Buried History

The Journal of the Australian Institute of Archaeology

2022 Volume 58

Buried History

Buried History is the annual journal of the Australian Institute of Archaeology. It publishes papers and reviews based on the results of research relating to Eastern Mediterranean, Near Eastern and Classical Archaeology, Epigraphy and the Biblical text, and the history of such research and archaeology generally for an informed readership. Papers are refereed in accordance with Australian HERDC specifications. Opinions expressed are those of the authors concerned and are not necessarily shared by the Australian Institute of Archaeology.

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Cover: Archibald Henry Sayce, from the frontispiece of *Monument Facts and Higher Critical Fancies* (1904).

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Editorial

This edition has suffered some minor delays in production, and I hope that this has not caused any inconvenience. After twenty years of annual publication *Buried History* is making a few changes that will be described throughout the editorial.

We begin by paying tribute to Henry Huggins. The Institute was privileged to have Henry as a volunteer for nearly twenty years. Henry had a significant career in Victoria as a pioneering police crime scene investigator that led to a retirement devoted partly to archaeology. I am indebted to his widow, Dora, who will celebrate her hundredth birthday as this edition is being mailed and his children Daniel and Flavia for family information about him and two photographs.

The first paper is also a form of tribute, in this case to Professor Henry Sayce (1845–1933), who was another pioneer. He was first to apply much newly discovered archaeological evidence to historical and biblical studies. In spite of his extraordinary memory, remarkable facility for ancient languages and personal relationship with nearly every archaeologist working in the field at the time, many biblical scholars have derided his many discoveries and opinions. Academics have often been unable to appreciate his scholarship because he wrote in a manner that was understandable to non-academics, and applied a theoretical basis that was unfamiliar to them but, as I have attempted to argue, was entirely appropriate for the nature of the evidence. The referees, two of whom identified themselves, made many helpful comments. I especially acknowledge Emeritus Regius Professor Hugh Williamson, who has advocated a contrary view but did not stand in the way of this publication.

One change to the journal has been the decision to include papers on the history of archaeology more generally and especially in Australia. Michael Lever's paper is the first of this type. Michael is a Research Fellow of the Institute and a doctoral candidate at the University of Sydney where his dissertation on the history of archaeology in Australia is currently being examined. He has a deep understanding of the history of archaeology's philosophical foundation. His dissertation topic derives from this longstanding interest in the history of archaeological thought and practice worldwide. When not studying, Michael works as a heritage consultant and archaeologist for a commercial consultancy based in Sydney.

The two preliminary excavation reports of the Kourion Urban Space Project (KUSP) have been lodged with the Department of Antiquities, Cyprus, as required. They have not been published by the Department's journal, which has been inactive for some years, so they are included here to fulfil the excavator's responsibility to make the preliminary results known to the international archaeological community in a timely manner. The Institute has been a member of the KUSP consortium from the first season in 2012.

There are two contrasting book reviews. Both books are written by world leading scholars, but one offers a large amount of new evidence, while the other presents a new approach to the history of the topic. As you will see, the conclusions of the reviews also differ in character.

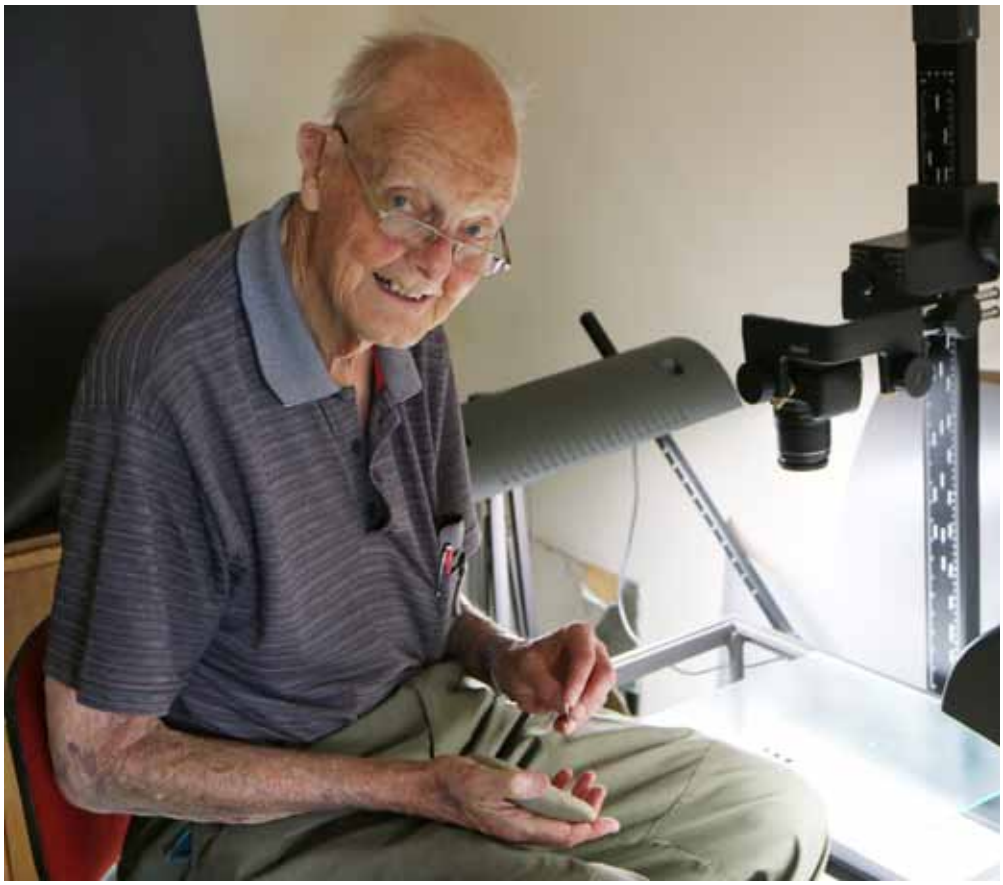
Some readers will have noted that there have been some changes to the Editorial Board. *Buried History* aims to be useful to school teachers and will increase content relevant to school curricula. Dr Alanna Nobbs and Dr Luis Siddall are involved in this sector in NSW and so have joined the Editorial Board. Dr Merrill Kitchen has retired from the Board after ceasing to be active in ancient history. We thank her for her contribution.

It has also been decided to make *Buried History* available online from the next issue as an open access journal. A hard copy will also be available for those who are prepared to pay for it, although rising postage costs may mean that there will need to be an increased subscription if the journal expands. It is intended that all editions back to 1999 will be made available online. Some papers prior to that may also be placed online if copyright issues can be resolved.

This approach is part of a growing trend. Researchers invariably make use of online journals because of the immediacy of access, but other people often prefer to have the feel of the paper journal. We aim to cater for both preferences.

As always, we acknowledge our referees, who give significant time to ensure reliability of content.

Christopher J Davey
Editor



Henry Gregory Huggins (1931–2022)

For nearly twenty years Henry Huggins was a volunteer at the Australian Institute of Archaeology serving in several expert roles. He undertook the photography of the museum collection and oversaw all aspects of security. He also carried out much needed administrative tasks, although this was not his preferred activity. He was a friend to all and promoted a respectful and positive atmosphere. Lunch breaks were often entertaining and informative, if not downright gory, as Henry would recount tales of his exploits as a Victorian Police Crime Scene Investigator. Henry had personal recollections of many of Australia's major crime figures and was able to comment reliably on their characterisations in television series such as *Blue Murder*.

Henry was born in Cardiff on 5 March 1931, the fourth child of the Reverend Arthur and Doris Huggins. He was educated at the historic Christ's Hospital during World War II. The school was established in 1552 by King Edward VI and students have continued to wear a uniform dating from that time. Henry played the clarinet in the school band. He retained his interest in music, playing the French Horn in the Whithorse Orchestra until the last couple of years of his life.

Henry emigrated to Australia in 1947 to work on a farm owned by his uncles and aunts at Hedley in Gippsland. There, he met a Swiss nurse, Dora Rauber, who was holidaying in Australia and staying at Toora. They married in April 1956. Henry joined the Police Force in 1959 and moved with his young family to Clayton. After serving as a traffic policeman he joined the Victoria Police Forensic Services Division, where he worked until his retirement in 1991. He attained the rank of Senior Sergeant and Officer-in-Charge of the Crime Scene Section. In the 1989 Queen's Birthday Honours he was awarded the Australian Police Medal for distinguished Police Service.

John Silvester wrote of Henry in *The Age* (9 April 2022):

> *We lost Henry, aged 90, in February and with him the last link to a time when you learned largely on the road. He joined in 1959 and soon found himself in the Scientific Section, driving a former army Studebaker canteen van refitted as a mobile lab to some of Victoria's biggest crime scenes.*
>
> *His son, Daniel, remembers family dinners when the phone rang and dad was gone, meal left untouched on the table.*
>
> *Victoria Police historian and former inspector Ralph Stavely says Huggins was "extremely intelligent, patient and hugely thorough". Before computers simplified the most difficult forensic problems, Huggins used his trained eye and skilled hands to crack the case of the cracked skull. A*

In Christ's Hospital Band in war-torn London, 1945.

man was found with fatal head wounds. A shattered vase was nearby. Piece by broken piece Huggins reconstructed the vase and found fingerprints on the neck that showed the offender used it as a weapon to bludgeon his victim. As well as the prints, Huggins found a button from the offender's shirt at the crime scene.

So respected was Huggins that he was called in to lead the 1986 crime scene examination when baby Azaria Chamberlain went missing near Uluru.

In Victoria, Henry pioneered the analysis of shoe-prints, tyre-prints and tool-marks. In the Fine Cotton scandal, he took casts of hooves on the racetrack to establish that a 'ring-in', a substitute horse, had raced instead of Fine Cotton. He used a comparison microscope to identify ballistic evidence, and when the microscope was replaced by a digital instrument, he arranged for it to be donated to the Institute.

Henry remembered the digging up the bodies of Douglas and Isabel Wilson, victims of hitman James Bazley, at Rye back beach. When seeing the X-Rays of the Institute's child mummy, he commented that it looked as if it had been buried briefly in a shallow grave. One incident that Henry was proud of was described by Silvester:

It looked for all the world a murder. The man was found with a fatal gunshot wound to the head, with the gun several metres away. Henry Huggins decided to test bite marks on the rifle stock, matching them to the dead man's dog, "Macca" – leading to the conclusion that the loyal German Shepherd dragged the gun away to try and protect his owner. The Coroner concluded it was suicide.

Henry had many interests and activities. He was a practising Anglican. At All Saints Clayton he was the choirmaster, and he spent his last forty years at St John's Diamond Creek. He was a leader at youth camps on Raymond Island and he sailed dinghies and catamarans. He traced his relatives, spending time tracking them down in counties such as Dorset. He always had a good workshop for wood working. Astronomy was another interest, but after retiring, it was archaeology that was his main passion.

He attended The University of Sydney's training excavation at Pella, Jordan, and gained the reputation as one of the best trowellers that the dig had seen. He also excavated in Melbourne at Spring Street and Viewbank, in Romania, Cyprus, Uzbekistan, China, Albania and at Winton, Qld.

With Sir Tony Robinson and the comparison microscope at Glenrowan, Victoria, 2015.

At Glenrowan in northern Victoria, Henry analysed the ballistics for an excavation directed by Adam Ford. The dig was filmed with Sir Tony Robinson of *Time Team* renown. After years of courtroom cross-examination, Henry had become very cautious with results, so we were treated to a scene where Tony put his arm around Henry encouraging him to concede that, since no-one's life depended upon it, there may be a possibility that ... ... Tony failed.

Christopher J. Davey

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The Old Testament encounters Archaeology: the controversy between Sayce and Driver

Christopher J Davey

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Abstract: The context and content of the debate between Archibald Henry Sayce, Professor of Assyriology and Samuel Rolles Driver, Regius Professor of Hebrew, Oxford University, are described. The inter-related backgrounds of both men are discussed. While Sayce's criticism was focussed on the literary analysis of the Old Testament, Driver criticised Sayce's aims, which he had misunderstood. The debate revealed that different methodologies were applied by the two men and these reflected the distinct mindsets associated with archaeological research and biblical studies.

Introduction

When Henry Layard published *Discoveries in the Ruins of Nineveh and Babylon*, he was able to list fifty-five names of kings, countries and cities known from the Old Testament that had been cited in recently discovered Assyrian documents (1853: 626-8, table 2). For the first time the biblical narrative could be compared with contemporary accounts and churchmen, such as George Rawlinson, saw the danger of non-theologians meddling in biblical matters. He was pleased when, in 1849, his older brother, Henry, returned from Baghdad where he had been working on the decryption of cuneiform, the script of the Assyrian documents that Layard had found at Nineveh and Nimrud; he hoped that Henry would cease his work. However, the decipherment had already been largely accomplished by another churchman, Rev Dr Edward Hincks (Sayce 1908: 19-23; Larsen 1994: 217-25; Collins 2021: 31-6), and it was he who supplied the Assyrian translations that Layard relied upon.

George Rawlinson became the Camden Professor of Ancient History at Oxford in 1861, a position he held until 1889. He seems to have embraced his brother's interests because the 1880 edition of his *Historical Illustrations of the Old Testament* (1871) lists: the Gilgamesh Flood story, the Sheshonq inscription at Karnak, the Moabite Stone and the Black Obelisk, amongst other material, as primary archaeological sources for the Old Testament period.

Prior to this, John Gardner Wilkinson had published *Manners and Customs of the Ancient Egyptians* (1835), three very popular volumes that described the culture of ancient Egypt as it was revealed by Egyptian tomb images. Another widely read book, William McClure Thomson's *The Land and the Book* (1859) detailed geographical and cultural information derived from his experience living and travelling in Palestine and Lebanon. Historical geographers such as Edward Robinson (Robinson & Smith 1841) and Arthur Penrhyn Stanley (1856) had explored many locations mentioned by the biblical text and had identified numerous biblical sites from current Arabic place-names (Rainey 1988). In 1873 a twelve volume set of translations of ancient Assyrian and Egyptian documents entitled *Records of the Past* was published under the editorship of Archibald Sayce and

Figure 1: *Archibald Henry Sayce, from the frontispiece of* Monument Facts and Higher Critical Fancies (1904).

with the support of Samuel Birch of the British Museum (Sayce 1873b; 1923: 89). These publications gave people reliable information about the Old Testament world from ancient non-biblical sources, enabling them to understand the biblical narrative in its historical context.

Meanwhile, Julius Wellhausen published *Prolegomena zur Geschichte Israels* (1878) which encapsulated many years of critical study of the Hebrew Bible. It was soon published in English, as *Prolegomena to the history of*

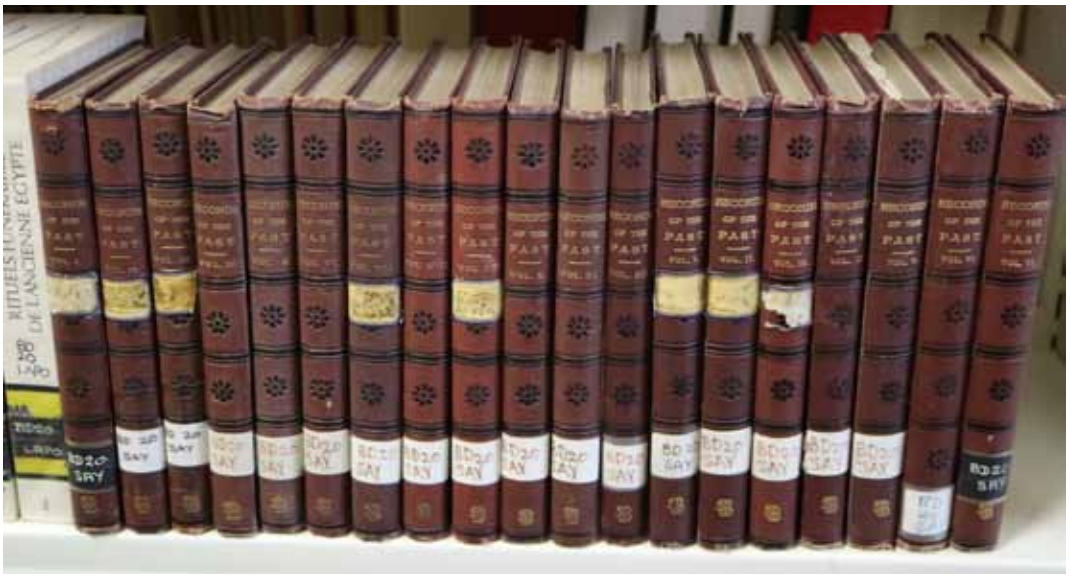

Figure 2: *The Institute's set of* Records of the Past, *which was edited by A.H. Sayce. The first series of twelve volumes was published in 1873 and the second series began in 1888.*

Israel (Wellhausen 1885), and presented what is generally known as the Documentary Hypothesis. This theory argues that the Old Testament was collated from several sources that were written during the Israelite monarchy and after, to address religious and political concerns of the respective times. This theorising about sources for which there was no physical evidence became known as 'higher criticism' to distinguish it from textual or 'lower' criticism.

From at least as early as 1753 it was deemed that such a hypothesis was needed because it was believed that Moses and the ancient Israelites could not have had the linguistic or religious ability to compose the Old Testament (Rogerson 1983: 16). For Wellhausen, the two earliest sources, J (Yahwist) and E (Elohist), were derived from oral tradition written down during the Israelite Monarchy, while the D (Deuteronomist) source was written later in Jerusalem and P (Priestly) was written still later in Babylon (Wellhausen 1885: 336). During the nineteenth century archaeology and geology had established that the world was older than six thousand years, and that writing had been common for thousands of years before the Old Testament was purported to have been written but biblical critics had continued to labour away largely on the basis that little had changed. For them, the Old Testament was based on numerous sources written hundreds of years after the events they described. As a result, the accounts were deemed to be historically unreliable and only meaningful to scholars, amongst whom there continues to be debate. Their methods lacked objectivity (Rogerson 1984: 11), no fragments of the sources had ever been discovered, and the scribal literary traditions envisaged had no known ancient parallel. These directions of study, historical/archaeological and higher criticism, would inevitably come into conflict and in England that came about at the turn of the century in the debate between two eminent Oxford scholars, Professors Sayce and Driver.

Archibald Henry Sayce

Archibald Henry Sayce (1845–1933) was born at Shirehampton, near Bristol, the son of an Anglican Clergyman (Figure 1). Both his parents had distinguished Welsh ancestry. We know much of his personal life from an engaging autobiography entitled *Reminiscences* (1923). His education was delayed because as a child he had not been expected to live. When his education did begin, he was reading Greek and Latin by the age of ten, but it was the *Arabian Nights* that intrigued him and led to a life-long fascination with the East. His poor health meant that winters had to be spent indoors, and when he became an adult, he travelled to places with warmer climates, such as Egypt.

His autobiography shows him to have been an inquiring child who could make useful friends both with poor boys his own age and also with retired university professors. As someone who was occasionally not far from death, he thought about the serious issues of life, 'I knew, as I knew nothing else, that everything is determined beforehand, and that whatever happens–at all events to oneself–is in accordance with the decree of an inexorable and passionless fate' (Sayce 1923: 18).

