 7: History of Australian archaeology revisited

7.1 OVERVIEW AND DISCUSSION

There are, I am sure, more ways to analyse and describe the historical formation of an academic community, than there are ways for such communities to form. It is the nature of analysis to unpack, dissect and expand that which previously may have appeared natural and taken for granted. At most basic, to those who would doubt the value of internal history to the discipline of archaeology, I would observe that in a discipline that is defined by the study of the human past, surely it is necessary for us to also understand the historical humanity that has resulted in our current discipline – as varied and diverse as it may currently be.

We are all artefacts of the past, and can be analysed, contextualised and comprehended through methods that metaphorically bear considerable similarity to techniques used by archaeologists in study of artefactual assemblages. The understanding of origins, formation and manufacture processes, relationships to other materials and function – all these techniques are commonly utilised in analysis of material culture. They are also valid metaphors for the study of archaeologists as artefacts, in seeking to know and understand how they came to occupy their place relative to the archaeological community – and indeed, how that community came to be.

Leon Edel (Edel 1986), provided a further metaphoric insight that I have raised several times in this thesis. That is the role of the biographical historian to detect the ‘figure under the carpet’ – the reality of the knots and weaves that lie behind or beneath the surface image on a rug. Archaeologists are often already adept at this. We are frequently presented with a mass of apparently inchoate data, or perhaps a single artefact such as a stone tool or fragment of a bowl or plate, and from these we unpack meaning, teasing threads of evidence apart, to produce a picture of origins, formation and manufacture processes,

relationships to other materials, function, social significance and historical specificity. The picture derived is frequently quite at odds with the initial appearance of the data.

So too is the case with archaeobiography (Carr 2012), a method of hybrid historical, biographical and archaeological analysis that affords deepened and unique insight not only to the subject of the biography, but to the academic community with which they interact, or by whom they are distanced. Lydia Carr's archaeobiography of Tessa Wheeler (Carr 2012) is a laudable early work in this genre, which with great insight sketches not only Tessa Wheeler's career and considerable capacities as an archaeologist, but does so against the human background of Tessa Wheeler's need to cope professionally and personally with the blatant and frequent infidelities of her husband Mortimer Wheeler, who often involved himself sexually (I hesitate to say romantically) with members of Tessa Wheeler's excavation staff. This approach adopted by Carr (2012) differs markedly from the "Great Men and Women" approaches to histories of the discipline such as Balter 2010, Cohen & Joukowsky 2004, Fagan 2003, Drower 1995, and Griffiths 2016, to name only a few.

It differs markedly too from the compendium or general histories of archaeology such as by Daniel, Schnapp or Trigger (Daniel 1975, Schnapp, 1996, Trigger 1989, 2006) in the detail in which it depicts and examines highly personal aspects of the protagonist's life, and also in the attention paid to the effects that these personal aspects of life likely had not only on their careers, but also on the careers of associates and the development of the archaeological discipline and community. This potential wider effect on the development of the archaeological discipline is a core aspect of my thesis.

The mode of archaeobiography also certainly differs from and highlights the distinction between biographical works of archaeologists that have been written from a non-critical perspective on the one hand (Balter 2010, Sabloff 1998) and on the other hand approaches such as Carr's (Carr 2012) which often seek to highlight rather than conceal the "figure beneath the carpet". The utility of archaeobiography is even further highlighted in instances where it contrasts further again from those particular forms of archaeological biography in which the autobiographical mode is utilised to entrench for the author a position at the disciplines core. Here I refer to autobiographies by those practitioners who through such works consolidate their position as "Great Figures" in the field – and by

automatic effect further marginalise those whose work and persons they have often already pushed to the margins of the discipline.

To only make brief mention of a pair of examples, John Mulvaney's autobiography places himself firmly at the centre of Australian archaeology, while roundly disparaging the "amateurs" who preceded him, and those whose subsequent approaches differed from his own (Mulvaney 1986). This will be explored at some length later in this chapter. Lewis Binford's autobiographical introduction to *An Archaeological Perspective* (Binford 1972) certainly depicts himself as the central pivot of the New Archaeology, and by default archaeology as it was to validly continue in Binford's eyes. Although written at the very end of her career, Margaret Murray's biography *My first hundred years* (Murray 1963), again places herself as central to her field of Egyptology. To reiterate, in carrying out a very intimate, critical and revealing biography of Tessa Wheeler, Carr (Carr, 2012) diverges considerably from the Great Figures approach that has dominated much archaeological biography and autobiography.

Yet, I believe a considerable difference exists between the motivation of Carr, and mine in this thesis. As elaborated in the introduction to this work, only after some self-reflection did I understand that my impetus to comprehending the human background of archaeology in the manner that I do, derived from my own embedded and culturally weighted notions of time as circular and spiral rather than linear, and further, from equally culturally specific perceptions of the deceased as still exercising agency in the world. I do not insist that these modes of thinking are rational, or that the reader adopt them. Nevertheless, I must acknowledge the role that they have played at times in my thinking - and invite the reader to explore the history of archaeology from a perspective perhaps not widely shared. In many ways, this way of thinking may reflect upon and bring one back closer to thoughts and experiences which for many in archaeology and beyond, might have formed our first attraction to the discipline. That is the perception (valid or not) that the past is within our experiential reach, that the world of the pharaohs, of the kings of Mesopotamia, of ancient Asia, Greece, Rome, Britain and beyond – all these can be almost tangibly felt through engagement with artefacts and sites. It could be critically observed that such views are anachronistic 'Boys Own' representations of the past. Yet what such criticism cannot subdue is the raw experiential thrill of archaeological discovery, in the moment in which the past reaches out to erupt through messy and clinging soil, through often laden

philosophical and theoretical considerations, and simply provides the discoverer with a sense (valid or not) of direct engagement with people of the past. This notion of a phenomenological experience of the past through encounter with artefacts and sites is not novel.

