SIR JOHN GARDNER WILKINSON (1797-1875): LOST WORLDS OF ANTIQUARIANISM IN EGYPT AND THE BALKANS

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Abstract

This project takes an "intellectual history" or "history of ideas" approach toward reconsidering the work of Sir John Gardner Wilkinson (1797-1875), who has been referred to as "the father of British Egyptology".

Existing interpretations of the history of archaeology and Egyptology in the early 19th century are exclusively dominated by a focus on the race to decipher hieroglyphs, the discovery of tombs and the scramble for collecting antiquities, a state of affairs which are presented as giving way to "scientific" archaeology as the 19th century progressed.

However, I argue that Wilkinson, and other early 19th-century scholars, pioneered an antiquarian approach, borrowing from a strong antiquarian tradition of scholarship widespread in 18th and 19th century Britain, and that they were interested in an array of research questions, which extended well beyond hieroglyphs and antiquities. Whereas later Egyptian archaeology focused on excavation and reconstructing individual site histories, Wilkinson studied classical texts and indigenous Egyptian material culture to gain insights.

Rather than studying Egyptian antiquities and monuments in isolation, he sited them in their original context, thus demonstrating a high degree of spatial awareness. This is notably evident in two of his research projects.

First, Wilkinson was interested in ascertaining the former extent of the ancient Egyptian empire in Nubia and Asia. This depended on fieldwork which was contingent upon the location of antiquities and monuments: temples, fragments of temples, boundary stones and ancient frontier fortresses. When Wilkinson was denied access to such sources, he turned to relief scenes of temples such as the Ramesseum and Medinet Habu, to reconstruct ancient Egyptian history and geography.

Second, Wilkinson conducted geomorphological research into the nature of the ancient Egyptian environment, which he then used to draw conclusions about Egyptian prehistory and economic development – an approach that was only rediscovered in the late 20th century. To further upset the current narrative, Wilkinson, and other antiquarian colleagues, showed that the first stratigraphic excavation, supported by leading

professional scientists such as Lyell and Darwin, relied on highly problematic assumptions.

Finally, in addition to his work in Egypt, Wilkinson undertook an extended research programme in Dalmatia and Montenegro, in the Balkans. Here, he carried over his antiquarian approach, of giving equal emphasis to texts and material sources, in an investigation of the classical topography of the region, its history, and its peoples: the last of these also drew on romantic antiquarianism, conditioned by classical sources.

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Chapter 1: Introduction

1.1 Gardner Wilkinson and Henry Wyndham Phillips

At some time in 1843/ 44, a young man in his early twenties, named Henry Wyndham Phillips, finished a portrait of a middle-aged man named Sir John Gardner Wilkinson (Fig. 1.1). Phillips had first exhibited at the Royal Academy five years earlier and would go on to paint several dozen eminent Victorians (Peach, 2008). Several were prominent statesmen or scientists, while others had become renowned for their travels abroad in the Near East. Some of the latter included William R. Hamilton, Lucie Duff Gordon, Austen Henry Layard and Henry Creswicke Rawlinson.[1] The latter two had moved into politics. Most wore clothing typical of their time; only Phillips's portrait of Layard showed anything atypical: Layard wore Arabian robes, and stood against a nondescript background. Phillips's painting of Wilkinson on the other hand, was something very different.

Phillips depicted Wilkinson sitting cross-legged, in Turkish dress. On his upper body, this consisted of a striped, probably silk shirt, and an open patterned black jacket. Wilkinson also wears Turkish trousers, *shurwal*, which were usually held up by a drawstring at the waist, not visible in the painting. On his head, Wilkinson wears two caps: a white one close to his head, and a red one, a *tarboush*, with a tassel. Both are only just visible, below a far more striking item of dress, a red-pink turban. In his left hand, resting on his lap, Wilkinson holds a Turkish sword, resembling a *scimitar* or *khanjar* sword, enclosed in a scabbard (Thompson, 1992a: 39; Thompson, 2010: 47-48).[2] Wilkinson gazes to one side, sporting a prominent moustache, though no beard. He is untanned, but the overall impression is of a man who was quite different from his contemporaries in early Victorian Britain.

[1] National Portrait Gallery (2022). Henry Wyndham Phillips (1820-1868), Painter, Available at: https://www.npg.org.uk/collections/search/person?LinkID=mp03546&role=art&wPage=1 [Accessed 7th January 2022].

[2] "'Journal', 1821-2", Bodleian Library Wilkinson manuscripts, dep. d. 47; Wilkinson had a collection of Egyptian swords, see "Miscellaneous loose papers of Lady Wilkinson, *c*.1850-1880", Bodleian Library Wilkinson manuscripts, dep. d. 158.

Image omitted due to copyright restrictions.

Fig. 1.1: Sir John Gardner Wilkinson in Turkish dress, by Henry Windham Phillips. Source: The National Trust. Available at: https://www.nationaltrustimages.org.uk/image/986832

The background of Phillips's painting is also highly unusual. The most notable feature is that Wilkinson sits on a large patterned pink carpet: to contemporaries, this would have been a clear signifier of the Orient. Above this, but below the horizon of the dark blue sky that dominates the upper half of the image, Phillips added a number of features, in varying shades of brown and beige, to represent a desert landscape, despite the fact that he was painting Wilkinson in England, probably in his studio. On the far left, two structures sit side by side, at an angle. On the far right, on the other side of Wilkinson's body, is a more extensive ruin, while raised hills stand behind this.