During his adolescence he read Hebrew, learnt some hieroglyphs and cuneiform, failed at mathematics, preferred geology to chemistry and pored over Wilkinson's *Manners and Customs* until he almost knew it by heart. He also fell under the spell of Bishop Colenso of Natal and his book on the Pentateuch, which queried its antiquity and reliability. With his father he attended the British Association at Bath where he heard the pioneering geologist, Lyell, and African explorers, Burton, and

Figure 3: *At Meroe, Sudan, 1911. From the left, Samuel Bey, Lord Kitchener, Lord Reginald and Lady Wingate, Archibald Sayce, and John Garstang. Sayce had been working with Garstang at Meroe since 1908. He and Kitchener had been personal friends from the 1870s when Kitchener participated in the Palestine Exploration Fund's Survey of Western Palestine. Courtesy Garstang Museum of Archaeology, University of Liverpool.*

Speke, who were friends of his father. In 1865 he won a scholarship to Queen's College, Oxford, to the horror of his parents; Queen's had an unsavoury reputation. He graduated in 1868 and became a Fellow of Queen's the following year, a position he held for 64 years until his death, during which time he occupied the same rooms. His research began with the publication of an Assyrian Grammar (1872a) and the presentation of papers on Semitic philology (1872b), where he identified what became known as the Sumerian Language (1923: 156), Karian Inscriptions (1873a), and Babylonian Astronomy (1874). He also edited a publication of translations of ancient texts, *Records of the Past* (1873b) (Figure 2).

His first university appointment was in 1876 as the Deputy Professor to Max Müller in Indo-Germanic Comparative Philology. While holding this position he wrote *Introduction to the Science of Language*, two volumes that emphasized the principle of partial assimilation and the linguistic principle of analogy (Langdon 1932-33). He became an ordained Anglican clergyman and was ultimately recognised for his contribution to Hittite studies. In 1890, Sayce resigned from his teaching responsibilities at Oxford and took up residence on a *dahabia* (sailing houseboat) on the Nile. The following year he accepted the newly created Chair of Assyriology at Oxford, a position that had no teaching obligations, which he occupied until 1919 (Sayce 1923: 280).

He was a regular participant and visitor at archaeological sites and excavations in Egypt and Western Asia. He was the first person to copy and translate the Siloam Inscription (Sayce 1881; 1923: 192) and, as an archaeologist, he dug with Schliemann at Mycenae, Petrie at Tell el-Amarna and Thebes in Egypt, and Garstang in the Sudan (Figure 3). He spent time with Mariette at Saqqara, he displayed enthusiasm for William Ramsay's explorations in Anatolia and undertook his own excavations at Elkab in Egypt.

Sayce was a founding member of the *Society of Biblical Archaeology*, over which he presided from 1898 until it was absorbed into the *Royal Asiatic Society* in 1919. He was also member of the *Royal Asiatic Society* from 1874 and a founding member of the *Society for the Promotion of Hellenic Studies.* Sayce appears to have had personal relationships with most scholars who were active in ancient Near Eastern archaeology and history. His views may be expected to reflect the vanguard of mainstream thinking of his archaeological colleagues, to whose publications he often contributed with translations of ancient texts discovered in the course of excavation.

Stephen Langdon, Sayce's successor at Oxford, wrote one of the more incisive tributes to him, which said in part:

> *He was a gentleman to the manner born, and never in his long life, either in the heat of controversy or in social life, did he ever lose the manner of*

a well bred and distinguished man ... [He was] recognized by Lenormant and Paul Haupt as having laid the foundations of Sumerology ... He was the first to publish an Assyrian Grammar (1872a), and the first interpreter of Babylonian astronomical texts. A large number of popular books on Babylonian Religion and the Old Testament came from his pen ... His memory was phenomenal. Even during the last week of his illness at Bath, he annotated a Ras-Shamrā Phoenician text with Hebrew, Phoenician, Arabic, Accadian and Egyptian cognate words entirely from memory ... But any subject lost its interest for him as soon as the period of decipherment was passed. That is why he never became a great specialist in any subject. His métier was that of a decipherer of any thing new, and his mind was restless unless he had some unresolved problem to work at ... His genius simply overwhelmed his capacity to specialise ... It was astonishing how he always spoke well of the good work of men who soundly derided him (1932-1933).

Sayce could write in over twenty ancient and modern languages, so the expectation that he specialise was rather unreasonable. However, he did hope to become the Regius Professor of Hebrew at Oxford, a Chair established in 1546 with royal patronage. In 1883 appointments were still made by the Monarch (Queen Victoria) on the recommendation of the Prime Minister (William Gladstone) who would put forward two names, one of whom was considered traditional or 'safe', while the other was known to be more adventurous and unlikely to be chosen. Sayce later described the situation and his disappointment:

Dr. Pusey died in September 1882, and the Hebrew Chair at Oxford became vacant. For some years past he had assumed that I should be his successor, and had more than once told me so. But the Conservative Government was now out of office, and though Gladstone and I were personal friends I was now regarded as one of the leaders of "German" critical theology at Oxford, and I knew that he considered me to be "unsafe." I had powerful friends, however, in the orthodox camp as well as the support of Chenery, and I had thus come to believe that the Regius Professorship would be offered to myself. Liddon called one day, however, and after telling me that Gladstone "would listen to no arguments," showed me a letter from him which put an end to all my hopes. It was a disappointment to me, as I had dreamed of making the professorship a means of introducing the study of Assyrian into the fast-closed ranks of British scholarship. But the disappointment was in some degree lessened by the fact that the professorship would have prevented my spending the winters in a southern climate, and as things

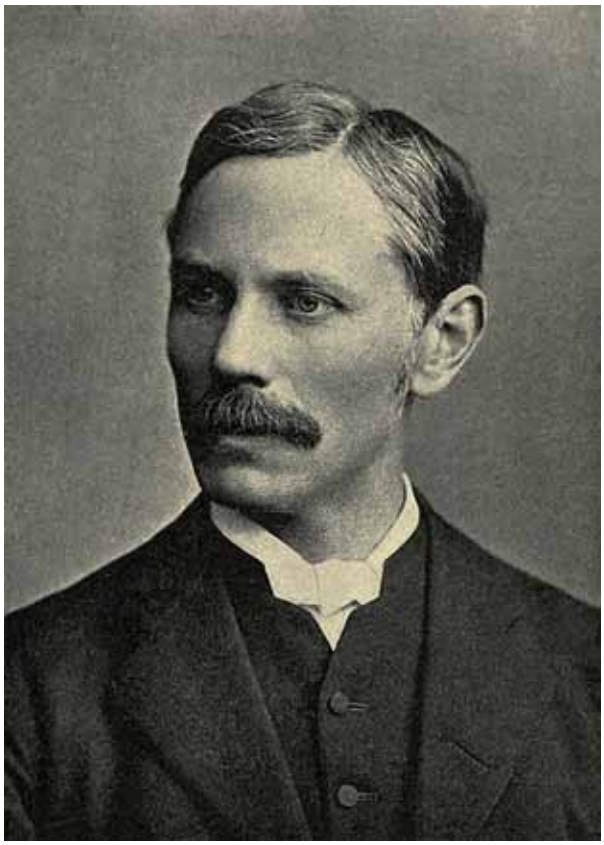

Figure 4: *Samuel Rolles Driver.*

then were that would probably have meant a shortened life. Moreover, the appointment of Driver to the Chair more than reconciled me to Gladstone's choice: Driver was one of the best, if not the best, Hebraists in the country, and from the point of view of the Hebrew specialist as opposed to the general Semitic scholar was a better choice than I should have been. Little did either Gladstone or myself then foresee that the time would come when Driver would be the protagonist of "German" higher criticism, and I should be regarded as a champion of orthodoxy, or that in the nineties Gladstone would be my associate in writing an introduction to an American illustrated Bible, and express his regrets that he had "listened to" other "counsels," and not given me the Oxford Chair of Hebrew (1923: 213-4).

Samuel Rolles Driver

Samuel Rolles Driver (1846–1914) was born to Rolles and Sarah Driver, at Southampton (Figure 4). They had a Quaker heritage. He was educated at Winchester College as a commoner, and in 1865 he received a scholarship to read Classics at New College, Oxford, however, it was Hebrew that occupied much of his study, being awarded the Pusey and Ellerton scholarship in 1866 and the Kennicott scholarship in 1870, both for Hebrew.

Hugh Williamson, Emeritus Regius Professor of Hebrew at Oxford, has commented that Driver's main Hebrew

teacher at Oxford was probably Adolf Neubauer (1831–1907) who was a sub-librarian at the Bodleian Library and from 1884, was reader in Rabbinic Hebrew at Oxford (Pers. comm.). Neubauer had been engaged to catalogue the Hebrew manuscripts in the Bodleian Library. In 1876 he published *Jewish Interpretations of the Fifty-third Chapter of Isaiah*, and the following year, translations were published jointly by him and Driver. In his tribute to Neubauer, Driver wrote what probably reflected his own experience: 'At Oxford he stimulated and encouraged the studies of younger scholars. By example and precept he taught the importance of independent research' (1912). Sayce also had a close relationship with Neubauer taking regular Sunday afternoon walks with him on which they discussed 'Old Testament criticism and Semitic philology' (Sayce 1923: 54).

Driver is known to all students of Hebrew because of his contribution to *A Hebrew and English Lexicon of the Old Testament*, more commonly known as Brown–Driver–Briggs or BDB. It was first published in 1906 and was based on the Hebrew-German lexicon of Wilhelm Gesenius. The chief editor was Francis Brown, with the co-operation of Driver and Charles Briggs.

Among Driver's numerous works are commentaries on Samuel (1890); Leviticus (1894); Deuteronomy (1895); Joel and Amos (1897); Daniel (1900); Job (1906a); Jeremiah (1906b); the Minor Prophets (1906c); Genesis (1907); and more general works including: *Treatise on the Use of the Tenses in Hebrew* (1874); *Isaiah, his Life and Times* (1893); *The Parallel Psalter* (1898); and *Modern Research as illustrating the Bible* (1909). Driver contributed significantly to the genre of biblical commentating.

Driver became the Regius Professor of Hebrew in 1884 and held the position until his death in 1914. According to Rogerson, it was 'Driver who became the leading advocate of a scholarly, cautious, yet totally committed Wellhausenian type of critical scholarship' (1984: 282). He traced the development of Driver's thought in *Sermons on subjects connected with the Old Testament* (1892) and in *An introduction to the literature of the Old Testament* (1891).

The Sayce Opening

Sayce's perspective on archaeology and the Bible is discussed by Thomas Davis (2004: 23-7). He traces its origin to Schliemann's experience at Troy and its connection with Homeric literary criticism. When in 1873 Schliemann announced that he had found the ruins of ancient Troy at Hissarlik, he called into question the prevailing scholarly scepticism about its existence.

In *Fresh Light from the Ancient Monuments* Sayce claimed, 'What striking confirmations of the Bible narrative have been afforded by the latest discoveries will be seen from the following pages' (1884: 3). His first example was the Hittites, whose existence had been

Figure 5: *An el-Amarna Letter, from Yapahu, King of Gezer, to the Pharaoh complaining about harassment from the Hapiru. Sayce was directly involved with the identification and translation of the collection of tablets (1923: 258, 262). E29832, 10.8 x 9.5cm, courtesy of the Trustees of the British Museum.*

doubted but, with Sayce's contribution, their records and ancient references to them had been discovered. It is hard to know what other than a 'confirmation' this occurrence may be called, but some scholars have interpreted this to have been Sayce's primary purpose. But the situation only arose because, in the absence of external evidence, the Hittites had been deemed not to exist. In fact Sayce was declaring scholarly speculation to be in error rather than the Bible to be right. More importantly, Sayce goes on to explain that archaeological evidence has 'a further interest than a merely historical one' (1884: 4). His real concern was hermeneutical, the establishment of meaning, which could be highlighted by the ancient world context as revealed by archaeology. By introducing the historical question, scholars had diverted attention from this.

While Sayce had a sophisticated view of archaeological 'facts' and was prepared to follow archaeological evidence wherever it went, Davis detected an apologetic strain (2004: 27), that is a defence of religious doctrine, in at least one of his later popular writings, *Monument facts and higher critical fancies* (1904). However, Driver and many later biblical scholars, such as Elliot (2003), have dismissed Sayce because they deemed his position to have been entirely apologetic. Their view was formed from a selective and self-conscious reading of Sayce.

In 1894 Sayce published *The "Higher Criticism" and the Verdict of the Monuments* in which he questioned the hypotheses of biblical critics and their apparent ignorance of archaeological evidence. In particular, Sayce had in mind the recently discovered Tell el-Amarna letters that revealed a sophisticated literate diplomatic relationship between Egypt and the southern Levant at the time of the Moses narrative (1894: 49; Figure 5). This discovery, amongst numerous others, in Sayce's view, eliminated the grounds that required the Documentary Hypothesis:

> *But the fault lay not with the "higher criticism" but with the "higher critic." He [the critic] had closed his eyes to a most important source of evidence, that of archaeology, and had preferred the conclusions he had arrived at from a narrower circle of facts to those which the wider circle opened out by oriental discovery would have forced him to adopt. There are popes in the "higher criticism"* (1894: 6).

It should be noted that Sayce always referred to the advocates of the Documentary Hypothesis using inverted commas because he objected to their use of the term 'critic' since it implied that other scholars, such as himself, were uncritical and unscientific (1904: Preface). The critics had sought to divide the text of the Old Testament into its original sources by applying a theological hypothesis. Sayce considered this to be pointless as many ancient texts, which were potential Old Testament sources, were being discovered and made known to a general readership. They believed that the interpretation of the Old Testament was a matter for university academics, who Sayce deemed to be 'popes'. When introducing the seventh edition, which ran to over 600 pages, Sayce wrote:

> *In no single case have the so-called "critical" theories been confirmed; on the contrary, wherever they could be tested by archaeological discovery, they have been proved to be groundless. Those of us who are in the forefront of archaeological research in its relation to the Old Testament stand in a very different position to-day from that which we occupied fourteen years ago* (1910: xiv).

Like the critics, Sayce believed that the Old Testament had been compiled from sources. His concern was that the conjectured sources derived from textual analysis that bore no relation to historical and archaeological evidence (1904: 44-5). His explicit criticism of the Documentary Hypothesis is found in *The Early History of the Hebrews*:

> *The huge edifice of modern Pentateuchal criticism is thus based on a theory and an assumption ... The theory, however, is philological, not historical. The analysis is philological rather than literary, and depends entirely on the occurrence and use of certain words and phrases ... A passage which runs counter to the theory of the critic is at once pronounced an interpolation, due to the clumsy hand of some later ' Redactor.'... Its very complication condemns it ... It deals with the writers and readers of the ancient East as if they were modern German professors and their literary audience* (1897: 105-8).

While Sayce was writing *The Early History of the Hebrews,* an Old Babylonian (c 1800 BC) version of the Akkadian Flood story was discovered. Critics had allocated the biblical flood narrative (Genesis ch. 6) to two sources, J and E, but the much earlier Akkadian text already had the combined text, which had been accurately copied down to the seventh century BC text that George Smith had discovered in the British Museum's Nineveh collection (1873). 'It is difficult to see … how 'literary analysis' of the Pentateuch can be any longer maintained' Sayce wrote (1897: vii). He took this matter up in *Monument Facts and Higher Critical Fancies*:

> *When we compare this story [the Deluge] with the account in Genesis, we find that it agrees not only with the so-called Elohistic version, but with the so-called Yahvistic version as well. It thus presupposes an account of the Deluge in which the Elohistic and Yahvistic elements were already combined together. And since it was written some centuries before the birth of Moses, there are only two ways of accounting for the fact, if the narrative in Genesis is really a composite one. Either the Babylonian poet had before him the present text of Genesis, or else the Elohist and Yahvist must have copied the Babylonian story on the mutual understanding that the one should insert what the other omitted. There is no third alternative. It follows from all this that the 'critical' method is scientifically unsound, and its results accordingly will not stand the application of a scientific test* (1904: 20-1).

Sayce offered a textual comparison of the Babylonian and biblical flood stories later in the book (1904: 47-52). Such a contrast has the potential to highlight the meaning of the text. Meanwhile, biblical scholars have continued to debate the sources of the Old Testament flood narrative, which are now thought to be J and P, although there is no agreement about their contents or their relationship to the ancient versions of the flood story (Wenham 2002: 169). Sayce also compared the Mosaic law and the Hammurabi Code, which had been discovered less than three years earlier (ch. 5). He considered that while the law codes had striking parallels, they were fundamentally different in character. He saw this discovery to be another nail in the coffin of the Documentary Hypothesis that had envisaged a much later date for Mosaic Law, 'It has for ever shattered the 'critical' theory which would put the Prophets before the Law, it has thrown light on the form and character of the Mosaic code, and it has indirectly vindicated the historical character of the narratives of Genesis' (1904: 87).

Driver's Attack

Driver delivered the inaugural Schweich Lectures in London in 1908 under the title *Modern research as illustrating the Bible* and demonstrated a good knowledge of recent archaeological discoveries. In the third lecture he stated:

> *To understand properly an ancient literature such as that of the Bible we need all the help and light that we can get from whatever quarter—from philology, from criticism, both documentary and historical, from many special studies, such as geography, geology, botany, zoology, from the observation of customs in Bible lands, and also from archaeology. The special value of archaeology consists in the fact that it affords us, in most cases, contemporary evidence; and hence in a most welcome manner, as the case may be, illustrates, supplements, confirms, or corrects, statements or representations contained in the Bible. It co-operates with documentary — otherwise, though not very clearly, called 'higher' — criticism, in helping us to distinguish narratives in the Bible which are contemporary with the events recorded from those which are of later date, thereby assisting us to place its different parts in their true historical perspective. We must, however, be on our guard against confusing, as is sometimes done, the facts of archaeology with the ingenious, but precarious, inferences or hypotheses sometimes founded upon them. Archaeology is moreover of value, as nothing else is, in enabling us to construct pictures of the civilizations by which Israel was surrounded — the imposing empires of Egypt, Babylon, and Assyria — perhaps before long we may be able to add the Hittites—and those of the smaller, but by no means unimportant, tribes or nations, neighbours of the Hebrews, in Arabia, Syria, and Phoenicia* (1909: 89).

Sayce had been putting most of what Driver was advocating into practice and may not have considered this to be a criticism of him. Indeed, Sayce's work contributed significantly to the subsequent recognition of the Hittite Empire. His expressed opinion was that the ancient Near Eastern documents, most of which were in languages unknown to Driver, did not support the Documentary Hypothesis.

Driver had specifically criticised Sayce in an earlier essay in *Authority and Archaeology* (1899) edited by David Hogarth. After a survey of archaeological evidence relating to the Old Testament, Driver concluded:

> *What Professor Sayce has done is firstly to draw from the monuments a picture of Palestine as it was in pre-Mosaic times, then to work the history of the patriarchs into it (chap. iv), and having done this, to argue, or imply, that he had proved the historical character of the latter! It is, of course, perfectly legitimate for those who, on independent grounds, accept the historical character of the narratives of Genesis to combine them with data derived from the monuments into a single picture: but those who undertake to prove from the monuments the historical character of the narratives of Genesis must, at all costs, distinguish carefully between statements which rest exclusively upon the authority of these narratives, and those which depend upon the testimony of the monuments; if they fail to do this, misunderstanding and confusion will inevitably result. Professor Sayce, unfortunately, often neglects this distinction; and confuses the illustration of a narrative, known, or reasonably supposed, to be authentic, with the confirmation of a narrative, the historical character of which is in dispute* (1899: 149-50).