Although Steve Brown's doctoral thesis (Brown 2015) primarily concerns attachments between people and place, it commences with a discussion of the manner in which meaning reaches out from artefacts themselves – specifically Aboriginal Tasmanian woven baskets: 'The baskets are not empty. They are full of makers, their stories, their thoughts while making. The baskets are never empty. All of the thoughts jump out of the basket onto all of us; (Verna Nichols in Brown 2015, p3). It is this meaning-laden sense of immediacy that artefacts are frequently permeated with, that I refer to here in describing the manner in which artefacts can short-circuit temporality.

The empathetic approach in archaeology is not without its critics even among its less positivist practitioners. Sarah Tarlow cautions against the direct study of emotion in archaeology, noting that emotions are culturally and biologically specific, and cannot be divorced from their cultural milieu (Tarlow 2000). Or more bluntly, that 'introspection is not in itself a methodology for understanding the emotions of past people' (Tarlow 2012). Yet, whatever the reservations expressed towards empathetic approaches to archaeology may be, founded as these reservations generally are on criticisms of epistemological weakness (Wylie 2012) – nevertheless the affective aspect of artefacts on their finders and beholders remains as a narrative thread that joins past and present. As John Tosh argues: 'The stories we tell ourselves about the past may not be completely coherent or completely convincing, but they are rooted in the fact that human beings not only believe them but enact them on the assumption that social action is a continuum through past, present and future' (Tosh 2006).

This phenomenon of discovering and understanding archaeologists metaphorically as artefacts of the past, artefacts which reach out through the past to the present, requires an element of analysis beyond that which can be brought to bear on physical artefacts. It may well be that for many historical forms "The past is a foreign country; they do things differently there" (Lowenthal 1985). Yet in archaeobiography, and particularly given my conceptions of direct interrelation between the past and the present, the past is not quite

foreign at all. If I am to derive the sorts of insight which I have previously depicted as *emic* or *verstehen* (empathetic knowledge) into the lives and motivation of both fringe actors and their archaeological mainstream community, then I can hardly do so without asking questions that seek empathetic insight to their personalities and characters. And I must accept that I can never be totally assured of correctly reading or interpreting the actions and interactions of past individuals, particularly those separated from me by divisions of class and gender. The proposition by Collingwood that history consists of reconstructing the thoughts of its actors (Collingwood 1946), or even less radically, the call by E. H. Carr to explore questions such as “what if?” in history (Carr 1961) – both these lines of approach presuppose a commonality between the thinking of the past actor and the present historian or biographer. This presumption of commonality is in itself problematic.

Here I would turn again to the perspectives of critical realism, which support the contention that although I may not be absolutely certain of my interpretation of past events, I can nonetheless through historical research, comparison and analysis, reach an understanding that is likely to resemble events of the past. For without this pragmatic synthesis, it would be very difficult to come to grips with the varied backgrounds of the individuals studied in this thesis, and the manner in which they would have interacted, at all.

Certainly, the privileged world in which Dermot Casey was raised is well outside my experience, as too was his life as an individual of considerable inherited wealth. But for all this estrangement of Casey from the social experiences of most then and now, including my own, it does seem possible, through a close examination of correspondence, diaries and records, to build a picture of Casey that is more than simply a litany of social achievements and military awards. Casey emerges as a shy individual somewhat overshadowed by his older brother Richard who also seems to have deflected some of their father’s stringent glare from Dermot’s shoulders.

I do not think we can underestimate the commitment shown to archaeology by Casey. It may be that he was not academically gifted, however in fieldwork, and his dedication to teaching fieldwork, Casey was notable. Having served with award in the First World War, Casey had volunteered for service during the Second World War. By this time Casey was a family man and near the end of the war was serving in the Royal Australian Artillery in New Guinea (Spriggs 2021, p.4). Mortimer Wheeler colluded with Richard Casey to have Casey

flown from New Guinea to take over an archaeological field school that Wheeler established in Taxila (now Pakistan). At the time, Mortimer Wheeler was almost certainly the most prominent “Great Figure” archaeologist in the English speaking world and was known particularly for his innovation and excellence in fieldwork techniques. That Wheeler went to considerable lengths to have Casey flown to Taxila to teach archaeology, speaks very loudly not only of Casey’s capabilities as a field archaeologist, but as an instructor too. This sense of duty and service, both militarily and archaeologically, and likely extending throughout life in general, reflects deeply entrenched social values that were inculcated to the Casey brothers by their father Richard Casey Senior (Casey 1966).

It was I imagine, with this sense of duty and service, that Casey a 59-year-old senior figure in the Victorian museum world with quite some standing in world and Australian archaeology reached out to the 31 year-old John Mulvaney to request a place on Mulvaney’s field team at Fromm’s Landing. Mulvaney was trepidatious, yet I have found no evidence to suggest that the fact of Casey being almost twice Mulvaney’s age, or the significant difference in class, social and economic standing, in any way expressed itself negatively in his dealings with Mulvaney. On the contrary, a mutually respectful and long lasting relationship developed (Mulvaney 2011:100).

The under-recognised extent of Casey’s contribution to Australian Archaeology, needs to be understood in light of the attitudes towards reward for service specific to his social niche, in which overt acceptance of praise or reward was somewhat tasteless – or as Casey himself put it to Isabel McBryde “no need to acknowledge” (Casey Archive FR: A.ai 20/7/1974.). The question remains to what extent this willing self-effacement by Casey in his assistance to John Mulvaney may have been played upon by Mulvaney in placing Casey as liminal to Mulvaney’s mainstream. I doubt this was overtly the case - Mulvaney certainly sought Casey’s expertise in Mulvaney’s *Australian archaeology: a guide to field techniques* (Mulvaney 1968). Yet one cannot help but wonder whether Casey’s reticence to accept public credit for his very considerable contributions to John Mulvaney’s field and published works, was in fact welcomed by John Mulvaney. What we have here then in Casey, is an archaeologist who was of expert standing by Australian and international standards, yet who by dint of a combination of personality, social standing and conditioning seems to have sought to place himself outside the core community of Australian archaeology.

The case of Alexander Gallus stands in stark contrast to that of Casey. Ten years younger than Casey, and nearly twenty years older than John Mulvaney, Alexander Gallus held on paper, all the qualifications for admission to the Australian archaeological fraternity. Holding two doctoral degrees and having held a directorate at the Hungarian National Museum, Alexander Gallus arrived in Australia after a period of refugee status in Austria during which, notwithstanding the difficulties of his refugee standing, he had managed to secure various forms of academic employment and had published academically.

Yet, the post war Australia to which Alexander Gallus arrived was a narrowly xenophobic one, a xenophobia which seems to have extended to the academic world too. Australian archaeology at the time can be depicted as almost wholly Anglo-Saxon, English speaking, and empiricist. Matthew Johnson provides a keenly insightful observation that empiricism is effectively a deeply embedded cultural habit of much Anglo-Saxon life. Among the characteristic manifestations of this empiricism that Johnson highlights are the presence of an imagined common community “we”, the appeal to the self-evident “we all know what this is”, and a rejection of rhetoric with a stress on “plain speaking” (Johnson 2011).

Taking into account Gallus’ origins in a very distinct cultural milieu, his multilingual ability, wide reading and incorporation of philosophical and psychoanalytical epistemology to his archaeological writings –all these only served to distance him further from the mainstream of Australian archaeology. Perhaps infuriatingly to this mainstream, Gallus garnered a devoted following among the Archaeological Society of Victoria (ASV), with many of his excavations published in the journal of the ASV, *The Artefact*. Gallus’ lectures at the ASV were well attended and received, and his fieldwork was philanthropically subsidised.

Gallus’ claims regarding his excavation at Koonalda Cave were subject to a parallel excavation by a young team primarily from the University of Sydney, with what was quite overtly the aim of fact checking the claims regarding the site that Gallus had previously made (Gallus 1968A). For all that the subsequent publication of this excavation season restrained itself to tersely polite prose, the text can be observed as an intentional adoption of the empirical “plain speaking” mode noted by Johnson as a crucial element in construction of empiricist argument. This was a line of argument rendered more apparent for its cultural embeddedness, when contrasted to the at times florid and expansive ideological explorations of Gallus’ chapters in the same work (Wright 1971).

Gallus was elected a member of the then Australian Institute of Aboriginal Studies in 1966, not without some raised eyebrows, and received some funding from that institution (Megaw 1983). Nevertheless, he never gained an academic position in Australia, despite his eminent regard overseas as a world authority in European archaeology. His popularity with the ASV, and his frequent publications in its journal *The Artefact* appear to only have further distanced himself from the Australian archaeological community, with John Mulvaney having the ill grace to publish a denigrating summary of Gallus' archaeological life work, in *The Artefact*, as an obituary shortly after Gallus' passing (Mulvaney 1998). The manner in which Mulvaney sought in this article to explicitly and quite vigorously distance himself and what he portrayed as professional or competent archaeology from the practice of Gallus, will be examined at some length in the following section that more specifically addresses Mulvaney's crafting of the history of Australian archaeology around himself.

I turn now from two individuals, Casey and Alexander Gallus, whose distancing from the archaeological mainstream I feel I have had some success in explicitly addressing. I cannot claim any such insight to the motives of Elsie Bramell, who possessed all the talent and intellect to have pursued a career in anthropology and archaeology but did not. My inability to gain further insight results from a combination of lack of information, and the specific moment in history at which Elsie lived which makes it impossible to apply a basic mechanism of historical reasoning to her case. In situations where historical information is lacking or not replete, the historian in seeking to understand a choice of action by a historical figure, or an outcome of events, may apply the test of *ceteris paribus* "all things being equal". In other words, if all considerations before the historical actor were equal, why may they have acted or chosen the in the manner that they did.

Elsie Bramell did not live at a time when all things were equal. She both lived at a time when women were still unequal to men in many ways, but also at a time when world events and nascent feminism (or more accurately suffragettism) resulted in great flux in gender relations and roles. The First World War had seen women engage in nursing and military roles to an extent not previously exercised, and to a limited extent women had taken up some modes of employment traditionally held by men. University places and a wider range of courses were more available to women, and pioneering women practitioners forged careers, despite often open and explicit misogyny. By the time Elsie

Bramell chose to marry Frederick McCarthy in 1940, besides teetering on the maximum extent of the Second World War, western society was also poised at the breaking peak of a wave of a historical reformation of gender roles – a wave that continues to carry society in its wash today.