Driver never answered Sayce's well-founded criticisms of the Documentary Hypothesis. Instead, he assumed that Sayce's main aim was to 'confirm the narrative' and then criticised him because his methodology was not appropriate to do that. This process 'cancelled' Sayce, to use a modern expression, and meant that his criticism could be ignored. When discussing the debate, Roger Moorey, a Keeper at the Ashmolean Museum, stated:

> *As Driver recurrently pointed out, although it may be legitimate for those who accept the historical character of the Old Testament narratives on independent grounds to combine them with archaeological information to present a composite picture, it is not legitimate to regard this, as Sayce and those of his persuasion often did, as proof of historicity* (1991: 44).

But Sayce never claimed to be proving historicity. The founder of the Australian Institute of Archaeology, Walter Beasley (1889–1976), enthusiastically annotated books by authors, such as Melvin Kyle, who sought to prove the biblical narrative (Davis 2004: 77). He owned most of Sayce's books and none of those which discuss the 'higher critics' have any marginalia indicating that he knew that Sayce did not share his own desire for proof. It is disturbing that a man like Beasley with a socially disadvantaged eighth-grade education could detect what the Regius Professor of Hebrew at Oxford could not. However, Driver's description of Sayce's method is largely correct and is discussed below.

Counterattack

Sayce's stated aim was to 'prove the historical character' of the narratives by studying the archaeology of the ancient world. He was arguing for the narratives' circumstantial and cultural plausibility, not their historicity. Driver and many other biblical scholars, and some archaeologists such as Moorey, assumed that Sayce shared their own concerns about authenticity. Driver, for

example, distinguished between archaeological evidence that was *direct* and *indirect*. Direct evidence 'is of the highest possible value, and, as a rule, determines a question decisively' (Driver 1899: 143). Sayce had no need to make the distinction because he did not set out to authenticate events; he aimed to ensure that academic analysis appropriately considered all the evidence that may contribute to the narrative's meaning:

> *Commentators have been more anxious to discover their own ideas in them, than to discover what the statements contained in them really mean. It is indeed strange how seldom we think of even trying to understand what a passage of Scripture must have originally signified to the author and his readers, or to realise its precise meaning* (1894: 26-7).

Driver's survey of archaeological evidence (1899) listed names of people and places and linguistic parallels, but he made little reference to cultural, political and religious practices, which were not directly useful for authentication. Sayce by comparison was interested in more than inscriptions (1904: 13). In the later Schweich Lectures, Driver also ventured into material culture, and evidence for occupation (1909).

Driver did not want the autonomy of his discipline to be compromised, but he was interested in archaeological information to enhance the study of the Old Testament. Meanwhile, Sayce was criticising biblical critics for their approach, which had limited their capacity to utilise properly the data from archaeology:

> *But where 'criticism' went wrong was in its belief that, unaided, it could solve all the problems of history. The result was the adoption of a false method, resting, in default of anything better, on assumptions and theories which have been shown to be without foundation, an exaggerated scorn of tradition, and a neglect of those facts of archaeology which are the only scientific criteria we possess for testing the truth of the traditions of the past* (1904: 123).

Sayce was primarily concerned about 'traditions' not events and historical figures. He appreciated the limitations of the evidence, especially those of ancient written sources:

> *Hebrew is a language that is very imperfectly known; it has long ceased to be spoken; only a fragment of its literature has come down to us, and that often in a corrupt state; and the meaning of many of the words which have survived, and even of the grammatical forms, is uncertain and disputed* (1904: 19).

While the Regius Professor of Hebrew at Oxford conceded that to understand the Bible 'we need all the help and light that we can get from whatever quarter' (1909: 89) it was the framework and integrity of that help that concerned him. He attacked, Sayce, the Professor of Assyriology, because he had not adopted the methodology that he considered appropriate for biblical studies. Sayce criticised critical biblical scholars because they had ignored the emerging archaeological evidence and constructed a pointless hypothesis. Both Driver and Sayce knew that biblical studies needed archaeology, but they disagreed on the character of the relationship.

Strategy: Managing Uncertainty

By 1910 Sayce had worked with many archaeologists and he described them:

> *The archaeologist is happily attached to no party; he has no theories to defend, no preconceived theory to uphold. He is bound to follow the facts brought to light by the progress of discovery and research, wherever they may lead him. Whether they support the views of the "higher critic" or of the upholders of traditional opinions is no concern of his. His duty is to state and explain them regardless of their consequences for theological controversy. All he is bound to do is to point out clearly where practical certainty ends and mere probability begins, where the facts tell their own tale and where their broken and dislocated character demands the hypothesis of the interpreter* (1910: 28).

Archaeologists knew that their evidence was often ambiguous, and Sayce considered it their responsibility to evaluate its accuracy:

> *To this imperfection of the record must be ascribed the frequent cases in which we are obliged to use terms like "probable" and "it seems," and to suggest an inference instead of proving it mathematically. No doubt future research will diminish the number of such cases; nevertheless there must always remain instances in which the amount of certainty rarely attainable in historical investigations as in common life can never be arrived at. We must be content with probability only. Still probability is better than the bare possibility which the 'critic' so often extracts from his inner consciousness* (1910: vi).

Coping with uncertainty is a fact of human existence. Years ago, before GPS, I was a recreational pilot who flew in the Australian outback. The charts we used were stamped with a letter indicating their level of reliability, which in our case was normally 'B' meaning that features may not be truly plotted. Railway lines and roads, of which there were few, could be relied upon, but other features were less accurate. To use these charts successfully it was important not to rely on the identification and location of any one feature. Instead, we would build up a picture with a sequence of features, drainage lines, vegetation boundaries, tracks, cattle station buildings etc, on the ground coinciding with a similar combination of features

Figure 6: *Salvador Dalí, Galatea of the Spheres, Oil on canvas, 1952.*
© Fundació Gala-Salvador Dalí, used with permission.

on the chart. The existence or absence of any one feature on the ground or on the chart was not grounds for rejecting the overall combination. By this method we gained an indication of our location.

The modern science of Quantum Mechanics provides useful scientific models for historical research in environments of uncertainty. Earlier models based on Newtonian Physics assume reliable data and remain common in the humanities and social sciences, but Sayce was applying a more sophisticated model closer to Quantum Mechanics. It is a complex scientific construct for unseen particle and energy behaviour at the sub-atomic level. It includes the Heisenberg uncertainty principle which states that properties of particles, such as their position and momentum, cannot be known with absolute certainty. This uncertainty is due to the fact that particles can only be observed one at a time and the act of observation changes the state of the particle. The more precisely one property is known, the less accurately others can be defined, so the more certain one is about one aspect of a particle, the less one knows about everything else.

An illustration of this concept was painted by Salvador Dali in 1952 and is called *Galatea of the Spheres* (Figure 6). During this phase of his work, Dali said that he had abandoned Freud and then considered himself a 'child of Heisenberg'. The further away from the picture you stand, the clearer its subject becomes. The more one concentrates on the components of the picture, the spheres, the greater become the uncertainties about the picture itself. The shape, colour and tone of any one sphere provides no direct information about the subject of the painting. If one were to study the spheres themselves critically and to erase those that do not conform to *a priori* criteria that presume a meaningful image, a blank canvas would result.

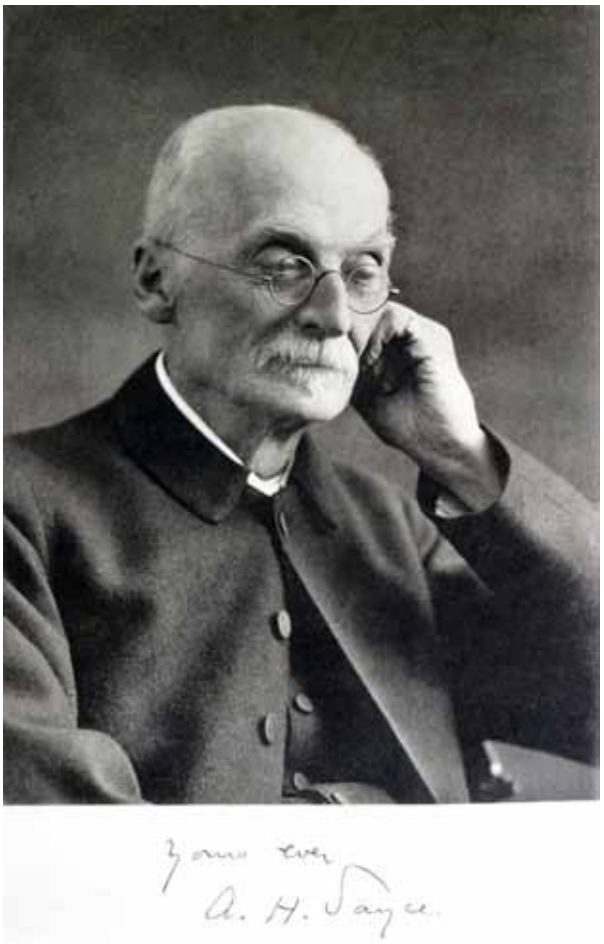

***Figure** 7: Archibald Henry Sayce from the frontispiece of* Reminiscences *(1923).*

Stalemate

A more recent analysis by Williamson focuses on Driver's disagreement with Sayce on points of detail before commenting on his methodology (2021: 29). Williamson criticises Sayce for ignoring the distinction between direct and indirect evidence and quotes Driver's criticism of Sayce's circular methodology, as cited above (1899: 149). He goes on to affirm that, 'there is, of course, plenty of room for discussion. The discussion, however, should respect the integrity of each source of historical data, as Driver clearly perceived, and not confuse them in such a way that either becomes contaminated' (2021: 31).

This approach assumes that biblical studies is always based on discrete and substantive historical information. The proposed procedure is like that used in the criminal justice system where pieces of evidence are interrogated in isolation and only admitted as evidence if they pass a rigorous examination. It requires well defined historical data often involving eyewitness accounts.

Sayce's hands-on experience of archaeological discovery and excavation, and ancient languages brought the realisation that forensic reliability could not normally be assumed in Archaeology and Biblical Studies. He therefore applied a methodology appropriate for uncertain data. The examples of unreliable flight charts and Quantum Mechanics demonstrate the importance of thinking in patterns. Sayce did not write about patterns, but he did refer to the linking of evidence and the management of the gaps in it (1904: 15-16). Very little ancient evidence can be relied upon absolutely, so it is reasonable for all the evidence, including the biblical text, to remain available for discussion to see how it may best fit together. The preferred interpretation will probably be the scenario that makes sense of the greatest amount of evidence. Dever calls this 'convergence' (2001: 83). It is a search for plausibility and meaning, not verification.

Driver did not defend the Documentary Hypothesis maybe out of a lack of respect for Sayce's criticisms, which he thought were based on a false methodology. He had an unrealistic approach that he thought Sayce should adopt for archaeology, and he did not engage constructively with the theoretical basis of the emerging field of archaeology and the epistemological frameworks in which archaeological information was obtained, analysed, and maintained. He was, after all, a specialist.

Concluding Comments

Sayce was philosophical about his life (Figure 7):

> *My span of life has been far longer than I ever anticipated ... I have been quick to see the results of evidence, but this quickness of perception, coupled with defective eyesight, has often led me to hasty and false conclusions ... My Hittite theories, as they were termed were received, to use Sir Richard Burton's words, "cum magno risu" [with much derision], and it was years before excavation finally compelled their recognition ... my attitude toward the so-called Higher Criticism of the Old Testament after the discovery of the Tell el-Amarna tablets brought upon me showers of controversy and abuse ... the discovery of the tablets were sufficient proof to me that merely subjective criticism of literary documents was a worthless pastime* (1923: 474-5).

Langdon acknowledged that specialists, by which he meant academics, could not understand Sayce because he did not 'specialise' (1933–1934: 342). Sayce's intellect, memory and experience enabled him to study in depth nearly every aspect of historical research. He applied some aspects of future post-processual archaeological theory; he was not troubled by the inexplicable, and he was comfortable in a multi-disciplinary environment. Unlike Driver, Sayce's pioneering academic research and extensive field experience meant that he could understand the strengths and limitations of philology, epigraphy, biblical studies, and archaeology. As Langdon suggests, academics at the time found the multi-disciplinary nature of his writings difficult to appreciate.

While Sayce had devised a workable scheme for extracting information from archaeological and textual data, biblical scholarship had not. Fifty years later

another genius, William Foxwell Albright, would have his research dismissed because a critical biblical scholar, Thomas L. Thompson, deemed it to be subjective and based on 'meaningless mathematical criteria such as the "balance of probability", which itself is established by the extremely undependable principles of analogy and harmonization' (1974: 7). Driver-like, he incorrectly assumed that Albright was trying to prove the historicity of the biblical narrative and he advocated an impossible level of certainty for the acceptance of archaeological evidence but, unlike Driver, he concluded that 'archaeology can tell us nothing' (1974: 328).

Sayce was an historical pioneer while Driver was a producer of reliable Hebrew scholarship. Both roles deserve respect. Sayce wrote in the tradition of Gardner Wilkinson explaining the latest scholarly research to an informed general readership in a manner that was comprehensible. In a timely fashion and with astute comment, he introduced people to the Siloam Inscription, the el-Amarna Letters, Akhenaton, the Hammurabi Stele and numerous other artefacts. Driver produced many well researched academic tomes primarily for students of Hebrew and the Old Testament.

Driver's dismissive and disrespectful attitude to Sayce was unfortunate. As a result, Sayce has often been falsely considered by biblical scholars to have been motivated by a clumsy desire to 'prove the Bible', when in fact he was seeking to have the Old Testament understood in its ancient context. He considered that the attempts to create literary sources from an analysis of the Old Testament text by applying literary processes not known in the ancient world to be pointless; it did not satisfy the biblical scholars' need for authentication and it offered no reliable basis for understanding. This attitude has no doubt led many students to ignore Sayce's significant body of scholarship and to discourage them from studying and researching the historical context and meaning of the biblical narrative. It has also fostered a dysfunctional relationship between Biblical Studies and Archaeology, which continues to the present. It is doubtful in the circumstances that there can ever be another Archibald Henry Sayce.

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Australian ethnography and frontier history: do we know what we think we know?

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Abstract: The early European settlement of much of Australia was typified by the advance of squatters well beyond boundaries permitted by governments of the time (Reynolds 2021). This article explores existing understandings of the relationship between such a settler, Peter Beveridge, and local Aboriginal people through a critical reading of documentary resources many of which were written by Beveridge himself. This article concludes that little of Peter Beveridge's claims regarding Aboriginal people or his knowledge about their lifestyles should be accepted without careful scrutiny.

Introduction

Source credibility is a primary concern in any historical endeavour. Yet little critical examination has been carried out on many of the early historical and particularly ethnographic sources which Australian archaeologists rely on. Ethnography is frequently used to inform understandings of Aboriginal life before colonisation, and to comprehend the events and processes inherent to the mechanisms of early colonisation and its frontier. A new wave of scholarship is bringing fresh perspectives to ethnographies and early histories with results that often challenge previous consensus. Sylvia Hallam made a powerful and incisive start on this process many years ago (Hallam 1975), but her work, like that of Eric Rolls (Rolls 1981), which also derives new perspectives on land usage from existing documents, was perhaps before its time and did not spark the same engagement produced by more recent works. More recently however, such revaluation has been solidly re-initiated through a groundswell of works (Gammage 2011; Pascoe 2016; Irish 2017; Gapps 2018; Brodie 2017; Gerritsen 2008). These authors re-read and interrogate existing sources and provide new voices for the past. Some critical work has also been carried out on the role of ethnographic literature in positioning Aboriginal people as worthy recipients of government protection or suppression (Boucher 2015). But the scope of these new works is generally broad and thematic and predominantly they do not grapple with the fine-grained verifiability of the many diverse ethnographic claims regarding Aboriginal life that combine to form a primary historical resource. The documentary interrogation of written ethnographies for historical veracity has not been the subject of extended investigation. I argue that a revaluation of these sources is well overdue. A discipline-wide critique, or even a critique of the works of a single major ethnographer, is beyond the scope of a single article. What I will set out to do here, is examine what can be derived from the application of a forensic analytic approach to the writings of a minor Victorian ethnographer, Peter Beveridge (1829–1885).

Figure 1: *Peter Beveridge circa 1865. La Trobe Picture collection, State Library of Victoria H10173.*

In attempting to evaluate the accuracy of Beveridge's observations of Aboriginal life I am bound by two primary constraints. Firstly, there is an embedded bias and inequity of substantiation in that I have no access to Aboriginal documented histories and narratives against which to balance Beveridge's inherently white account. Secondly,

as a non-Aboriginal man conducting research at my own behest, it is not appropriate for me to attempt to reclaim, rediscover or retell Aboriginal narratives and stories of the past, or even postulate what Aboriginal perceptions of Beveridge may have been. Elsewhere in my practice I do make exactly such retellings and speculations in weighing historical colonialist individuals against each other. Yet, such a process is inherently inequitable in this instance. This is because in the face of the great historical and ongoing imbalances of power that are inherent between the coloniser and the colonised in Australia, the appropriation of the Aboriginal voice or speculation about Aboriginal perspective by an non-Aboriginal, is in itself I believe, an act of colonisation (Tuhiwai-Smith 2012; McNiven & Russell 2005; Land 2015). Almost certainly there will be Aboriginal history and story around Peter Beveridge, but those histories and stories belong to the Watti Watti and other Aboriginal people and are theirs, not mine, to explore and share.

Ethnography or creative writing?

Culture is not a static entity and projecting the Aboriginal lifeways that were observed shortly after colonisation back into the deep Aboriginal past is therefore an inherently fraught endeavour. Yet in many archaeological studies, ethnography provides a starting point for interpretation of the archaeological record (McBryde 1978). The field of ethno-archaeology has waxed and waned in popularity and has often been most strongly advocated by archaeologists with anthropological training, such as is common in the United States (David & Kramer 2001; Hayden 1987).

In Australia ethnoarchaeology has been most prominently applied by the American archaeologist and anthropologist Richard Gould (Gould 1969; Gould 1978; Gould, Koster, & Sontz 1971). Yet despite rises and falls in popularity of the ethnographic approach there has been a steady ongoing utilisation of ethnography in Australia. This is particularly the case for reports written by heritage consultants in assessment of known or potential impacts on the archaeological record of proposed development. In such reports a formulaic approach is usually adopted, largely dictated by the legislative and practice guidelines of the state within which the proposed development is planned to occur. Ethnography is an almost unvarying step in such assessments and is used to evaluate the range of past local Aboriginal activities and the types of archaeological evidence that may be present as a result of them. Most, if not all, of the ethnographies used in eastern Australia date from the 19th century, and little extended critical analysis has been carried out to assess their accuracy. Establishing the credibility of 19th century ethnographers is a complex undertaking yet it is surely essential if we are to continue to use their works as widely as is the case at present. In the following paragraphs I will provide a brief example of the uncertainty surrounding the works of some very well-known ethnographers before turning to an assessment of Peter Beveridge.

Martin Thomas (Thomas 2011) points out in his biography of the autodidact Australian ethnographer R. H. Mathews (1841–1918) that major Australian academic anthropologists and ethnographers of the time such as A. W. Howitt (1830–1908), Lorimer Fison (1832–1907) and Brough Smyth (1830–1899) generally did not carry out field work or observe Aboriginal people directly. Rather, they often relied on responses to mailed surveys, questionnaires, and letters from and conversations with a large number of persons living in rural areas who were or had been in contact with Aboriginal people (Thomas 2011). The accuracy of observations recorded in such correspondence, often from people with considerable motivation to portray Aboriginal people negatively, was not subject to detailed verification. E. Curr (1798–1889) was dismissive of Howitt and Fison's work as reliant on unqualified sources, and of Smyth as incompetent in the bush (Curr 1866). Yet Curr too relied chiefly on remote informers but insisted that his informers were more credible than those informers used by Howitt and Fison.

The engagement in direct observation and conversation with Aboriginal people such as Mathews characteristically carried out, was seen by academics of the time as amateur or unworthy. Partly due to his direct engagement with Aboriginal people, Mathews was frequently publicly ridiculed by Howitt and Fison (Thomas 2011). Mathews suggested to anthropologist and ethnographer, Baldwin Spencer, that the work of Howitt and Fison was inferior for not relying on direct observation – Baldwin Spencer later responded by accusing Mathews of being a 'perfect fraud' (Thomas 2011: 260). Thomas presents Mathews as a careful record keeper and prodigious publisher, and states that the anthropologist and archaeologist, Norman Tindale, had come to the view that Mathews was 'our greatest recorder of primary anthropological data' (Thomas 2011: 11) – a view shared by A. P. Elkin (Elkin 1975). In contrast Diane Barwick considered that due to his personal enmity towards A.W. Howitt, Mathews had ignored and distorted Howitt's work (Barwick 1984). Two decades after Barwick's analysis Howitt was reassessed as a far more thorough scholar than Mathews (Rose, James & Watson 2003). If we adopt a worst-case scenario that all stated criticisms are accurate, that Howitt and Fison largely relied on uncorroborated reports from biased sources, and that Mathews was untrained and inaccurate, then one might wonder whether much benefit is to be gained at all by consulting their works. Yet an image of respectability is attributed to these works which continue to be widely consulted. It is in considering this image of respectability itself that I was brought to reappraise the evidence regarding another but far less well-known 19th century Australian ethnographer, Peter Beveridge. Although perhaps not widely cited, Beveridge's works have been drawn upon by the academic archaeological community. In particular, Beveridge's descriptions of Aboriginal oven mounds (Beveridge 1869; 1889) informed studies of Aboriginal subsistence strategies (Williams 1988) and were used to support the proposition

Figure 2: *Tyntynder Homestead. Photo: M. Lever, August 2018.*

of intensification of Aboriginal subsistence activities in the late Australian Holocene (Lourandos 1977; Lourandos 1980).

Peter Beveridge – a young colonist

I will briefly critically examine Peter Beveridge's works in historical context with a view to establishing Beveridge's motivation for publishing his ethnographies and to detect any biases that his writing displays. But first, a few lines about Peter Beveridge and critical events of his life.

Peter Beveridge was a Scottish-born squatter. In 1845 at the age of fifteen he accompanied his considerably older brother Andrew and Edmund and James Kirby, two sons of a neighbour in Melbourne, in driving 1,000 cattle from southern Victoria to a place in Watti Watti country in the northwest of the State. They settled at a location 16 kilometres northwest of Swan Hill on the Murray River, beyond the areas then permitted for European settlement (Kirby 1897). There the Beveridge and Kirby brothers established the first pastoral run in the region; Tyntynder station. Guided by a Mr McDougall who had not been to the area before, the party also included two bullock drivers, two building hands and a male cook (Kirby 1897: 25). At the time of this trip, Andrew Beveridge had already obtained a degree in divinity from Edinburgh (Steele 1899) and was probably a formative mentor to the young Peter. In a retrospective work regarding the founding of Tyntynder, it was proposed that, given Andrew's degree in divinity, a large consideration in settling near the Murray was the opportunity for him to spread the Gospel of Christianity to Aboriginal people, who were favourably treated by the Beveridge family (Steele 1899). This sentiment is found on Andrew Beveridge's gravestone which states that his death was considered to be 'a loss sustained by the Christian church'. From the memoirs of James Kirby (Kirby 1897) it seems Tyntynder was under the management of the Beveridges. The ongoing role of the Kirby brothers is not well defined. Finding the conditions at Tyntynder promising, the Beveridge brothers sent for a third sibling, George Beveridge, to bring their sheep holdings and to establish the sheep station 'Piangil' about 15 kilometres from Tyntynder. In 1846 shortly after the arrival of George Beveridge, Andrew Beveridge was speared to death by local Aboriginal people at Piangil (Hone 1969). In 1847 Peter Beveridge's parents and remaining siblings joined him at Tyntnder, which was then run as a partnership between Peter and George Beveridge. The brothers initially constructed a drop-log cabin as their residence, later building a more elaborate brick homestead (Figure 2).

Despite these financially promising beginnings, the Tyntynder station was an economic failure in the long term. In 1868 the partnership between Peter and George Beveridge was declared insolvent, with court minutes reporting that the two brothers had a total of £67 in assets and £17,500 in debts. Drought was the stated cause for insolvency (*Age* 10 September 1867: 5; 25 April 1868: 2). The family left Tyntynder and dispersed, Peter to French Island and his parents to Kilmore, Victoria. In his final illness, Peter moved to his mother's house in Kilmore where he passed away in 1885, aged 56. Little is to hand on Peter Beveridge's activities after leaving Tyntynder. He had been spared the disgrace of being declared a bankrupt, due to a technicality relating to the legal definition of his activities as a squatter (*Age* 25 April 1868: 2). Nevertheless, at the time, substantial ignominy

Figure 3: *Grave of Andrew Beveridge at Tyntynder. Photo: M. Lever, August 2018.*

was associated with the status of an insolvent and it is likely that Peter Beveridge would not have continued to be a well-regarded member of wider society.

Appearances and Reality on the Lower Murray

It is with this diminished respectability and social standing in mind that I will turn to Peter Beveridge's publications. Peter saw himself as an ethnographer and claimed in works published after leaving Tyntynder that he had spent his 23 years at there in close observation of Aboriginal people. This claim is supported by at least two pieces of evidence. In 1859 the Victorian Report of the Select Committee of the Legislative Council on the Aborigines included extended interviews with persons deemed knowledgeable on Aboriginal lifeways. Peter Beveridge was a prominent respondent to questions posed by the Committee. His tone of assertive confidence in his knowledge matched the tone of other respondents, whose testimonies nevertheless frequently strongly contradicted each other. In 1863, Peter Beveridge was appointed an honorary correspondent for the Swan Hill District of the Victorian Central Board for the Care of the Aborigines (*Star* 4 May 1863: 3).

While still at Tyntynder Peter Beveridge sent an article to the Royal Society of Victoria titled *A Few Notes on the Dialects, Habits, Customs, and Mythology of the Lower Murray Tribes*. The article was read and accepted by the society in 1861 (Beveridge 1861), although due to delays in publishing it was not printed until 1868. In 1869, probably penned after leaving Tyntynder, Beveridge had a short and perhaps heavily edited piece on Aboriginal Ovens read at the Royal Anthropological Society of London, and published in the prefatory notes to that organisation's journal (Beveridge 1869). By far the longest work published during Beveridge's lifetime was his article for the Royal Society of New South Wales, *On the Aborigines inhabiting the Great Lacustrine and Riverine Depression of the Lower Murray, Lower Murrumbidgee, Lower Lachlan, and Lower Darling* (Beveridge 1883). This expanded significantly his previous piece published by the Royal Society of Victoria. These items comprise the core of a posthumously published collective work titled *The Aborigines of Victoria and Riverina as seen by Peter Beveridge* (Beveridge 1889).

Beveridge was considered sufficiently noteworthy as a colonist to be included, with a brief entry in *The Dictionary of Australasian Biography* (Mennell 1892). In his published works, Beveridge speaks of his close association with Aboriginal people, a relationship which he claimed had allowed him unprecedented access to observe all aspects of their life directly. His works certainly read as though he had gained a level of sustained insight to the lives of local Aboriginal people. When reviewing the re-publication of Beverudge's main work, *The Aborigines of Victoria and Riverina* (2008), Ryan (2011) actually called for a more positive engagement with his ethnography.

In the following sections I will apply techniques of biographical analysis to his works, with the aim of trying to perceive what can be observed of Beveridge the individual. This is a forensic biographical technique described by the biographer Leon Edel as learning to see 'the figure under the carpet' (Edel 1986). Edel uses the

Figure 4: *Tyntynder homestead complex as seen from Andrew Beveridge's grave. Photo: M. Lever, August 2018.*

metaphor of viewing a patterned carpet, where the visible pattern is that which the carpet-maker wishes the viewer to see. However, from the visible pattern, the informed and critical viewer can also discern those aspects the carpet-maker wishes to remain hidden on the underside of the carpet; the knots, knot density, twists and floats and differences in warp and weft. Biographical analysis allows one to discern information on the working and intent of an author from biographical text.

A primary technique in biographical analysis is the search for disconformities or omissions within a written corpus when compared to known events in the author's life. Such gaps often reflect acts of erasure by the author and when taken with further corroborating evidence may reflect an effort to rewrite the past in a manner that points the reader in a different direction from events that transpired. Such a gap exists noticeably in Peter Beveridge's writing about the killing of his older brother Andrew by local Aboriginal people. Peter Beveridge frequently depicts Aboriginal violence and judicial killings disparagingly. Yet not once in the 250-odd published pages written by him is there a mention of the spearing of his own brother who was buried only some tens of metres from the Tyntynder Homestead (Figures 3 & 4). At the time of Andrew Beveridge's killing, Peter would have been only seventeen years of age. It was he who recovered his brothers speared body, rode with it 15 kilometres to Tyntynder, and buried him within plain view of the homestead. It is hard to believe that these events would not have had a major impact on Peter Beveridge's emotional and mental state, and on his attitudes towards Aboriginal people as well.

I propose that this gap is a highly significant one – for it is in examining the death of Andrew Beveridge and the subsequent prosecution of his killers, that a very different picture of the Beveridge brothers emerges from the image provided in contemporary media accounts and in subsequent listings of the Beveridge family in dictionaries of biography (Hone 1969; Mennell 1892). Even at first glance, the course of events provided in media accounts of the time (*Argus* 1 September 1846: 2; 22 September 1846: 2; *Port Phillip Patriot and Morning Advertiser* 26 November 1846: 2; 18 December 1846: 2) do not cohere with the overt motives and causes that these articles attributed to the participants in events leading to Andrew Beveridge's death.

Eyewitness accounts reproduced in these media reports stated that several Aboriginal men had approached the hut of Andrew Beveridge at Piangil on 23 August 1846, announcing themselves by cooeeing as they approached, and calling for Andrew. He approached them and an argument ensued which ended when the Aboriginal men speared Andrew Beveridge in the stomach and back from close quarters. The newspapers invariably reported that the argument arose over sheep that Andrew Beveridge accused the Aboriginal men of stealing. A £10 reward was posted for the capture of Andrew Beveridge's killers, and in short order three Aboriginal men, Bulleteye, Bobby and Ptolemy were captured, accused and brought to Melbourne for trial. A trial lasting less than four hours ensued, on 25 February 1847 (Ryan E. 2016: 46). All three Aboriginal men were defended by Redmond Barry; they pleaded innocent to the charges of murder and named individuals who they claimed were responsible for the

killing. The jury reached its decision without leaving the jury box, finding Bulleteye not guilty, and Bobby and Ptolemy guilty of murder. The judge noted to the accused that the release of Bulleteye would allow him to return to the Murray and advise other Aboriginals of the consequences of such actions and that, given the actions of the court, pastoralists had no need to take the law into their own hands. As for Bobby and Ptolemy, the judge 'most impressively sentenced them to be hanged' (*Argus* 2 March 1847)

Many years later, Mitchell Beveridge—the youngest brother—–wrote of the affair:

> *As far as my knowledge goes, the natives in this district were humanely treated by the white settlers, and no provocation was given for such outrages as that in which my brother lost his life. (Beveridge M. 1911).*

There is much in this account of events that does not cohere. If these Aboriginal men were furtive sheep thieves with a debt to Andrew Beveridge, why would they loudly announce their approach? Why would they deliver killing blows to Andrew Beveridge over his anger at their theft? Fortunately, a detailed examination of the events around Andrew Beveridge's death has been recently undertaken. Ryan (2016) reveals that the underlying causes of events were actions and themes that 19th century society was not willing or able to address openly and which were left concealed beneath a thin and implausible narrative which maintained some modicum of respectability for the white actors involved.

These concealed motives are reflected in notes taken by William Thomas, the Assistant Protector of Aborigines. He visited the three Aboriginal men accused of Andrew Beveridge's murder in prison. He recorded discrepancies between the records of events that had been given in court and the version of events given to him by the accused men. They claimed that the spearing of Andrew Beveridge was motivated by the capture and sexual enslavement of Aboriginal women by staff at Piangil, and the murder of Aboriginal men who had attempted to rescue these women. They told him:

> *That the shepherds entice lubras into the huts & made the men in the [illegible] go away & because they wanted to take the women to miams, they shot 2 blacks; 1 afterwards died.* (Victorian Public record Series P1, Inward Correspondence, Superintendent of Port Phillip 19/89/400, cited in Ryan 2016)

Writing some 50 years after the spearing of Andrew Beveridge, James Kirby, who had accompanied the Beveridges to Tyntynder, also gave a chronicle of events that was not consistent with the casual killing of Andrew during an argument over stolen sheep. In his description of the establishment of Tyntynder, Kirby is emphatic that the white settlers were at first not 'in any way' molested by local Aboriginal people (Kirby 1897: 56). At some point, however, spearing of cattle commenced, then increased. Kirby did not provide a motive for these Aboriginal spearings, which did not appear to be related to food gathering. He claimed that it was as part of ongoing tension that arose when three Aboriginal men were killed by a party led by Andrew Beveridge for stealing sheep shortly before Andrew Beveridge's death (Ryan 2016). After Andrew Beveridge had sent an Aboriginal interpreter to remonstrate with local Aboriginal people, and to threaten them with reprisals for further damages to stock, the message was sent in reply that they, the Aboriginal people, would be the ones to carry out any killing – specifically of Andrew Beveridge at dawn on a defined day (Kirby 1897: 56). Kirby did not personally witness the events of Andrew Beveridge's spearing, but he and Peter Beveridge rapidly proceeded from Tyntynder to the scene of the murder at Piangil, and questioned staff there. Arriving at Piangil on 23 August 1846, James Kirby and Peter Beveridge found Andrew Beveridge dead of multiple spear wounds and his tent perforated by spears. The other staff at Piangil had been allowed to flee without harm, and 'the sheep were still in the yard' (Kirby 1897: 56).

In light of the evidence assembled above, the spearing of Andrew Beveridge does not at all sound like the result of an impassioned argument over stolen sheep. Rather, it resembles an organised and judicial delivery of punishment —'pay back'—by local Aboriginal men, for the abduction and sexual abuse of Aboriginal women, for the killing of Aboriginal men attempting to rescue their women, and for the killing of Aboriginal men because of sheep theft. This pay back was delivered at a time and place stipulated by Aboriginal men, who announced themselves and called Andrew Beveridge out. Peter Beveridge himself stated several times that among the local Aboriginal people, capital punishment by spearing was only meted out for the crime of murder (Beveridge 1883: 55; Beveridge 1889: 108). After Andrew Beveridge's death, Piangil was taken over by a Mr Byerly. James Kirby mentioned several times that the Byerlys and their cattle were left unmolested by the local Aboriginal people with whom the Byerlys enjoyed good relations (Kirby 1897: 81). In stark contrast, Kirby described how the nights at Tyntynder were spent shut up with Peter Beveridge in a log cabin, taking turns to keep for hostile Aboriginal men watch through gunports (Kirby 1897: 65). Given the peace enjoyed by the Byerlys in their neighbouring property, it seems fairly clear that the cause of conflict at Tyntynder was not cattle, sheep or white people generally, but the Beveridges themselves.

The impact of the Beveridges on local Aboriginal life had not escaped official notice. The Beveridge's station at Tyntynder was in breach of the law – as pointed out by Crown Lands Commissioner Frederick Powlett to the Superintendent of Port Phillip, Charles La Trobe, who had refused the Beveridge family request for squatting

rights there. Powlett had recommended that the lands of Tyntynder should be reserved for Aboriginal people, and that conflict with Aboriginal people was likely were the Beveridges to remain (VPRS 30/5/1-28-8 cited in Ryan 2016)

Inconsistencies and insights

If Andrew Beveridge had been involved in outrages against Aboriginal women and murder of Aboriginal men then it may be more understandable why Peter Beveridge made no mention of his older brother's death at the hands of Aboriginal men in his own writings. It is also understandable why court proceedings accepted the shaky narrative provided by the police in support of Andrew Beveridge's innocence. Andrew Beveridge's killing and associated events would have had a traumatic impact on the 17-year-old Peter Beveridge and the expression of this trauma would be expected to somehow work its way, if only unwittingly, into his writings. Although he makes no overt mention of his brother's death in his ethnographic writings, nevertheless it seems evident to me that traces of his trauma can be detected in his published writings, particularly in his attitudes towards Aboriginal women. For it is in the inconsistencies in his writings on Aboriginal women that Beveridge's prejudices, and possibly his and his brother Andrew's own actions against Aboriginal women, are most detectable, and a very different 'figure under the carpet' emerges.