It is understandable therefore that the rise and disappearance of Elsie Bramell from the archaeological (or more strictly speaking anthropological) Australian community is one that strains my capacity to assess, notwithstanding applications of critical realism and historical methodology. Elsie sparkled as a young woman and academic. Captaining her high school, she went on to study at the University of Sydney under under Radcliff-Brown, Raymond Firth, Camilla Wedgewood and Adolphus Elkin. Her classmates included Phyllis Kaberry, W.E.H. Stanner, Gregory Bateson, Margaret Mead and Reo Fortune – all of whom went on to major careers of international renown.

Yet, Elsie took on a relatively constrained role compared to her classmates, as Scientific Assistant at the Australian Museum. Here too she excelled and forged new paths, taking the Museum and its contents to the public via radio and in-person lectures. Despite this, Elsie seems to have accepted a restricted career compared to her above-mentioned classmates. Certainly, in choosing to marry her junior and less qualified colleague Fred McCarthy, Elsie would have recognised the end to her career in the museum. Yet, a world of anthropology existed outside of the Australian Museum, and surely Elsie would have been aware of the potential for married women to continue in academe - as did her former classmate Margaret Mead.

This is where the difficulty in understanding Elsie's choices arises. Certainly, the patriarchal mainstream social mores of the time emphasised a woman's place as in the home and as secondary to the husband. It is very difficult to determine to what extent Elsie may simply have held views sympathetic to this worldview, notwithstanding her anthropological and apparently generally independent character. Then again, perhaps Elsie understood the innate negative implications of rebelling against the dominant patriarchy, with a depth of insight not apparent to her fellows. For, although Margaret Mead and Phyllis Kaberry did proceed to international anthropological fame, they do not appear to have formed stable relationships within the social norms that so strongly prevailed at the time, certainly within conservative Anglo and Anglo--Australian society, in which singledom too was regarded

askance. Margaret Mead moved from fraught relationship to relationship (Caffey & Francis 2006), while Phyllis Kaberry passed away of alcoholism (Toussaint 1999).

In contrast, Elsie and Frederick McCarthy appear to have enjoyed a long (and from appearances) happy marriage, with Elsie raising their three children while Fred carried out fieldwork and his duties at the Australian Museum. Certainly, stigma around divorce may have shielded marital discord from public and the historical view. Yet from the evidence available I have no indication that this was the case in the McCarthy's marriage. In audio recordings of Elsie and Fred (duCros 1983a), when Fred nostalgically aggrandises his memories of the hardships of fieldwork, Elsie makes one of her rare interjections: "you should have tried chasing three children" – but the tone is gently joking, with no malice or jealousy evident, and all impressions are of an affectionate couple.

It would be simple to blame the patriarchy for denying Elsie Bramell a career in anthropology. However, this would concomitantly deny Elsie agency to have made choices in keeping with her internal and now unknown values and aspirations. Elsie may never have wanted a lifelong career and may have always wanted life as a mother and housewife. Of course, these aspirations also result from social programming. Elsie Bramell chose to leave anthropology and archaeology; Elsie Bramell was forced out of anthropology and archaeology – either or, or both, we can probably no longer determine which.

7.2 THE GATEKEEPERS OF AUSTRALIAN ARCHAEOLOGY

What I hope to have drawn out through these three case studies and my investigation of methodology, is that an *emic* or *verstehen* approach that seeks to intimately interrogate biographical data reveals a history of Australian archaeology that is quite unlike official versions to date. In particular it differs from the history by Mulvaney (Mulvaney 1986) which seeks to portray Australian archaeology as having moved abruptly from an amateur to a professional phase (Mulvaney 1986, Moser 1995).

In its stead we see Mulvaney 'the professional' deeply reliant on Dermot Casey 'the amateur'. We see Alexander Gallus who possessed all the qualifications and publication history to be considered a professional, excluded from the discipline, and we see Elsie Bramell, again eminently qualified and employed in the discipline, leaving the field likely due to social constraints of the time. All three of these individuals (Bramell, Casey and Gallus) practiced before the putative era of disciplinary professionalisation. What we see

then is a far messier, far more entangled and interpersonal development of the discipline than a clean transition from amateur to professional - if in fact these terms of “amateur and professional” are helpful or hold any validity at all in application to the history of Australian archaeology.

There certainly are other individuals beyond the scope of this thesis who have considerable bodies of peer reviewed archaeological output, but who are considered marginal or outside the Australian archaeological mainstream. These include Robert Bednarik, an autodidact and the founder and editor of the journal *Rock Art Research*. Bednarik has a publication record of at least 9 books and by his own count over 1,400 peer reviewed articles.²⁰ The late Grahame Walshe was also an autodidact who undertook decades of work on Aboriginal rock art in the Kimberly region. Walsh received a D.Litt. from the University of Melbourne in 2004. The citation for this degree in part read ‘His contribution to the field of rock art research, management and conservation has been immense, and of international as well as national significance.’ Walshe was awarded a thesis-based Ph.D. from Griffith University in 2007 (Myers & Myers 2020). Bednarik lists multiple honorary academic positions on his resume, but makes no mention of his postgraduate or other academic qualifications.²¹ Despite their considerable achievements either Bednarik nor Walshe held positions in Australian university archaeology departments, and one suspects this is largely due to their not having progressed through the formative systems of university education and membership in the mainstream disciplinary community.