Beveridge often positions himself as sympathetic to the Aboriginal race, which he depicts as dying out. Yet even where he adopts such a sympathetic attitude, he is irrationally vicious towards Aboriginal women, placing the blame for Aboriginal racial decline on their shoulders. He informs us that venereal disease was a chief cause of death and depopulation among Aboriginal people and that it had entered the Aboriginal community as a result of the 'wanton profligacy' of Aboriginal women. This is a claim which appears regularly throughout his works, often in opening passages (Beveridge 1861: 14; 1883: 22; 1889: 7, 16). Beveridge is at pains to assure the reader that this transfer of venereal disease to Aboriginal women could not have been due to white men – but was due to Aboriginal women being characteristically 'wantonly profligate' and having sexual relations with infected Maccassan and Chinese traders in northern Australia, from where Aboriginal women spread venereal disease across the continent. As if seeking to exonerate the Maccassan and Chinese from blame, he notes that Aboriginal women had a characteristic tendency to freeze in fear when sexually assaulted, thus making their rape unresisted and almost blameless (Beveridge1889: 7, 14). Apart from the odious act of blaming Aboriginal rape victims for being assaulted, the denial of white participation in sexual crimes and spread of venereal disease is untenable (Butlin 1983). Possibly most pointed though, is the question of how Beveridge was supposed to have gained the knowledge that Aboriginal women

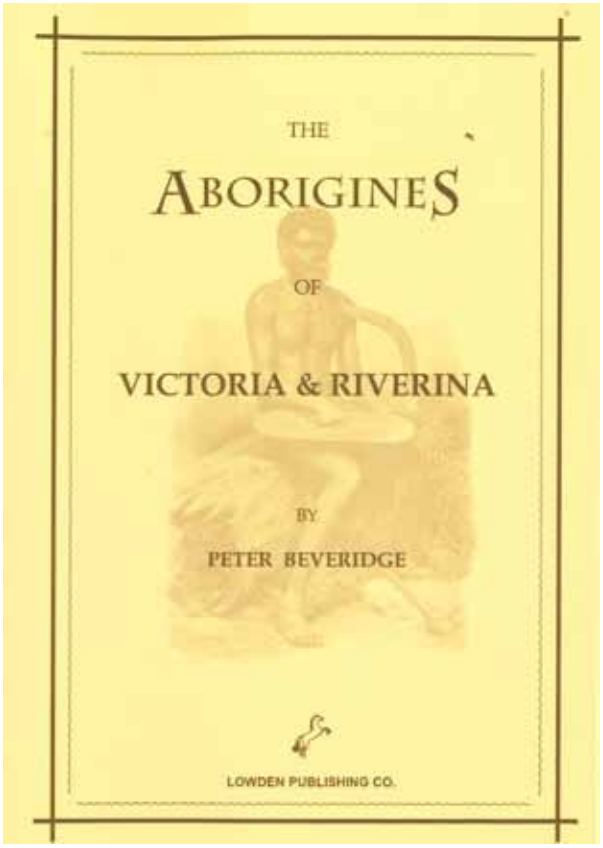

Figure 5: *Originally published postumously, Peter Beveridge's* The Aborigines of Victoria and Riverina *(1889) was republished in 2008. It has been claimed to provide 'a unique insight into Aboriginal lore, customs, daily life and very survival'.* (https://www.historyvictoria.org.au/product/the-aborigines-of-victoria-and-riverina-by-peter-beveridge/).

(apparently in the far north of Australia) tended to freeze in the face of sexual assault. Did Beveridge enjoy close communication with Maccassan and Chinese traders? Or is it possible that he was speaking here from his own personal experience or of those close to him?

Tellingly, Beveridge's earliest published piece (Beveridge 1861) contains his derision of the Aboriginal bloodline as weak in the face of European 'blood', noting that in 1861, the local Aboriginal population under the age of 15 were 'mainly from European fathers'. It was the Beveridges and their station hands who were the first whites to arrive in the area – almost exactly 16 years before this statement went to print, and who were the most likely parents of such children. Possibly in his eagerness to deride the Aboriginal bloodline as corrupted by white blood, Beveridge has implicated himself and his family as among the corruptors. In possible external corroboration of the Beveridges' activities, in 1881 a young 'half-caste' Aboriginal woman living on Ebenezer Mission had the name Rebecca Beveridge and gave her father's name as Peter Beveridge (Ryan 2016) – possible evidence of the

sexual practices, which Peter Beveridge goes to great length to claim, did not occur. Perhaps, Beveridge's frequent denigration of Aboriginal women was intended to assure his readers that he would not have been intimate with them.

The pattern of inconsistency continues, with Beveridge producing ongoing descriptions of Aboriginals which make it evident that he either had not observed Aboriginal life anywhere as closely as he would have the reader believe, or that he was simply fabricating sensationalist denigrating allegations possibly to highlight his own standing as an authority on Aboriginality. Thus, among many other risible claims he informs us that Aboriginal people were incapable of sustained physical exertion (Beveridge 1861: 18; 1883: 27), had no religion (Beveridge 1861: 18), no laws (Beveridge 1861: 21; 1889: 107), had no love of any sort (Beveridge 1883: 21, 65) and practiced cannibalism (Beveridge 1861: 22). In particular he was eager to point out on several occasions, in keeping with his general misogyny towards Aboriginal women, that Aboriginal women would often eat their infant children due to no more motivation than 'laziness' (Beveridge 1861: 22; 1889: 26, 57). By way of contrast, James Kirby, of similar age, close familiarity, and observing the same Aboriginal women as Peter Beveridge was adamant that Aboriginal people, specifically women, never practiced cannibalism (Kirby 1897: 110).

The sensationalist nature of much of Beveridge's writing and the very fact that he submitted pieces to two Australian Royal Societies, and an esteemed London journal indicate to me that he sought to gain acceptance and status in the scholarly and academic world, perhaps at the very same time that his social standing reached its nadir with his descent into insolvency in 1868. If he was left with no financial capital to show for 23 years' work in remote northern Victoria, perhaps he sought to eke out some social capital from these decades through scholarly standing based on his claimed knowledge of Aboriginal life. Such claims of knowledge would in any case be difficult to contradict. Beveridge had already experienced in his testimony to the 1859 Victorian Report of the Select Committee of the Legislative Council on the Aborigines that expert committees seemed perfectly willing to accept totally contradictory accounts of Aboriginal life so long as these accounts were voiced by white men. Such variation in accounts of Aboriginal behaviour could further be attributed by Beveridge to what he frequently claimed as the primitive incoherence of Aboriginal life, reflected in the multiplicity of mutually unintelligible Aboriginal languages in close proximity to each other.

Perhaps the clearest evidence of both Beveridge's desire to ingratiate himself and to achieve acceptance in the scholarly world, and his tendency to do so through sensationalist accounts that project sexual violence as the accepted fate of Aboriginal women, comes in fullest length of about half a page in his paper to the Royal Society of NSW (Beveridge 1883: 32) and is reprinted in shortened form in his posthumously published work (Beveridge 1889). The extract is too foul to reproduce here but is telling in two ways. Firstly, it is written in Latin. Given the young age at which Beveridge started his pastoral career, I doubt that he was capable of being the author of this piece himself, and probably had it translated for him. Writing in Latin served a clear purpose among participants in scholarly writing of the 19th century. It was a signal to readers that the subject matter was unsuitable for the general public and also that it was of such a disturbing nature that only the initiated few, lofty minds with a classical education could be entrusted with its contents. In resorting to Latin, Beveridge sought to elevate himself to the realm of educated thinkers.

I have had the text translated and checked by a Latinist, K. Conrau-Lewis, Department of Classics, Yale University. In it, Beveridge doubly confounds himself. Firstly, the Latin text is not scholarly Latin but is plagued by errors and would hardly have impressed academics of Beveridge's time. Secondly, its content is so outrageous that at the time it was almost certainly disbelieved as sensationalism. I believe it is simply a further instance of Peter Beveridge exercising a sexually violent imagination against Aboriginal women.

Peter Beveridge's youngest brother Mitchell Kilgour provides further insight to what the Beveridge household may have been like – particularly before the Beveridge parents arrived in 1857. In a whimsical aside, Mitchell recalls his mother's affront on arriving in Tyntynder to find that the female staff allocated to her service worked in the house in the nude. Victorian-era Victoria was not a place where nudity was tolerated. This observation itself raises any number of pointed questions of which I will here only raise two: what sort of person would welcome their own middle-class Victorian mother to their house with the surprise of naked domestics? What would the atmosphere and relationships of power have resembled in a small house run by two armed single young white men with naked Aboriginal female household help?

More Questions than Answers

Of the many questions raised by the analysis and information I have provided here, two queries come to the fore in their ethical imperative. Firstly, can we rely on Peter Beveridge's writings as a reflection of Aboriginal life in Watti Watti country in the 19th century? Secondly what are the implications of these findings for the ongoing and wider use of 19th century ethnographies?

With regard to the first question, I believe I have demonstrated here that the works of Peter Beveridge, noted as valuable sources of ethnographic information, contain considerable elements of sensationalist fabrications that also served as an outlet for the author's violent sexual imaginations towards Aboriginal women. His statements on a whole range of topics, from law to religion to physical properties and cannibalism are so plainly unrealistic that it is not unreasonable to ask where the line

lies in his works between fiction and fact. Why should we accept Beveridge's observations of Aboriginal subsistence activities as factual or directly observed, given not only the prejudice evident against Aboriginal people in his writings, but his quite obvious dissembling particularly around the nature of Aboriginal women and white sexual relations with them? We need to consider the relations of power within which Beveridge gained the knowledge he claimed to possess. Was such information willingly given? Did the provision of information to him result from fear of reprisal and punishment? What implications might this power imbalance have had for the truthful nature of information provided to Beveridge? And finally, whether we wish to use information that could have been gained in such manner.

As stated at the outset, it is beyond the scope of this paper to undertake a wide-scale analysis and problematisation of the ongoing reliance on 19th century ethnographers such as Mathews, Howitt and Fison. Through examination of one minor 19th century ethnographer however, this paper adds weight to the call that such an overarching reassessment is firmly required. The writings of Peter Beveridge, a frontier colonist have been examined here. These writings are purportedly the result of direct observation of Aboriginal people, yet I have found that they incorporate what are almost certainly intentionally misleading statements regarding Aboriginal people and life. My findings demonstrate the extent to which often unidentified and unstated motives can serve to skew putative primary observations of Aboriginal life. These claimed observations, once they are sanctified through the rites of academic publication, become commodified scientific observations that can be reproduced, and against which Aboriginal claims regarding the past may be held to measure. Most disturbingly, these documented narratives can attain greater evidentiary weight than oral Aboriginal histories of the same events and processes. The recent turn to rereading ethnography is a laudable endeavour to glean deeper understandings of the past from a small body of evidence. Yet unless this rereading is made with a very critical eye we may be left with a distorted view of the past provided by prejudiced authors who had very powerful reasons to dissemble.

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Kourion Urban Space Project: Preliminary Report of the 2016 Season

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Abstract: The 2016 season of the Kourion Urban Space Project (KUSP) focused on the excavation of a structure situated to the southwest of the Earthquake House on the city's acropolis. Excavation revealed a large building, designated Building 4, that had been damaged suddenly and further decayed over time. Though it is uncertain if Building 4 was a domestic structure, administrative facility or both, it is evident from the construction materials, particularly marble and painted plaster wall facing, and imported finds that it served elite members of the Kourion community. This paper provides a preliminary description and analysis of the archaeology of Building 4. It was lodged with the Department of Antiquities, Cyprus, in January 2018 and is published here with the authors' affiliations as they were then.

The Kourion Urban Space Project (KUSP) has been excavating a section of the city of Kourion on the southern coast of Cyprus since 2012. Our project space is situated on an acropolis overlooking Episkopi Bay, along the east side of a depression that separates the Kourion Basilica and Forum from the southern portion of the site, just to the southwest of the Earthquake House (Figure 1). Beginning in 2013, our work centred on and about a mound in this area that initially produced, through excavation, the remains of a mosaic floor framed by large cut stones with riveted marble veneer. These initial investigations have been documented and are available in the *Report of the Department of Antiquities, Cyprus* (Grimsley et al. 2016).

The primary aim of the 2016 excavation season was to continue uncovering the remains of Building 4, the large and rich structure that created the mound in this area as it disintegrated (Figure 2). Using the data obtained from this building, along with that from our excavations in this sector of the city, and the Kourion Mapping Project, our overall goal is to examine the diachronic, economic and social changes that took place within this city during the 4th and 5th centuries AD.

Because the excavation of Building 4 is still in a nascent phase, we have only a limited understanding of this structure and its connection to, and function within the city of Kourion. It is evident, however, that Building 4 was

Figure 1: *An Aerial photograph of the southrn part of the Kourion acropolis. The location of the KUSP excavation is indicated with a yellow box. Image: Courtesy of the Cyprus University of Technology.*

Figure 2: *A plan of Building 4. Excavation Unit names begin with the letter A, and Space names are indicated with triangles. Wall names are enclosed in rectangles and begin with the letter W.*

large and at least two stories tall. The walls of Building 4 were constructed with cut limestone blocks resting on floors and topped by a superstructure of stacked and mortared angular cobbles faced with imported marble, painted plaster or both. The structure was roofed with tiles, common for this period, and probably included decorative and structural elements such as archways and columns. These construction materials along with the presence of mosaic floors strongly suggest that, at least at its inception, this structure was either an elite residence or an administrative facility.

Methodologically, the excavation of Building 4 presents multiple challenges. The unknown extent of the building and the height of the decomposed remains, over 2 meters, and moreover the growing instability of the mound as it was bisected, dictated our excavation method. Three units along the northeastern edge of the mound were initially excavated to floor level in 2013 and 2014. These probes gave us an understanding of some of the activities that took place in this area and hinted that Building 4 may have changed function over time and was structurally damaged, perhaps in an earthquake. With this information, the decision was made that during the 2016 season it was most pertinent to focus on (1) finding the extent of the building, (2) understanding the internal layout of rooms and number of stories, and (3) tracing the evolution of the building's decomposition including understanding the processes of collapse and decay over time and the formation of the mound.

The 2016 excavations of Building 4 continued the work begun in previous years in Units 3 and 9 along the southern edge of the mound, and A7 in the northwest. New excavation units were established along the southwest edge of the mound, Units 13 and 14, and the south, Unit 12. This configuration of excavation units helped trace Building 4's interior architecture and overall extent. The growing instability of the mound, due to the large amount of tumbled wall stones necessitated two excavations units in the centre, Units 10 and 11, and the removal of some baulks. The excavation of Unit 11, only removed the topsoil and a few centimeters of the stratum below in order to stabilize the mound, ensure minimal collapse in winter rain, and provide a safer environment for members of our team while they worked.

The stratigraphy of the Building 4 mound is important for understanding the use-life and decay of this structure and section of the city. Our excavations have revealed three main strata in this area. Stratum 1 is composed of topsoil, dark in color as a result of the decomposition of organic matter on the mound's surface. This stratum is narrow,

between 2 and 5 cm thick in most areas. Immediately below, Stratum 2 is much thicker, up to 2 meters in some areas and composed of yellowish sand. Within this stratum is a ribbon of finds mixed with a substantial deposit of tumbled wall stones, comprising roughly 95% of the layer, and in some units, large deposits of roof tiles. It is likely that the collapse and decay of Building 4's walls and the subsequent infilling of Aeolian sand from the beach below produced this stratum. Most of the artifacts found in Building 4 originate in this stratum; they include a dense concentration of pottery sherds many with adhered wall mortar, and fragments of painted plaster. In rooms where floors have been reached (Spaces 28, 35, 36 and 41) the artifact scatter is minimal compared to what is found above in Stratum 2. The deposit of pottery sherds among construction materials within this stratum, strongly suggests that they were used as wall chinking between irregularly shaped stones. The presence of adhered mortar on many of these sherds along with the observation of this construction method in some of the still-standing walls of Building 4 and elsewhere at Kourion furthers this argument. Because it appears that Building 4 is multi-storied, it is also very probable that some of this pottery as well as Stratum 2's other finds were originally situated in upper floors and fell as the building collapsed and decayed. Stratum 3 lies below the tumbled wall stones of Stratum 2. It is also composed of yellow sand and includes standing or collapsed architectural elements such as the large-cut limestone foundation blocks of walls and carved architectural pieces, and mosaic, ash or compacted earth floors. This stratum contains very few artifacts, particularly in comparison with the stratum above.

Collapse, decay or both

In 2014, the remains of four individuals were found in one of the initial excavation units along the northeast edge of the mound, later to be referred to as Space 35. Their presence, below tumbled wall stones, suggests that Building 4 was damaged and at least partially collapsed suddenly. Though we have not conclusively dated the construction or use of Building 4, it can be argued that this structure was affected by an earthquake. A series of earthquake or "earthquake storms" in the mid-4th Century have been documented for the Eastern Mediterranean, the largest of which halted activity within this city and left many buildings and public works irreparably damaged (Costello 2014). Further evidence of Building 4's earthquake damage can be seen in the coursed tumble found in Units A13-14, an offset wall (WE), which appears to have slumped over after it was pushed and finally a large cut stone block dislodged and tumbled from its original placement atop other large wall stones in Space 28. Wall WT also appears to have been pushed and slumped (Figure 3). However, some of Building 4's walls appear to have undergone a slower decay process, dropping stones over time. Stone structures can completely collapse in earthquakes or can partially collapse, be abandoned, and decay further over time. Though we cannot yet say conclusively which process ended the use-life of Building 4, we are currently working with the hypothesis that all three occurred; parts of the structure were destroyed or damaged during an earthquake and the entire building further decayed over time.

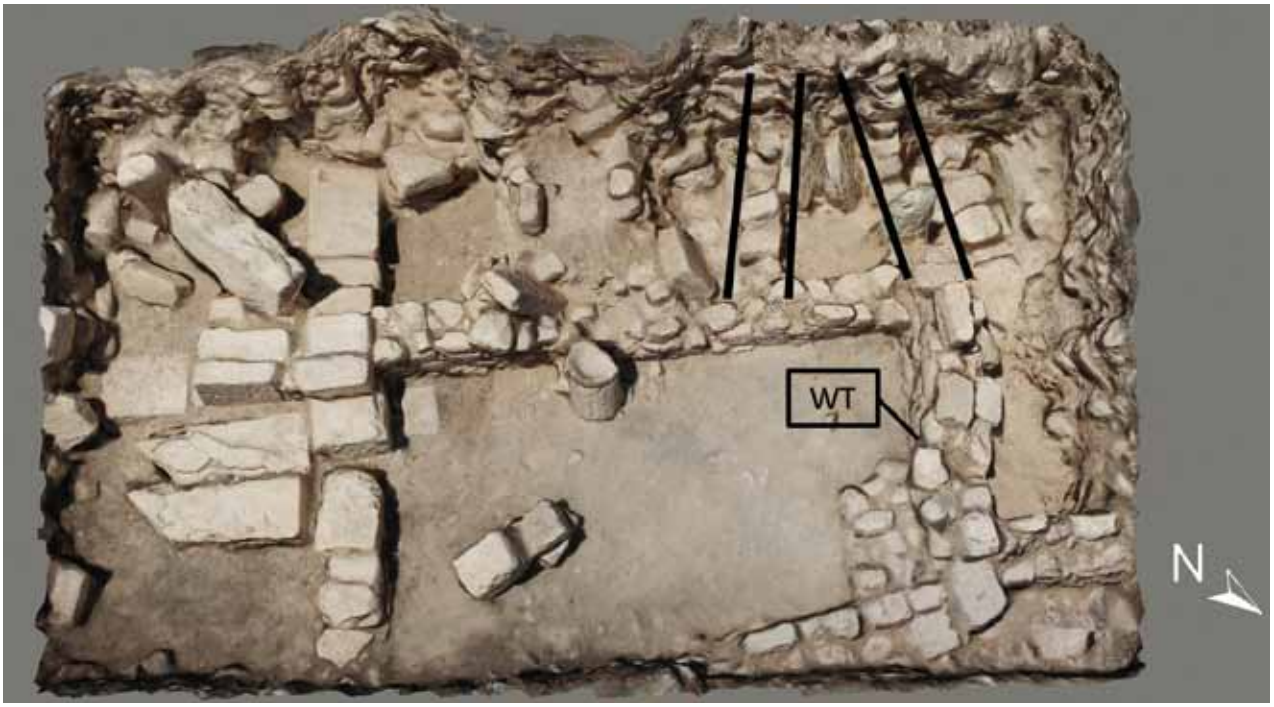

Figure 3: *A geo-referenced overhead photograph of the Space 41 corridor. The black lines indicate the walls that border the corridor and may have once contained a stairway.*

The Excavation of Building 4

The 2016 excavation of Building 4 continued and expanded on work from previous seasons in this area of Kourion. Four units were reopened or extended (A3, A7, A9, A10) and three new units were established (A12, A13, A14). Each unit measures 5 m x 5 m with 1 m baulks between. Excavation of units was conducted in quadrants to better trace the sequencing of both the building's collapse and the distribution of material fragments found throughout the stratums.

Unit A7

Unit A7 was partially excavated in the 2014 season, concluding when the tops of the preserved walls were reached. In 2016, excavation continued in this unit to further expose the architecture and reach the floor. This unit represents the northernmost section of Building 4 that we have exposed; it is unlikely, however, that this is the extent of the structure in this area.

Excavation of A7 revealed a complex series of walls (WE, WS, WT, WU, and WV) that enclose at least two known rooms (Space 36, 41). Wall WE, exposed previously in the adjacent excavation unit (A4), extends into A7, nearly bisecting the entire unit. A large wall, WE measures 4.6 m x 0.54 m x 0.49 m and was constructed of irregular cobbles chinked with pottery sherds. At its north-western extent, it abuts the north-southwest running wall WT that extends 4 m and is preserved up to a height of 0.91 m. Wall WT leans to the northwest and is partially collapsed to the southeast. Two additional walls, WV and WU, extend from the northern terminus of WT to the northwest and southeast respectively.

Bordered by WE, WT and WU, Space 36 extends across units A4 and A7 measuring 4 m x 2 m. Within this Space, resting against the northeast face of WE, a limestone column measuring less than 1 m in height topped with a carved limestone basin, 47 cm in diameter, was found. The function of this pedestalled basin is uncertain; it is probable that the column was an architectural piece that was repurposed.

Space 41, formed by the abutment of WE and WT and further defined to the south by WS, is a narrow corridor. Within Space 41, three upright piers were found, suggesting that this area may have once contained a stairway (Figure 3). The height of the mound formed by Building 4, the large amount of tumbled wall stones and the presence of material culture not used as wall chinking within Stratum 1 strongly support the notion that this structure had multiple stories that required a stairway to access.

Many of the finds from A7 came from on or near the floor in Space 36. Directly resting on a beaten earth floor were

Figure 4: *An Egyptian amphora, Egloff form 172 in the Kellia typology, in situ in Space 36, Building 4.*

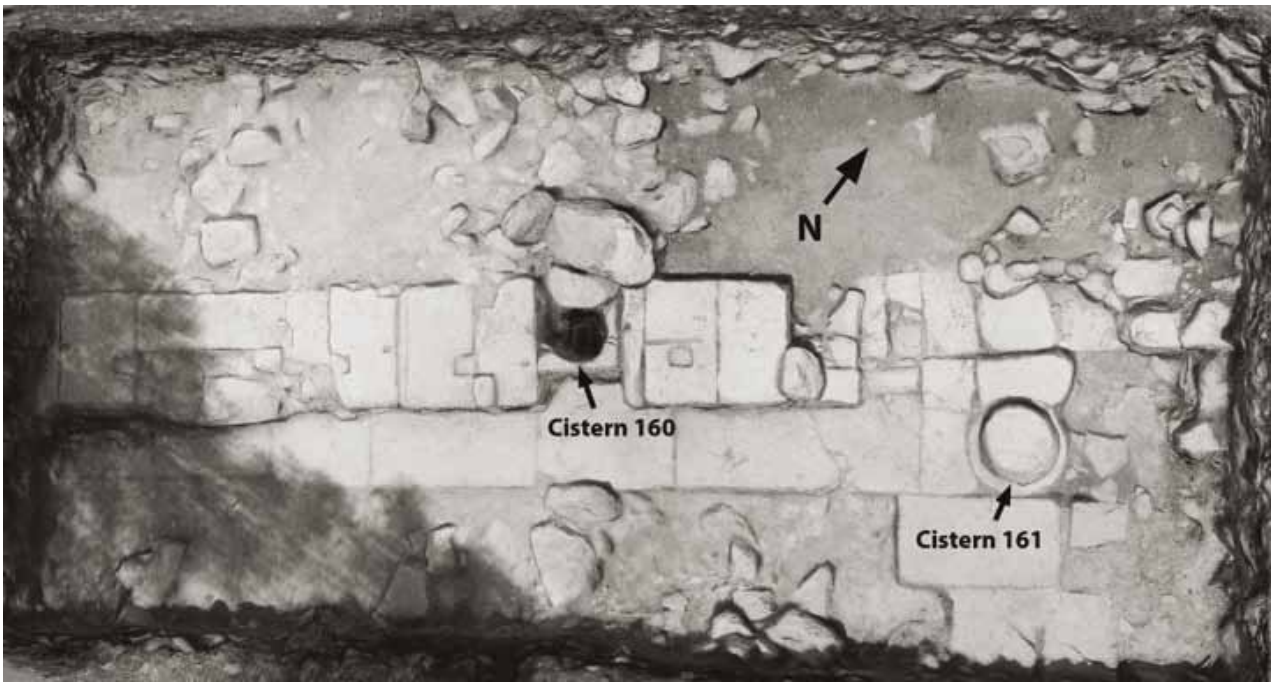

Figure 5: *An overhead photograph of Units A9 and A12. There are two co-joined cisterns 160 and 161; Cistern 161 opens into a pavement and is still capped by a cylindrical stone; Cistern 160 is situated within Wall WK and had a rectangular cover that had been dislodged leaving the cistern open allowing it to be partially filled.*

faunal remains, a near complete Egyptian amphora Egloff form 172 in the Kellia typology (Egloff 1977; Figure 4), fragmentary cooking vessels, lamp fragments, a nearly complete glass unguentarium, and three small copper-alloy coins: two illegible, and one with the Emperor Valens depicted on the obverse.

Unit A10

In 2014, the top of Wall WO was reached in Unit A10, but time did not permit further excavation. This unit was reopened in 2016 with a focus on exposing more of WO, tracing its extent and further defining the internal arrangement of walls in Building 4.

The exposed section of WO is composed of a large, shaped, rectangular stone topped by smaller mortared cobbles. On the east face of WO within Unit A10, a marble slab almost 2 m long was found upright but at a slight angle from the face of the wall. The gap between the wall and the slab contained pot sherds along with mortar indicating that broken pottery was used to help adhere the slab to the face of the wall, filling in any irregularities or gaps. It is also apparent from divots cut along the top of the marble slab at regular intervals, some with copper staining, and the remains of brackets imbedded in the north section of the wall's rectangular block that the marble was once held by copper alloy wall brackets.

Also on the east side of WO in this Unit, numerous painted plaster fragments were found among smaller tumbled wall stones. These fragments are decorated with parallel and bisecting lines of red, black, and greenish-blue. The plaster fragments indicate that the upper portion of WO was decorated above the marble wall facing. Additionally, small rectangular fragments of marble with one squared long edge and one rounded long edge were recovered in the stone tumble along with the painted plaster fragments. These pieces may have lined the top of the marble facing separating it from the painted plaster, producing a wainscoting effect as seen in wall decoration today. WO is the best-preserved wall in Building 4 and is a good indication of how many of the walls were constructed in this building, as similar construction materials (large-shaped stones, cobble tumble, ceramic fragments with mortar, and plaster fragments) have been found in other units.

Most of the ceramics from this unit were mixed with wall tumble in Stratum 2 and still held wall mortar, having been used as wall chinking. Additional fragments of an imported Egyptian glass plate with millefiori design, initially found in 2014 (Grimsley et al. 2016; Swantek and Grimsley 2016), were located on the west side of WO, at least 3 m from the other pieces found in A7 previously. All other cultural material was primarily found in a thin ribbon from the bottom of Stratum 1 down through the tumble of Stratum 2. The density of cultural material significantly decreased below this tumble.

Units A3, A9, A12

Previous excavation revealed the continuation of Wall WE in Unit A3, and its connection to a long northeast-southwest wall, WK, running through the adjacent excavation Unit A9, along the southeast base of the Building 4 mound. This substantial wall, which may be a perimeter wall for Building 4, was further excavated

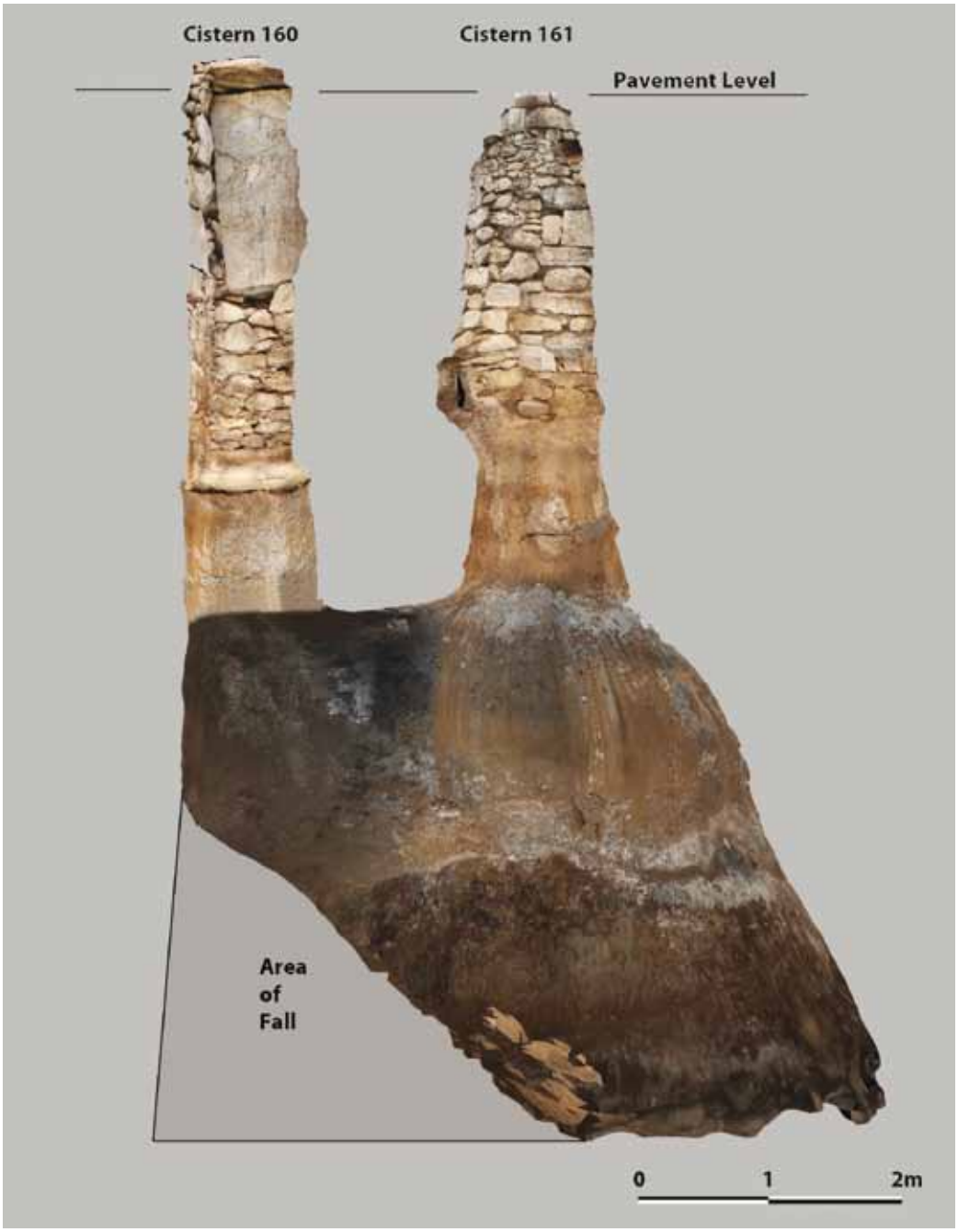

Figure 6: *An orthophotograph of a section looking north of the conjoined Cisterns 160 and 161 in Units A9 And A12. These cisterns have separate openings and shafts which lead to a single reservoir. Image generated by Agisoft Photoscan.*

in 2016 with the continued excavation of Unit A3, a 1m southern extension of A9, and the opening of A12 (Figure 5).

The full length of WK is still unknown, but the northern terminus was found in the southwestern section of A3. The exposed section of WK measures 7 m x 0.5 m and is constructed using square cut limestone blocks that overlay flat cut-rectangular slabs that may have functioned as paving stones. Set within WK is the collar of Cistern 160 in A12 and in the adjacent limestone pavement is the collar of Cistern 161 in A9. These underground openings have been imaged using a remotely operated camera to produce a 3D image, one projection of which is in Figure 6.

The shaft-collar of Cistern 160 is 0.48 m square and Cistern 161 is 0.42 m diameter. Cistern 161 has a circular cover stone in situ while Cistern 160's rectangular cover was dislodged leaving the cistern open. Cistern 160 has niches in the stones adjacent to the collar that may have secured the axel of a windlass. The two shafts lead to the same underground reservoir in which there is a heap of rubble and soil the top of which is under the shaft of

Figure 7: *Tumbled wall stones found across Units A13 and A14. The majority of the wall stones found in these units were large, shaped limestone blocks. A high concentration of broken roof tiles was also found in these units.*

Cistern 160. It is probable that the cistern was operating when Building 4 was abandoned and that the debris entered the cistern subsequently through Cistern 160, which had been left open. The shaft of each cistern has a similar design and construction. Below the collars are dry-stone lined 0.8 m square shafts down to the bedrock, which is 2-3 m below the surface. The shafts continue through the bedrock at about 1 m diameter into to the reservoir, the top of which is about 4 m below the surface. The reservoir of Cistern 161 is a rock-cut bell-shaped void with a base measuring 5-6 m diameter about the shaft centreline 8 m below the collar. This reservoir was extended westward to meet Cistern 160. Cistern 161 has a 0.2 m diameter pipe entering it about 1 m from the surface. The fall of the pipe is yet to be measured but it is most likely toward the cistern as there is no other means to fill it. Cisterns 160 and 161 were structurally integral to Building 4 and, together with the network of pipes and drains that filled these cisterns, were constructed contemporaneously with the surface structures.

Units A13 and A14

Two new excavation units were established in 2016 along the southwest side of the mound, Units A13 and 14. Both units contained a large concentration of tumbled wall stones and broken roof tiles, and appear to represent the same space within Building 4, as no bisecting wall was found (Figure 7).

Excavation revealed the presence of two partially preserved walls in A13. Wall WW runs roughly southeast-northwest along the northern edge of A13, and partially extends into the southeast corner of A14 before disappearing into the baulk. On the southwest face of WW a small piece of wall plaster with red paint was found still adhered to the wall with thick mortar. Wall WO extends into A13 from A4 and A10, intersecting WW.

In both A13 and 14, Stratum 3 contained large cut blocks that were displaced or possibly tumbled from walls, along with other architectural stones such as column fragments. It is not clear from the pattern of tumbled stones from

which direction they fell, however, a partial collapse in a southward direction seems evident in A14.

Similar to all other excavation units, the majority of ceramics found in A13 and 14 came from the upper portion of Stratum 2, mixed with tumbled wall cobbles. The cobble tumble was especially deep in A14, approximately 2 m, as it was close to the highest point of the mound. Many of the ceramic sherds were smeared with mortar, and this combined with their presence in the tumble suggests they were used in wall construction. Also present within the cobble tumble were large fragments of mortar, possibly used to as flooring for an upper story, ranging in size between 5 cm to 20 cm and a thickness of 2 cm to 7 cm. The highest concentration of this floor mortar was found A14. Near the bottom of Stratum 2 there was an increase in marble fragments and painted plaster and a decrease in ceramic sherds. Nearly 1,250 marble fragments were uncovered along with over 850 painted plaster fragments, similar in design to those found in A10. In A14, a plaster fragment molded to fit around a wall corner was found with a similar design on both sides executed in different colors. The design is reminiscent of the natural veining in marble. The largest concentrations of roof tiles were found in A13 in the south and west portions of the unit and in A14. In all, hundreds of roof tile fragments were found in these units, while only small fragments of roof tiles were found randomly dispersed throughout the other units in Building 4.

Future Work

The continued excavation of Building 4 will reveal the overall dimensions, layout and function of the structure but will also provide insight into the socio-economic processes that shaped and reshaped the city of Kourion over time. This work will also give us a better understanding of how sudden natural disasters and decay over time change the use of buildings and create archaeological sites in urban environments.

Work on Building 4 will continue to trace architecture, particularly to find the elusive perimeter walls, but will also include excavation of floor deposits, with special care given to mosaics working in collaboration with the Department of Antiquities. Future work will also continue to involve the use of calibrated photogrammetry inside and around all cisterns and the use of GPR to trace pipes and drains to better understand the unique water system that existed on the Kourion acropolis.

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Kourion Urban Space Project: Preliminary Report of the 2018 Season

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Abstract: The 2018 season of the Kourion Urban Space Project (KUSP) continued the excavation of Building 4, a large structure situated just to the southwest of the Earthquake House on the Kourion promontory. Focusing on tracing the architecture of this building, this season revealed the northwest perimeter wall and identified two internal rooms: the courtyard and Room 35. Building 4 continues to yield high quality and imported artifacts including glass vessels and fragments of marble statuary along with imported marble and stone construction materials. These artifacts combined with the overall large size of this structure and construction materials suggest that this building served the elite class at Kourion, though it is still uncertain if it served a public or private purpose. This report was lodged with the Department of Antiquities, Cyprus, in January 2022 and is published here with the authors' affiliations as they were at the time of the excavation.

The Life of the Ancient City of Kourion

The ancient city of Kourion is located near the modern village of Episkopi on a promontory overlooking the Mediterranean Sea. Situated between Paphos and Amathous, Kourion was a thriving city from the first century BCE until the late 4th Century CE. Kourion was a dynamic city. Moderately sized in the Hellenistic period, Kourion was complete with a theatre in the city centre by the 2nd century CE and a stadium and sanctuary dedicated to Apollo to the west.

Kourion underwent many changes throughout its life, growing and shrinking with political and environmental changes. Perhaps the strongest influence on Kourion, shaping and reshaping its urban layout, were the earthquakes that occurred throughout its life. Cyprus lies just north of the Cyprus Arc, the tectonic boundary of the African and Eurasian lithospheric plates. It is the collision of these plates just southwest of the island that causes the majority of the earthquakes felt on Cyprus (Papadimitriou and Karakostas 2006). Historical analysis of earthquake activity along the Cyprus Arc indicates that this boundary will cycle through long periods of inactivity and shorter active periods (Kythreoti et.al. 2005). One such active period occurred between 1995 and 1999 with a series of very strong earthquakes averaging 5.6-6.5 on the Richter scale (NGDC 2018). It is apparent from archaeological evidence as well as written sources that during the Hellenistic and Roman periods, seismic activity affected life in the coastal cities of Cyprus.

The earthquake of 15 CE described by Dio Cassius (54.23.7; Penelope 2011) affected both Paphos and Kourion with the former city so devastated that imperial intervention was required. The Kourion theater was damaged during this event and required restoration work. A later earthquake in 76 CE may have also affected this structure and prompted renovations. This earthquake was felt along the entire southern coast and eastern coast of the island at Paphos, Salamis and Kition (Costello 2011). There are no indications of seismic activity during the 2nd century, or at least no devastating earthquakes were described. In the 2nd Century, the city center of Kourion expanded and was renewed; colonnaded stoas were added to the Forum, a second water conduit was added to bring water to this naturally dry city, the House of Achilles which likely fulfilled a public function was built, and the theater was again renovated. Outside of the main city, the Sanctuary to Apollo was also renovated and a stadium was built. Sometime in the 3rd century the House of the Gladiators and the House of the Apsed Triclinium were also built.