While my approach has been reliant on a more intimate portrayal of a limited number of individuals, the conclusions I have reached - that Australian archaeology has developed in a manner that cannot be cleanly divided into categorical divisions such as amateur or professional, are well demonstrated by a recent article that takes a very different, but incisive approach to reach similar conclusions to mine. The recent article by Mathew Spriggs “Everything You’ve Been Told About the History of Australian Archaeology is Wrong!” (Spriggs 2020), discusses notions of the putative amateur / professional divide in Australian archaeology more incisively and at greater depth than Spriggs more recent works

²⁰ <http://www.ifrao.com/robert-g-bednarik/>

²¹ <http://www.ifrao.com/robert-g-bednarik/>

on the topic (Urwin & Spriggs 2021, Spriggs & Howes 2022). Spriggs (2020) is worth quoting selectively at some length to demonstrate the manner in which its findings closely (and those of Spriggs subsequent works) align with my findings in this thesis:

The foundation myth of Australian archaeology: The title of this paper may sound a bit extreme; however, it is largely justified when one examines the primary evidence for the history of Australian archaeology compared to the myth of its development promoted by the 1950s' and 1960s' generation of academic archaeologists. It is myth unthinkingly repeated by scholars as the standard paradigm for the discipline's history in Australia today. The myth creates a rupture between the practices of modern 'professionals' and past 'amateurs' where none in fact existed and ignores a considerable body of earlier archaeological research of a very high standard, work that is now very largely unreferenced and forgotten. It also allows us to disown the more unpleasant aspects of our disciplinary history. One of Cambridge-trained John Mulvaney's undoubted achievements was to invent the entire topic of the historiography of Australian archaeology" (Spriggs, 2020: 1).

This rupture between past and present archaeological practice is further emphasised by Billy Griffiths, who referring to Mulvaney's excavations at Fromms Landing writes: "The modern era of archaeological investigation began on Friday 13 January 1956" (Griffiths 2017, p. 106), it was "the dawn of a new era of research in Australia. It was where the first university-trained archaeologist sank his trowel in Australia" (Griffiths, 2017, p. 100). The imagery could hardly be more dramatic and evocative – the sinking of a trowel to delineate in the very earth, the difference between amateur and professional archaeologists. It is worth bearing in mind that it was not until the second phase of excavation at Fromm's Landing, that Dermot Casey brought his far greater practical archaeological skills to bear on the site.

The explicit inference here, in my findings and those of Spriggs (2020) is that there has been in Australian archaeology a strong disciplinary movement and process of exclusion, and by default of inclusion and self-definition, and that these movements and processes have been accepted and are now innate not only to the paradigm of the history of the discipline, but to the praxis of the discipline too. Spriggs illustrates that these processes of exclusion and definition were not neatly separated in time – on the contrary, practitioners existed during the supposed amateur era, who well fulfilled the criteria of professional practice, but who were overlooked or left out of the professional era described by Mulvaney (Mulvaney

1986). These include Dermot Casey, Fred McCarthy and Norman Tindale (1900–1993). palaeontologist Edmund Gill (1908–1986), dental anthropologist Thomas Draper Campbell (1893–1967) Dean of the Faculty of Dentistry 1938–1958 at the University of Adelaide, geologist Paul S. Hossfeld (1895–1967), latterly Senior Lecturer in Geology at the same institution. Robert Alexander Keble (1884–1963) was a Melbourne-born geologist and palaeontologist, Donald Sutherland Davidson (1900–1952) and Herbert Vander Vord Noone (1880–1955) (Spriggs 2020, pp. 1-5).

All of these practitioners were active well before Mulvaney gained his first dedicated archaeological position, in prehistory, in 1965 at the Australian National University. Prior to that, Mulvaney held a position teaching Greek and Roman history at the University of Melbourne, offering his first archaeological course, in Pacific Prehistory, in 1957. If we are to apply Mulvaney's criteria for professionalism, then surely these practitioners were professional too.

One cannot help but be reminded of Mulvaney's negative assessment of Alexander Gallus' work, by the following passage in this article by Spriggs:

"In re-reading Mulvaney's classic early summary of 'The Stone Age of Australia' (Mulvaney, 1961), what strikes one is the continually negative judgment on earlier work, largely through scepticism of claims made for human associations with Pleistocene contexts and/or radiocarbon dates, even when collected by competent specialists." (Spriggs 2020, pp.9-10).

Thus, through a process of public negative judgement of predecessors, and intentional crafting of the history of archaeology in Australia, Mulvaney was able to both cement the centrality of his place as the founding figure of Australian archaeology, and simultaneously adopt the role of arbiter of what constituted competent (professional) archaeological practice and what did not fit such criteria. The automatic corollary of both these practices would be the capacity to marginalise or distance from the discipline those practitioners such as Gallus (regardless of their qualifications) whose practice did not cohere with Mulvaney's empiricism.

Mulvaney was a trained historian, whose first academic postings were in teaching history. It is difficult to conceive that the manner in which he created the historiography of Australian

history was not intentional- certainly Spriggs indicates this was the case, and a strategic one:

“One of Cambridge-trained John Mulvaney’s undoubted achievements was to invent the entire topic of the historiography of Australian archaeology.....I use the term ‘invent’ advisedly, because as well as seeking to understand and build upon previous archaeological research, he was also establishing an act of distancing his own practice from that of previous generations, an act that was strategic ” (Spriggs 2020, p. 1).