The 4th century brought major changes to Kourion and disruptions to life within the city. A series of earthquake storms or a period of intense seismic activity occurred during this century. Two earthquakes were recorded at Salamis in 332/3 and 342. The second of these events was so destructive that Salamis was re-founded as Constantia, a smaller city and the new capital of Cyprus. It is possible that other parts of Cyprus felt these earthquakes, but no major damage is described. The date of the earthquake that destroyed Kourion is uncertain; Soren and colleagues (1985) have argued that it is the same earthquake described by Ammianus Marcellinus as occurring on 21 July 365. This earthquake caused damage across the Mediterranean, producing a tsunami that destroyed several cities on Crete. Mediterranean

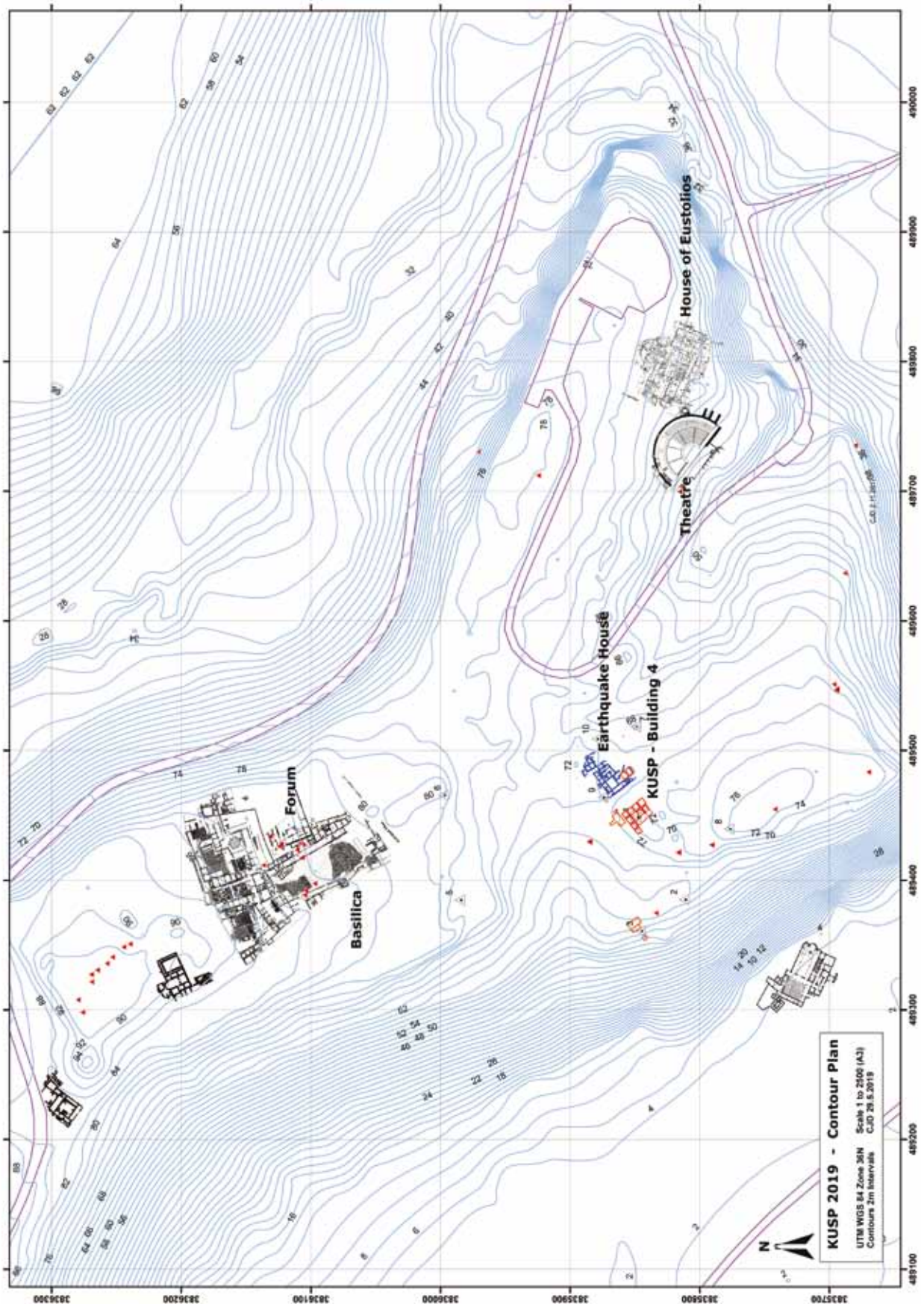

Figure 1: *An annotated contour plan of Kourion prepared by KUSP.*

tsunamis are rare but have been documented as associated with earthquakes in 1222 and 1953 (NGDC). Costello (2011) has argued that it is unlikely that the earthquake that devastated Kourion was the same that hit Crete, but archaeological evidence suggests that the destruction of Kourion occurred sometime in the late 4th century.

The events that followed the Kourion earthquake are uncertain, however the city appears to have been abandoned for at least forty years (Costello 2011: 39). Clean-up and reoccupation were slow, with minimal squatter occupations appearing first (Soren 1987). The character of Kourion was completely changed when rebuilding began. Most of the major buildings that had been destroyed were never rebuilt and those that were preserved, did not continue to serve the same function (Costello 2011). Prominent at the archaeological site today, and a major change to the Kourion promontory, was a new Christian basilica that was built on the foundations of the Roman civic basilica. It functioned as the seat of the Bishop of Kourion. Also added to the city at this time was the House of Eustolios. The function of this building is unknown; however, a mosaic inscription indicates that whomever Eustolios was, he was involved in rebuilding the city or 'restoring calm in the earthquake-struck land' (Mitford 1971).

The Kourion Urban Space Project excavation of Building 4

The Kourion Urban Space Project (KUSP), begun in 2012, has focused its archaeological work on the years leading up to the destructive earthquake and the effect of this event on the urban topography, and socio-economic and religious systems of this period. In particular, KUSP examines and compares the lifeways of elite and non-elite inhabitants of Kourion before and after the earthquake to understand the link between changes in the socio-economic system and the structure of the urban environment.

The KUSP archeological area is situated just southwest of the Earthquake House in a depression that separates the basilica and forum or centre of the city from the theatre and the House of Eustolios (Figure 1). This area has produced at least three new buildings (Buildings 2-4), the manipulation of the landscape for walking surfaces and a water holding tank. The focus of KUSP's work in the 2018 season was the excavation of Building 4, just south of the Earthquake House. Originally thought to be a natural hill, the excavations beginning in 2013 revealed that this land form was not the product of the natural deposition of Aeolian sand and subsequent soil formation but was instead created by the remains of a large structure. We estimate that this building is around 20 m wide, based on its proximity to other structures, and 70 m long, ending just where the ground level abruptly slopes downward towards the Sea (Figure 1).

Building 4 Stratigraphy

It is apparent from the thick deposit and depositional pattern of tumbled wall stones that Building 4 was suddenly and catastrophically damaged. Preliminary analysis of pottery below the tumble indicates that it was destroyed during the earthquakes of the late 4th century. It is uncertain, however, if this building was in use up to its destruction or was abandoned and subsequently destroyed. During the 2014 excavation season, KUSP unearthed the commingled and disarticulated skeletal remains of four individuals: an adolescent, a child, a young child and an infant (Osterholtz 2014). These individuals may have been inhabitants of the building during its use-life or may represent squatters using an abandoned building. They may also have run into the buildings seeking refuge only to be killed by falling walls and roofs. As more of this building is unearthed and artifacts on floors are examined, we will have a clearer understanding of its use life and destruction.

While it is uncertain if Building 4 was in use up to the earthquake, it is evident that it was never rebuilt after its destruction. Atop the tumbled wall stones is a ribbon of broken artifacts, primarily pottery and glass. It appears that this depositional pattern is the product of a particular action common throughout the city of Kourion; following the earthquake, refuse in the form of broken objects and sometimes wall stones that were not reused elsewhere were dumped in abandoned areas to aid the clean-up of the city. This ribbon of finds on top of the destroyed remains of the building seem to indicate this action, and the final abandonment of the building.

The 2018 Excavation Season

The KUSP excavation plan since 2016 has been to trace the architectural extent of the building and interior divisions of the space (Figures 2 & 3). As such, we have not reached the floor in most of the rooms we have isolated. Previous reports of our work have described the floor assemblages that have been excavated in Units A3, 7 and 9, corresponding to Rooms 28, 36 and 41 (Grimsley et al. 2023). We have revealed the architecture for roughly 400 square metres of this building or a 20 m x 20 m area and have isolated at least five interior rooms, and a possible staircase and courtyard.

Excavations during the 2018 season focused on identifying the northwest perimeter wall of Building 4, and the architecture in what might be the centre of the building. Excavation units A15-18 were placed along the northwest extent of our excavation, and A10, 11 and part of 7 were revisited.

Excavation units A15-18 and building 4's exterior wall

Units A15-18 revealed a long, straight wall running north-southwest, designated WX. Because of the proximity of this wall to Building 3, previously excavated by KUSP, it

Figure 2: *A plan of Building 4. Rooms 35 and 41 are indicated. Wall names begin with the letter W. Rooms are indicated by numbers in squares. Excavation Units begin with the letter A.*

is very likely that WX is the perimeter wall of Building 4 on this side. WX was constructed from both cut blocks at the base, primarily found in Units A17-18, and unworked cobbles of local limestone. The stones were mortared together, and gaps were filled with both small stones and pottery used as chinking. WX is, on average, 0.65 m wide and is at least 11 m long based on the portion that has been revealed. At its northern extent, WX joins to WV. This forms the north corner of Building 4.

Courtyard

The excavation of Units A17 and 18 in 2018 and the discovery of the perimeter wall WX has delineated the northwest boundary of what we think is a central courtyard within Building 4. Partially excavated in 2016, this section of the building appears to be an open space, as no cross-cutting walls have been found running through it. The earthquake debris in this area varied from large cut-architectural stones in the northwest to small stones and then back to larger blocks to the southeast. The southern section of the courtyard yielded the largest deposit of roof tiles we have associated with Building 4. It is possible that this courtyard was partially roofed; the roof collapsed either during the earthquake and, perhaps, decayed further over time as roofbeams rotted. In the northern section of the courtyard, fragments of painted plaster and marble revetment or floor covering were found among the plain and decoratively carved architectural stones.

The 2018 excavations in the northern extension of the courtyard revealed more decorative marble that once adorned walls and floors, but also several fragments of sculptures. A fragment of a leg, and knee or elbow were found as well as the leg of an animal. Porphyry, a purple-red stone imported from Egypt, was also found in this area. Flat on both sides, it was most likely used as decorative wall or floor covering.

Rooms 35 and 41

Room 35 lies just below what was once the apex of the Building 4 hill. The highest preserved walls of this building border Room 35; it is impossible to say at present if this is because Room 35 is actually a second story room, or if the preservation of the walls is a result of how the building responded to the earthquake waves. The former hypothesis is supported by Room 41 tucked into the northern corner of Room 35. Room 41 has been published elsewhere as a possible stairway (Grimsley et al. 2018). The 2018 excavations did not refute this possible function, though additional excavation in this area did not elucidate the function of this small space.

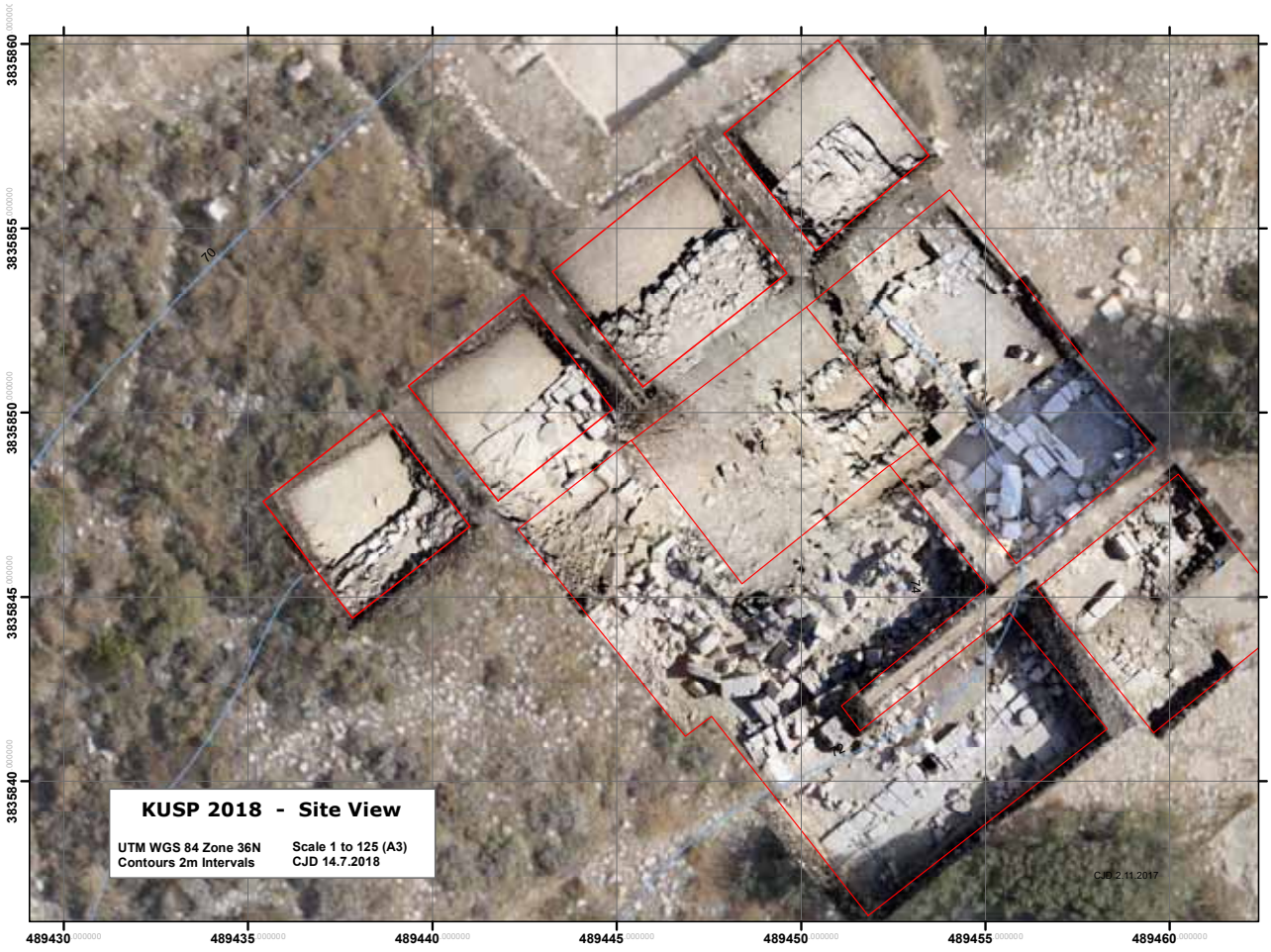

Figure 3: *A composite geo-referenced overhead orthophotograph of Building 4.*

Room 35 is difficult to interpret at present. Three walls seem to run parallel through this space but do not completely divide the room (Figure 4). Two of the walls are significantly slumped to the south, leaning on top of Aeolian sand fill. WT, the northern most wall, is the most vertical and extends the farthest forming one side of Rooms 41 and 36. Room 36 has been reported on before, but it is interesting to note here that this section of WT is lifted and twisted as a result of the earthquake waves uplifting and moving the ground beneath the building. These three walls as they exist in Room 35 appear to have been partially destroyed in the earthquake and decayed over time, slumping under their own weight onto sand that infilled after the building was abandoned. This process formed what appears to be a bipartite or tripartite division. Future work in this area will test the hypotheses that this series of walls formed a divided storage area, a tripartite entry way, part of the stairway that led to the second floor or second floor walls.

The majority of the artifacts recovered from this room were found between the series of three walls. The high quality of the artifacts mixed with many floor mortar fragments seems to support the hypothesis that the remains in this room are the collapsed upper story of the building and do not reflect a clean-up and dumping episode following the earthquake. Among the notable artifacts recovered from between these walls and mixed with wall tumble was a restorable cooking pot and cooking pot lid and six coins that date from the 1st-4th centuries. Of the six copper-alloy coins found, four were issued during Valens reign (364-378), and one under Julian II (361–363). The sixth coin was minted in Philippi, Macedonia. Victory facing left is depicted on the obverse; three Roman legionary standards are on the reverse.

This room has yielded fragments of several high-quality glass vessels. Previous excavations in this room revealed fragments of a mosaic glass plate imported from Egypt. Reported on before (Grimsley et. al. 2023; Swantek and Grimsley 2016), this plate measures roughly 60 cm in diameter and is made from a green matrix with inset yellow rods. Along with this plate, a fragment of a glass vessel decorated with a raised hexagonal pattern and another with blue marquis shaped insets were found. Three joining pieces of a glass cage cup were also unearthed in this room (Figure 5). This vessel is dated to the 4th century and was probably produced during the Constantinian dynasty and imported to Cyprus (O'Hea *Pers. Comm*. 2018). Cage cups are extremely rare, around

Figure 4: *A 3D view Units 10, 11 and 16. The three slumped walls in Room 35 are in the foreground. The view is looking southwest. Image: generated by Agisoft Photoscan.*

fifty fragments have survived (Harden 1987). Perhaps the most famous cage cup is the Lycurgus Cup made from dichroic glass, now in the British Museum (Online 2019). The fragmentary cage cup found in Building 4 was probably once clear glass with a clear or milky white and green cage. This colour combination is similar to the Cologne cage cup in the Romano-Germanic Museum in Cologne; this cup is decorated with a raised glass inscription in Greek reading 'Drink, live well forever' (Trier and Naumann-Steckner, eds. 2016).

KUSP's Future Work

The architectural remains and artifacts recovered from Building 4 from 2013 to 2018 suggest that this structure served the elite of Kourion as either a public building or private residence. Imported marble and porphyry was used as wall and floor covering, painted plaster adorned walls in most rooms and carved gypsum and limestone trim likely used along walls and ceilings indicate that the visual presentation of this building was both important and reflected the status of its users. Artifacts found below tumble and on floors supports the hypothesis that this building reflects elite lifeways at Kourion prior to the destructive earthquake.

Archaeological investigations of Building 4 will continue in the coming years. KUSP intends to continue to trace the perimeter and internal walls of this structure and determine its overall vertical and horizontal size. Excavating to the floors of Building 4's rooms has presented a conservation challenge as many of the floors are mosaiced. Working along with the conservators from the Department of Antiquities, we will follow their protocols for exposing and preserving these mosaics. Further challenges are faced with Building 4's slumped walls and the removal of the unstable sandy fill on which they rest. We are devising a method to digitally map these walls and the order and placement of stones within them, so they can be rebuilt and stabilized.