Approaching the history of Australian archaeology from two very different perspectives, Spriggs (2020) and I have reached quite similar conclusions. The metaphoric approach I have adopted would observe that in keeping with Leon Edel’s notion of “the figure under the carpet”, all three of the case studies I have raised here; Dermot Casey, Alexander Gallus and Elsie Bramell – are knots and weaves under the carpet that are not visible at first glance on the surface of the carpet that constitutes the history of Australian archaeology. I have investigated in some depth, plausible reasons why this may be the case, with particular focus on the biographical and personal aspects of each individual’s life. This has revealed something more of the personal nature of the distancing or marginalisation that took place between each of these individuals and the archaeological mainstream and its gatekeepers.

Spriggs (2020) on the other hand has adopted a far more clinical approach, in attacking Mulvaney’s claims (Mulvaney 1986), and those of subsequent historians of archaeology such as Griffiths (2017), that an abrupt and marked transition took place between the amateur and professional phases of the discipline. Spriggs insists that such nomenclature of “amateur” and “professional” is not accurate or worthwhile, and he makes his case most simply and powerfully through the primary mechanism available to historians and archaeologists – chronology.

7.3 MYTHBUSTING AUSTRALIA ARCHAEOLOGY

Whether approached from the perspective of Spriggs’ chronological analysis, or from that of my broader, internalist and metaphorical approach, what emerges from the new perspective gained of the history of Australian archaeology, is that a fresh paradigm or paradigms are required to write it anew. Australian archaeology cannot be divided

vertically chronologically into neat strata of pre and post Fromms Landing, with its professional era commencing with a stroke of Mulvaney's trowel. There had been sufficient qualified controlled archaeological excavation in Australia before this to invalidate such a claim. Neither can the history of Australian archaeology be divided horizontally, that is between those deemed 'amateur' and those deemed "professional" who were working at the same time. Alexander Gallus (and one suspects others too) was eminently academically qualified but was not allowed entry to the Australian archaeological 'professional' community.

In this thesis I have illustrated how understanding more of the biographical detail of a few Australian archaeological practitioners, lends insight to the manner in which they perceived and interacted with the Australian archaeological community, and in turn how the Australian archaeological community interacted with them. To return yet again to Edel's metaphor of background knots and weaves and the differing figures they create on the back and front of the carpet – far more knots and weaves need to be investigated before we can reach a more coherent and historically accurate image of the development of Australian archaeology.

In this work I have focussed on marginal figures, at least those who are marginal in the annals of mainstream histories of Australian archaeology. This has been due to at least two factors. Firstly, I wanted to highlight the contribution to Australian archaeology of those who have not been included in the (at times autobiographical) "Great Men & Women" histories of the discipline – and who in fact appeared to have been marginal or marginalised. It is such people, the masses of 'ordinary' practitioners I contend, who have been behind the real growth and development of the discipline as a result of the sheer number of hours that they have contributed to it. Secondly, I wanted to explore whether there were identifiable reasons behind their apparent marginal status, reasons that may shed light on their specific relationship with the discipline, and the mechanisms of exclusion and inclusion of the discipline overall.

The outcome of this investigation is that complex processes of self-marginalisation and of gatekeeping and exclusion can be seen at play. Dermot Casey apparently chose marginalisation, however the extent to which this was consciously utilised by Mulvaney has not been determined here. It could be similarly claimed that Elsie Bramell chose self

exclusion from the discipline. Alexander Gallus is the clearest case of disciplinary exclusion. He displayed extraordinary perseverance in persisting with archaeology despite the overt local disdain towards his practice.

The prospect raised by this thesis is that a revisionary history of Australian archaeology is well overdue. Given the predominance to date of the “foundation myth of Australian archaeology” (Spriggs 2020), and the centrality of John Mulvaney – the inventor, promulgator and prime beneficiary of this myth, it is understandable that considerable effort would be required to successfully construct an alternative paradigm acceptable to the archaeological mainstream. Paradigm shift, at least according to Thomas Kuhn, does not come about solely through logic, but through a mixture of enthusiasm, sociology and scientific promise (Kuhn 1962). The methods utilised in this thesis are, I propose, an effective means of engaging the archaeological community in understanding the personal and disciplinary methods by which archaeological practitioners have been and are still marginalised or forefronted by the profession.

As outlined, I have focussed on liminal or marginal figures in Australian archaeology. I suspect that further research would reveal large numbers of similar individuals, some of whom may not have gone on to practice in the longer term, or who were content to labour away at their profession without seeking self-aggrandisement or to diminish and distance others. The everywoman and everyman of the archaeological world. Among those distanced from the discipline we may find individuals kept at bay due to factors including gender identity, sexuality, religion, race and ethnicity and socio-economic background.

A further potentially rich avenue of investigation would be into the fates of other qualified European archaeologists, who like Alexander Gallus may have arrived in Australia after World War Two, but who unlike Alexander Gallus may not have had the fortitude to persevere in archaeology in the face of considerable difficulties.