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Figure 5: *The restored fragments of the cage cup found in Room 35.*

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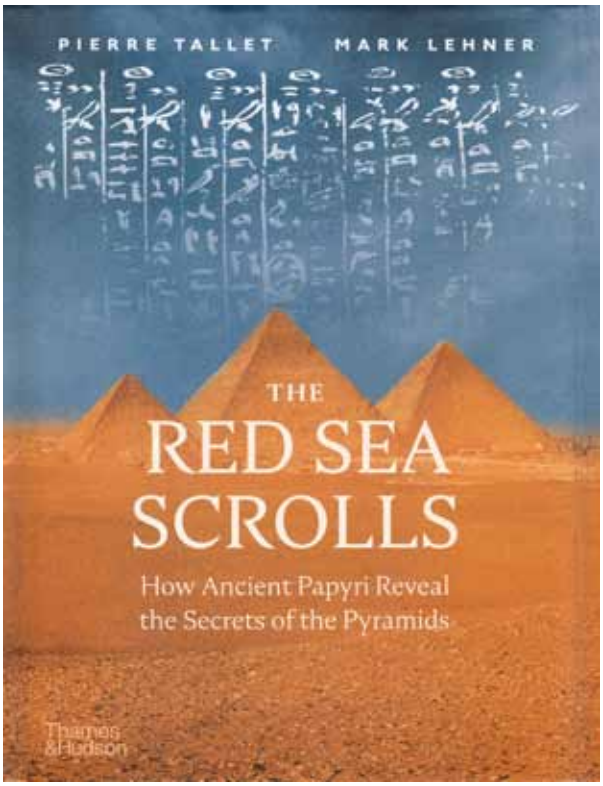
Reviews



Pierre Tallet and Mark Lehner, 2021 *The Red Sea Scrolls: How Ancient Papyri Reveal the Secrets of the Pyramids,* London: Thames and Hudson, ISBN 978-0500052112, pp. 320, illus. A$60.

Reviewed by Christopher J. Davey

When Professor Tallet visited Melbourne some years ago he spoke about his discovery on the Egyptian Red Sea coast of papyri that were written by workers associated with the construction of the Great Pyramid of Giza. His lecture was given on a sleepy winter Sunday afternoon to a sparse audience in a theatre at the deserted Monash University campus. Surely, I thought, this significant discovery deserved to be more widely publicised, although the absence of aliens may have dampened the enthusiasm of some local media. This profusely illustrated and comprehensive book, which was obtained from a Melbourne bookshop, goes a long way to rectifying the situation.

Pierre Tallet is Professor of Egyptology at the University of Paris-Sorbonne, President of the French Society of Egyptology, and director of the CNRS research unit *Orient et Méditerranée*. He directed the archaeological team that discovered the papyri at Wadi el-Jarf. Mark Lehner is President of the Ancient Egypt Research Associates and an Associate at the Oriental Institute, University of Chicago. He is arguably the current leading world authority on Egyptian pyramids. They give the book a distinctive imprimatur of authority.

The circumstances of the discovery of the papyri are described in the Prologue and Introduction. Between 2011 and 2013, Tallet's team found underground galleries at the ancient Red Sea port of Wadi el-Jarf, which contained disassembled boats and an archive of papyrus documents. The galleries were sealed with limestone blocks bearing the cartouche of King Khufu (2633–2605 BC). The documents were dated to the 26th year of Khufu's reign and included a daily log of the sailors who were responsible for transporting limestone blocks and supplies for the construction of the Great Pyramid. For many years Mark Lehner had been conducting excavations at Giza. The results from these two excavations are the subject of the book, which tells how this 'parallel research has totally transformed what we know about one of the greatest construction projects ever undertaken, and how that project shaped the creation of the ancient Egyptian state' (p. 12).

The book is written in six parts: On the trail of the pyramid builders; Why the Red Sea and the pyramids are linked; The world's oldest written documents; The story of Inspector Merer; How they built the Great Pyramid; and Legacy. Most books about the pyramids describe the evolution of their design, their religious significance, and funerary functions. This book uses data from the excavations at Giza, the Red Sea coast and Sinai, and the information contained in the papyri, to describe the logistics that bought together the stone and metal resources and the processes used to build the Great Pyramid.

The first chapter, *Riches in Egypt's Remote Deserts*, describes Tallet's exploration in the Western Desert where he located Middle Kingdom inscriptions at the Bahariya Oasis, and in an Old Kingdom mining area of Sinai, where he discovered inscriptions and rock carved images, many of which were of boats. The modern urban mind may find this beginning of a book about Egyptian pyramids rather strange, but Tallet is demonstrating that complex societies, such as Old Kingdom Egypt, were dependent on the discovery, procurement and utilisation of resources in far-flung places.

The following chapter examines the logistics associated with the transportation of the resources to the Nile valley. The southern most port on the Red Sea was Mersa Gawasis. It was discovered in 1976 and was found to have operated during the Middle Kingdom. In the north, Ayn Sukhna was excavated in 2001 revealing it to have been active from the Old Kingdom until the Coptic period. Wadi el-Jarf, the Old Kingdom port and place where the papyri were discovered, is 100km south of Ayn Sukhna. All sites had underground galleries for storage, especially of disassembled boats. Their presence in the galleries indicates that the ports were used for campaigns to Sinai and Africa and were probably not operated perennially.

The link between the Red Sea ports and the pyramids arose because of the need for copper sourced from Sinai

to fashion tools used in the preparation of stone blocks. Chapter three discusses the copper production (Tallet) and its use in pyramid construction (Lehner). Tallet's team estimated there to be at least 3,000 smelting units at Seh Nasb in southern Sinai, but it was not possible to date them. The images of them are reminiscent of the smelters at New Kingdom Timna, and the suggested reconstructions are not convincing (p. 76). Rather than importing wood to fuel the smelters, as the book suggests, it was more likely that charcoal was used. There is no mention of artifacts such as those from Serabit el-Khadim published by Beit-Arieh in *Levant* 1985. Sadly, the political situation prevents any further exploration of the area. Lehner implies that many thousands of tonnes of copper were required to build the pyramids and to fabricate funerary equipment and much of it came from Sinai.

The design and construction of Khufu's Great Pyramid complex, and those before it, are discussed in a chapter that conveys Lehner's current thinking. In his view, the Meidum pyramid was Sneferu's first pyramid project, it had its construction restarted in the latter years of Sneferu's reign and was left unfinished when he died. This leads to the proposition in the following chapter that the port of Wadi el-Jarf began during Sneferu's reign because of its convenient location to Meidum. It continued to be used during the time of Khufu but was abandoned when Ayn Sukhna became the preferred port during the reign of Khafre.

The finds from the galleries of Wadi el-Jarf are described in chapter six. There were many well-preserved organic artifacts associated with seafaring including rope, wood and textiles. Pottery was produced at the site and markings on it led to the conclusion that the port was used for about 70 years. Inscriptions written on pottery sherds and limestone blocks yielded information about the identity of work-gangs active at the site, but it was the bundles of papyri discovered in Galleries 1 and 2 that provide the primary impetus for the book.

The papyri date to about 2607 BC, near the end of the reign of Khufu and comprise accounts describing the delivery of food and equipment to the work-gang (*aper*), and most importantly, the remains of many logbooks reporting on the daily activities of the gang. There are over a thousand fragments from what were originally at least thirty scrolls, they are written in the hieratic script and are the oldest explicitly dated Egyptian documents.

Chapter seven lists comparative collections of Old Kingdom papyri, describes the conservation techniques applied and the reconstitution of the fragments into a series of incomplete documents. Using tables, images and drawings of the papyri, Tallet explains the organisation of the documents and provides translations of Papyri A and B. He concedes that the repetitive nature of the documents may not be fascinating reading, but they do 'provide a clear insight into the topography of the area of the construction site at Giza during this period, especially the network of canals and artificial ponds that had been developed to facilitate the delivery of massive amounts of material brought by boat' (p. 175). The writer of the papyri, Merer, saw the Great Pyramid being built.

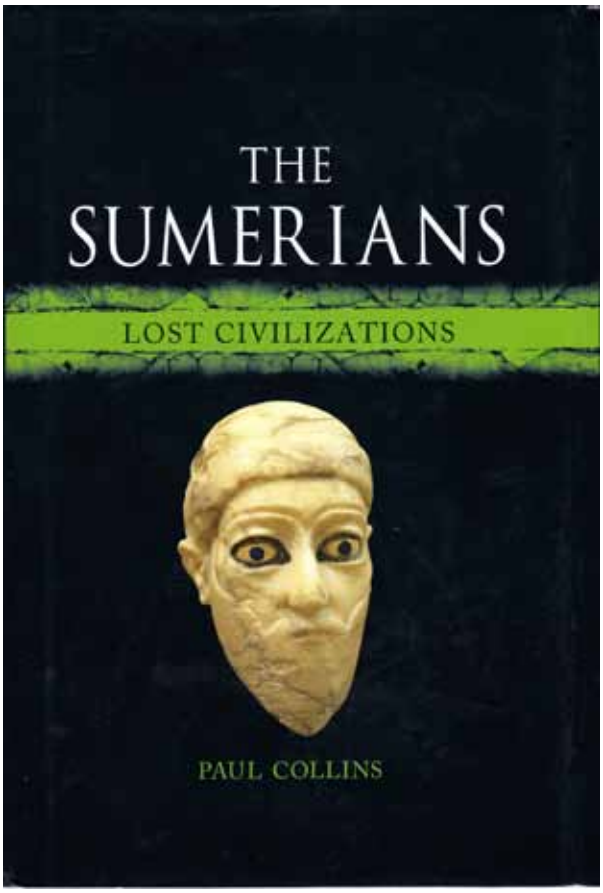
Part four of the book is devoted to the working life of Merer and his crew as it can be deduced from the papyri and other evidence. While the documents do not describe how the work was undertaken, they do provide logistical data that imply the numbers of boats and men operating. In addition to carrying limestone blocks from Tura to Giza, the team did labouring tasks, such as constructing port facilities, work programs in the Nile delta, voyages across the Red Sea and they participated in the king's cultic program. The papyri were written over a one-year period and reveal the mobility and versatility of the work-gang. It is possible that the documents were left at Wadi el-Jarf on the death of King Khufu.

The penultimate part of the book discusses the construction of the Great Pyramid. Using information from recent excavations by Lehner, it begins with a discussion about the workers' accommodation at Giza, where Merer's work-gang occasionally resided. Also discussed are the formation of canals, docks and ramps, the quarrying, movement and dressing of stone blocks, and the construction of temples, causeways and boat-pits.

The contribution that the pyramid construction project made to the formation of the Old Kingdom State of Egypt is the subject of the final part. The project consolidated 'the Two Lands into a unified, territorial state' (p. 288). Workmen who were members of households and villages were welded into work groups with titles to serve the state. This fostered a bureaucratic mindset that tracked products and people, and it broke down clan and regional allegiances. The pyramids and the processes employed to create them became part of the Egyptian identity.

The book has an index, bibliography for further reading and an Egyptian chronology. The text is admirably supported with photographs, tables, maps and diagrams. It is both a resource and a good read.

So much ancient history has no tangible presence in the modern world, but the pyramids of Giza remain some of the most extraordinary ancient monuments ever known. This book systematically and professionally uses the most recent discoveries and research to describe how the Great Pyramid was built and how the workmen involved lived. It demonstrates how to interpret ancient administrative documents, archaeological excavation findings, geological data, geography, and climate to envisage a credible construction program that achieved the incredible. This book is essential reading for anyone studying Old Kingdom Egypt, state formation, ancient engineering and complex society.



Paul Collins, 2021 *The Sumerians: Lost Civilizations*, London: Reaktion Books, ISBN 978-1789144154, pp. 214, illus. A$35.

Reviewed by Christopher J. Davey

The history and archaeology of ancient Mesopotamia has changed direction several times during the last two hundred years, and it continues to do so. Students during the latter part of last century struggled without up-to-date textbooks as the field had moved rapidly on from earlier concepts and theories. Books by Susan Pollock, Amélie Kuhrt, Nicholas Postgate, Marc Van De Mieroop, and Morgens T. Larsen in the last twenty-five years have addressed the more recent evidence. Whether Paul Collins' book joins these tomes, will depend on the acceptance of its controversial conclusions.

Paul Collins is the Jaleh Hearn Curator of Ancient Near East at the Ashmolean Museum and holds Fellowships at Jesus College and Wolfson College, University of Oxford. He has previously worked as a curator in the Middle East Department of the British Museum and the Ancient Near Eastern Art Department of the Metropolitan Museum of Art. He is currently Chair of the British Institute for the Study of Iraq.

After a brief reference to the city of Eridu, the opening chapter of *The Sumerians* focuses on the history of Mesopotamian archaeology and historiography. It moves comfortably through descriptions of the discovery, exploration and geography of Sumer while addressing issues such as Orientalism and the ideas of civilization. Collins includes a remarkable amount of detail to give character and authority to the narrative, and passes over matters that may distract from the story. Claudius Rich's Quakerism and Henry Layard's Unitarianism go unmentioned, but Major-General Henry Rawlinson's exploits at Behistun are described in some detail, as are their irrelevance to the decipherment of cuneiform and the discovery of the Sumerian language, both of which were accomplished by the Reverend Edward Hincks.

The problematic identification of the Sumerians is discussed in chapter two beginning with the context of nineteenth century European academic concepts of 'race'. These scholarly prejudices were applied to many of the world's peoples at that time, including Australia's indigenous population. Collins describes how the Sumerian language and physical appearance led to an alignment with 'Aryans' from the East, who were to be distinguished from the bearded Semitic Akkadians from the Arabian deserts. The 'Aryan' Sumerians were deemed to be the origin of Western civilization, an idea that went out of favour after World War II. The question of Sumerian identity is not answered at this stage and it becomes the book's main theme.

The progress of Sumerian archaeology in the twentieth century is described in the context of the politics, nationalism and heritage of Iraq and the accumulation of knowledge of their material culture from archaeology. There are many references to names of people and places that will challenge newcomers to the subject. The chronology at the beginning of the book will assist with some of the details, but it is the knowledge of and attitude to the Sumerians that is important. The French excavation of Tello (Girsu) and the finds from it in the Louvre are mentioned, but it is the British activity that is focussed upon. Hall's excavations at Tell al-'Ubaid found significant material culture deemed to be Sumerian, but as Collins points out, the history of Sumer was reconstructed from cuneiform texts using a 'rise and fall' of civilisation motif, which he deems to have been unhelpful (p. 66).

Fifteen pages are devoted to the excavations at Ur, most of which deal with the Royal Tombs and the finds from them, including the Standard of Ur. Collins discusses the interpretation of the tombs concluding with more recent hypotheses and a comment that based on her name-form, Queen Pu-abi 'the most famous Sumerian queen turns out to be Akkadian!' (p. 79). The discussion of the discovery of flood levels at Ur leads to a description of the still largely unpublished excavations at Kish and the king lists found there distinguishing kings that ruled before the Flood from those who ruled after it. Collins alludes to the excavation director, Professor Langdon, and his preference for inscribed tablets over other excavated material. However the presence at Kish of stamp seals known from excavations in the Indus Valley

was considered important. Collins discusses how this bolstered the idea of the Sumerian's possible Eastern origins, which he links to extremist fascist beliefs (p. 91).

After a brief description of the German excavations at Uruk, Thorkild Jacobsen's rejection of the Sumerian-Akkadian conflict hypothesis and the concept of Sumer's 'primitive democracy' are discussed; neither remained popular after the 1930s. The proposition that Sumerian governments associated with temple complexes and irrigation organisation also failed to enthuse scholars. Collins' discussion of these hypotheses in parallel with Iraq's political and cultural development makes interesting reading. The ideas of Samuel Kramer, Ignace Gelb, Benno Landsberger and popularists, such as Erich von Däniken, are mentioned even though none of them have endured.

Chapter Five is where substantive matters are considered. It begins with a discussion of the Jemdet Nasr period (c 3,100 BC) administrative system at Uruk as evidenced by seals and tablets, which are generally thought to have been written in the Sumerian language. Collins queries this because there are no Sumerian name-forms in them (p. 131). After briefly mentioning the marsh environment of the 'Ubaid period, Collins then focuses on the end of that period when there were large Uruk style cities and settlements at Tell Brak, Habuba Kabira and Jebel Aruda in northern Syria and he questions the primacy of urban development in southern Mesopotamian. Some large cities in southern Iraq are also mentioned as are domestic architecture and lifestyle at Tell Asmar in the Diyala region. The origins of warfare and the establishment of empires are considered, before the chapter finishes with the Old Babylonian period when, according to Collins, Sumerian was no longer spoken but scribes continued to learn the language as is indicated by the many 'practice tablets' of Sumerian literature that have been found.

The final chapter describes how later Mesopotamian kings, Kassite, Elamite and Assyrian, often sought legitimacy by identifying themselves with the traditions of early Mesopotamian society and its religion. The Sumerian language was retained until the end of the Neo-Assyrian Empire as a link with the past. Ashurbanipal, for example, claimed to read Sumerian demonstrating his 'wide knowledge and broad perception' (p. 177).

Collins concludes that the Sumerians may never have been lost because they never actually existed as a distinct ethno-linguistic population (p. 181). He suggests that they are the creation of modern Assyriologists who assumed that the Sumerian language must have been spoken by an ethnic group, when in fact it was a language devised by a scribe probably in the Late Uruk period.

Interestingly, when introducing another recent study of the Sumerians, *The Sumerian World* (London: Routledge 2016), Harriet Crawford acknowledged that 'there is no physical anthropological evidence to support' the existence of the Sumerian people and that therefore only Sumerian culture and language would be discussed in that book. Apparently, until the Sumerian's DNA is discovered, they are considered not to have existed.

This reviewer is far from convinced. It may well be the case that modern descriptions of Sumerian culture and identity have uncritically included Akkadian and early Mesopotamian characteristics, but it is not clear that all aspects of the descriptions of Sumer derive from these cultures. The world that Collins studies is post-3,500 BC. At Eridu there is a well-known temple sequence, not mentioned by Collins, that began in about 5,000 BC and continued until the Late Uruk period (3,500–3,100 BC). Recent surveys confirm that Eridu and the other towns and cities of southern Mesopotamia began in a maritime environment. Collins' proposition assumes that it was the farmers from northern Mesopotamia or the Semitic speaking people from the desert who became boat builders and sailors. This is unlikely. The idea that the Sumerian language was created by a scribe is also implausible in the absence of a credible linguistic explanation for such a process.

The final chapter also describes how in 2016 the sites of Uruk, Ur and Eridu and four wetland marsh areas were inscribed on the World Heritage List. Collins sees this as the land of Sumer. If the book began by exploring the early world of the marsh environment and the marsh-dwellers and their interactions with neighbouring peoples at sites such as Eridu, Uruk and Ur, it may not have concluded as it does. It certainly would not have left the fundamental question of Sumerian existence to last three pages of the book.

The book's illustrations are of high quality and directly relevant to the content. Those wanting to research a particular topic will find endnotes, a bibliography, and an index to begin the analysis. The early chapters include concise explanations so that no specialist knowledge is required, but the last two chapters do not flow in a coherent sequence. The relationship between the history of modern Iraq, the progress of archaeology and the development of Assyriology is interesting. But the contention that the inhabitants of Sumer derived from ancient farmers and desert dwellers only appears credible because the book starts with the Late Uruk period, at least 2000 years after archaeological evidence for Sumer, at sites such as Eridu, first appears.

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