Then, there is the question of investigating the gatekeepers – those who have not formed the focus of investigation in this thesis, but who have inevitably come to notice as acting to marginalise others. It is not only the marginal who may have been excluded, allegations were laid in the 1990’s that it was impossible to gain a position at ANU without a degree from Cambridge University (Martin 1990). While the case was not upheld, it is noteworthy

that the complaint came from an eminently qualified and published scholar, Johan Kamminga, who subsequently published with John Mulvaney the authoritative synthesis *Prehistory of Australia* (Mulvaney & Kamminga 1999). Was Kamminga perhaps, like Gallus, eminently qualified, but not the “right type of person” for Australia academe? Answering that question is beyond the scope of this thesis, but what does seem quite evident from my investigations here, is that the decisions as to who was or was not the “right type of person” or in other words could hope to be a “professional archaeologist” was one made by metaphorical spiders on a web, with central actors pulling threads to which others responded.

7.4 ON THE MARGINS OF PROFESSIONALISM – A PERSONAL PERSPECTIVE

The preceding sections of this chapter have discussed the notions of professionalism in archaeology, including the use of the term to delineate between those within and outside the archaeological community. In keeping with Spriggs (2020), I doubt that the terms ‘amateur’ and ‘professional’ are useful diagnostic criteria, either with regards to historical or current practice. Both Mulvaney (1986) and Moser (1995) took professionalism of the discipline as primarily reflected in the shift from museum-based and also non-vocational practice to a discipline dominated by archaeologists employed in the university sector. This narrow definition may to some extent satisfy dictionary definitions of the term ‘professional’, yet as I will briefly demonstrate, the term ‘professional’ also carries other connotations that I would contend academic archaeology in Australia struggles to meet, and which in this failure continues to exercise processes of marginalisation.

The Oxford English Dictionary (*Oxford English Dictionary Online*, June 2022) defines Professional as

Professional:

3.a Of a person or persons: that engages in a specified occupation or activity for money or as a means of earning a living, rather than as a pastime. Contrasted with amateur

3.b Of an event, activity, occupation, etc. (now esp. a sport): undertaken or engaged in for money; engaged in by professionals (as distinct from non-professionals or amateurs).

4: Of, belonging to, or proper to a profession.

4.b Relating to, connected with, or befitting a (particular) profession or calling; preliminary or necessary to the practice of a profession. (Oxford English Dictionary, online – insert correct cite).

The above cited definitions of professionalism all rotate around the paid nature of work and the acquisition of skill sets as the primary distinctive measure of professional practice. Were this to be comprehensively the case, I may be more ready to accept John Mulvaney's definition of himself and his colleagues as professionals contrasted with amateur predecessors. But there is to my mind a patently obvious gap in the dictionary definition provided above. Namely, the extent to which professional ethics and conduct form an inseparable part of the establishment of professional practice and reputation.

The Hippocratic Oath, or its modern successors such as the Declaration of Geneva and in Australia the Code of Conduct for Doctors in Australia are important and publicly regarded statements of values and behaviours expected from medical practitioners. In the Australian legal profession, the Australian Solicitors Conduct Rules enshrines 'the collective view of the profession about the standards of conduct that members of the profession are expected to maintain'.²² The Australian Association of Consulting Archaeologists Incorporated (AACAI) similarly provides a code of ethics governing behaviour by members towards the public, the archaeological record, and each other.²³

Such codes of ethics are so thoroughly the standard among professional bodies, that any practitioner acting in a manner contrary to their ethical codes (regardless of their expertise), would not be considered to be acting in a *professional* manner. Professionalism then is only partially predicated by employment, but is significantly determined by the manner in which that employment is carried out in a manner that includes courtesy to colleagues and the public and maintains a respectable public image of the discipline.

²² <https://www.lawcouncil.asn.au/policy-agenda/regulation-of-the-profession-and-ethics/australian-solicitors-conduct-rules>

²³ <https://www.aacai.com.au/about-aacai/code-of-ethics/>

I believe it is the case that most academic archaeologists fit the dictionary definition of professionalism provided above, relating to paid employment (with the exception of honorary research fellows and emeritus staff), and also the definitions I have suggested of ethical conduct. Nevertheless, I have long found it difficult to reconcile considerable elements of accepted academic archaeological behaviour as ‘professional’ according to the yardstick of the behavioural professional ethics as outlined above. This is certainly the case in reflection on my own practice as an employee in a mid-sized heritage consultancy in Sydney, and comparison of the stringent parameters of acceptable behaviours in that commercial environment, compared to those acceptable behaviours that I have observed in academic gatherings.

I must admit having spent some years uncritically following the flow of events in academic archaeology in Australia, and engaging or tacitly approving behaviours that simply do not gel with my values or self-image. But such is the nature of pack mentality, of being on the inside, and not wanting to let go of a sense of inclusion. Only on deliberation and hindsight, and as a mature age observer, did the problematic nature of behaviours often enshrined by the Australian ‘professional’ (academic) archaeological community become apparent to me.

Perhaps nothing could better illustrate this than the range of frequently accepted behaviours at Australian Archaeological Association (AAA) conferences. It is a feature of these conferences, that although predominantly attended by archaeologists working in commercial consultancies, they are largely organised by and for academic archaeologists. Most sessions are organised and addressed by academic archaeologists, while generally perhaps one or two sessions are set aside for presentations by those in the commercial world. At the first of these conferences that I attended, the opening session was delayed while the facilitator (a research academic) was bailed from custody after being arrested for drunken assault the night before. Later, the recipient of a book award (again an academic researcher) had to be tracked down, sleeping off a drunk on a bench. On accepting the award, said person was presented with a large bouquet of flowers - which they dropped on the stage while marching off, and declaiming “Bloody flowers? Could have gotten me a bottle of whiskey!”. The conference reached its apogee at the end of conference dinner when as the band played ‘Eagle Rock’ the tradition was followed for male attendees to drop their trousers on the dance floor.

Tacit approval of assault, of alcohol abuse, and explicit acceptance of inappropriate behaviour. None of this cohered with behavioural norms I would hope for students or young graduates to absorb. Nor would they be tolerated for an instant in any commercial archaeological environment that I have worked in. After two or so further conferences with behavioural norms along the same lines, I took the view that approval of these behaviours through my ongoing participation was a step further than I felt comfortable with.

7.5 THE SUBTLE CREATION OF HISTORIES

It is beyond the scope of this thesis, but nevertheless worth considering, the extent to which tributes, single and compendium papers in honour, and festschriften play a part in creating and reinforcing notions of historical centrality and power, embedded as they often are, in adulatory biographical tributes to figures that have advanced the author's career. The effects of such publications are several-fold.

Firstly, the authors chosen to contribute to such festschriften are almost inevitably vetted (typically a colleague, collaborator and/or previous student) as suitably sympathetic to the festschrift recipient. This creates for the reader a highly skewed image of the individuals past and interaction with the discipline, an image from which conflict and argument is almost invariably edited out.

Secondly, is the issue of entrenched and reified hierarchies of power. The recipient of a festschrift is almost invariably an elder academic of considerable standing and at the end of their career. The contributors to festschriften are also generally senior academics, often in teaching positions in universities, or holding considerable influence in a research field. Their adulatory contribution to a festschrift not only situates the festschrift recipient biographically and historically, in a position of authority, it also situates the festschrift author as in some way sharing this position of authority, biographical and historical context, and likely affects the mutual perception of their relationship with students and research affiliates.

Lastly, is the extent to which festschriften are both generally adulatory or uncritical biographical tributes, and at the same time, are frequently the only source of published biographical information available on a given scholar. To the student with no personal knowledge of the festschrift recipient, and no alternative source of information, the festschrift represents a direct channeling of a conformist tradition embedded in a history designed to curate a specific conservative image of the festschrift recipient.

I term this process a ‘subtle creation of history’, because overtly no history is being created by papers in tribute. Yet covertly, through the processes outlined above, a powerful history of elite figures is created and situated as normative.

7.6 THE OTHERED ARE THE NORM

It is quite likely as a result of my self-conscious self-exclusion from these conferences, and AAA generally, that I became aware of my own position as both on the one hand active in the centrality of Australian archaeology (the 90% of archaeology that is commercial in nature), and on the other hand marginal to the official frameworks of archaeology that have been held up as the model of professionalism (academic archaeology). These official frameworks through conferences and journal publications continue to appropriate for themselves Mulvaney’s territory of ‘professional archaeology’. And so, to return to the beginning of this chapter, I was led to reflect on the manner in which my own biographical specifics, as a mature age individual from a non-Anglo background, had led me to my own position relative to the mainstream of Australian archaeology.

From this reflection an sense of alterity, my interest in ‘othered’ archaeologists grew. The approach I have adopted here of archaeobiography requires deep vertical study of individuals to tease out not only their apparent historical context, but to delve beyond into their ‘figure beneath the carpet’ (Edel 1986). In taking this approach I can only hope that future histories of Australian archaeology will also place the ‘othered’ at the focus of their study, in producing what I suspect will be an image of the discipline in which the ‘othered’ are the norm.

7.7 BEGININGS AND ENDINGS

“All endings are bound up in their beginnings, and all beginnings in their end” (*Sefer Yetzira* n.d)²⁴

²⁴ The *Sefer Yetzira* or *Book of Creation* is the earliest, although undated, acknowledged work of Jewish mysticism.

This thesis has reached a number of firm conclusions. Yet it was not the chief goal of this work, at outset, to seek concrete findings. Rather, to explore what might emerge of the history of Australian archaeology through the application to it of novel processes of investigation. And, through this exploration to gain a richer insight into formative processes in the history of Australian archaeology. This thesis set out to explore, and through the process of exploration reach deeper understandings, valuable in themselves as much as any conclusions to which they have led. As such, at the end of this thesis I would restate that it is in the ongoing exploration and application of my methods utilised here, that new understandings are likely to arise, rather than were we simply to take stock of its results. It is an exercise and demonstration of novel methodology, and it is in the appropriation, expansion and adoption of this methodology that I believe this thesis will have its fullest impact on the history of archaeology.

Despite this call for greater exploration, the exploration carried out so far in itself has demonstrated some firm findings and has highlighted what seems to be an irreconcilable tension between established histories of Australian archaeology to date, and history such as I have produced in which greater attention is paid to the close biographical context of Australian archaeologists. Australian archaeology deserves a better history than that that produced by those who write themselves central to it, or by their acolytes.

A challenge lies ahead then, to use the processes of investigation illustrated in this thesis to re-examine the available source material and reconstruct anew the artefactual assemblage that is the history of Australian archaeology in its diversity and variation through and across time. I have demonstrated here the potential scope of historical revision that can result from the application of the novel exploratory techniques that I have developed, and I look forward to others adopting elements of my approach they find useful

Chapter 8: Bibliography

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8.6 EMAIL CORRESPONDENCE

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