The landscape was not an imaginary one, but was in fact one with which Wilkinson was intimately familiar. The structures on the left can be identified as the Colossi of Memnon, two huge 15-metre-high sculptures, each carved out of a single block of stone, in the 14th century BC. The more extensive structure on the right is the Ramesseum, a slightly later

Pharaonic monument, but less well-preserved: its main colonnade can be made out, as can some of the other standing pillars. The hills in the background are the Theban Hills. Beyond these lay the Valley of the Kings, the famous burial site of ancient Egypt's New Kingdom Pharaohs. Upon them were the entrances to the tombs of the nobles, in one of which Wilkinson lived for several years, while conducting fieldwork and collecting material which he later used in his groundbreaking *Manners and Customs of the Ancient Egyptians* (1837) (Thompson, 1992a; Thompson, 1996). Effectively, the entire scene is the one that Wilkinson encountered each time he left his house, except in reverse.

A thin blue line in the middle right of the painting, likely the River Nile, separates Wilkinson from these features. Precisely where Wilkinson is sitting is harder to ascertain, but, given his height in relation to the features on the opposite bank, and the flat stone-like surface in the foreground, as well as the apparently circular structure beneath the carpet, it is possible that Phillips placed Wilkinson on top of one of the drums on the colonnade of the temple of Luxor – the most prominent Pharaonic-era structure on the opposite bank, after the ruins at nearby Karnak.

Image omitted due to copyright restrictions.

Fig. 1.2: Photographs of Wilkinson in later life ("Letters, 1857-8", Bodleian Library Wilkinson manuscripts, dep. e. 76.)

Several other images of Wilkinson exist, including at least three photographs. All three of these convey the impression of a typical gentleman scholar. In one he is shown seated and reading, in another seated at a table with a cane, and in a third sitting in what appears to be a library (Figure 1.2).[3] A woodcut and at least two portraits also exist.[4] Nearly all of these show Wilkinson in later life. Phillips's painting of Wilkinson, on the other hand, shows him at a time closer to when he was undertaking pioneering research – just over a decade after his return from Egypt, and just before he undertook an in-depth study of Dalmatia, Montenegro and bordering parts of the Ottoman Empire.

Wilkinson's portrait makes a fitting starting-point on two other counts. First, it introduces a number of the sites and themes which will be prominent in the following chapters: the Ramesseum at Thebes and the Colossi of Memnon will feature in the fourth and fifth chapters respectively, while the representation of people in traditional dress will do so in the sixth. Second, the overall impact of Phillips's painting echoes one of the central arguments forwarded in this thesis. Namely, Wilkinson was a scholar who looked at sites in the context of the wider landscape – something that has been frequently noted at a superficial level for antiquaries, but something which archaeologists, who in the late 19th century were mainly concerned with reconstructing the history of individual sites through excavation, seldom did (Koelsch, 2013).

The aim of this thesis will be to show that Wilkinson was, first and foremost, an antiquary, and that he approached ancient Egypt and the Balkans using an antiquarian methodology, which assigned equal value to ancient texts and material culture, as opposed to elevating one over the other. This holistic approach was strengthened furthered by his outdoor fieldwork, and his constant attention to geographical context. By classing Wilkinson as an "antiquary" and using the word "antiquarian", this thesis does not align itself with the negative way in which the term was used for much of the 20th century – often in a superficial way by those seeking a synonym for academic provincialism – and which still lingers, but is becoming increasingly rare in scholarly

[3] "Letters, 1857-8", Bodleian Library Wilkinson manuscripts, dep. e. 76.
[4] Victoria and Albert Museum (2022). GW. [Sir John Gardner Wilkinson]. Available at: https://collections.vam.ac.uk/item/O153103/gw-sir-john-gardner-wilkinson-watercolour-vigne-godfrey-thomas [Accessed 7th January 2022]; National Portrait Gallery (2022). Sir John Gardner Wilkinson. Available at: https://www.npg.org.uk/collections/search/portrait/mw06787/Sir-John-Gardner-Wilkinson [Accessed 7th January 2022].

circles. Instead, it takes its lead from what may be called the "rehabilitated antiquarianism" that reassessments of the term since the late 1990s have arrived at. Classing Wilkinson within this intellectual lineage is to place him in a highly appropriate contemporary context: his scholarship owed much to the direct and indirect influence of more than a dozen antiquaries, active during the preceding two centuries, who followed the methodological approach just outlined.

By positioning Wilkinson's work in relation to that of his predecessors – rather than assessing his contribution to modern scholarship on ancient Egypt and necessarily emphasising aspects which have since become scholarly consensus – it becomes possible to appreciate that many histories of the scholarly tradition have been written in an overly schematic format. Almost universally – as a result of self-congratulatory early 20th century biographies and as a result of more recent work which has largely followed them or else prioritised the collection of antiquities above other lines of enquiry – a narrative exists in which early 19th century looters and crude amateurism gave way to "scientific" and "systematic" archaeological work, which culminated in the development of stratigraphic excavation. However, as this thesis seeks to show, if the museum and university-based professional Egyptology of the later 19th century was absent at the beginning of the century, there nevertheless existed an earlier antiquarian scholarly tradition which was no less serious, and in fact encompassed research priorities that archaeology would later expand into, during the 20th century. In other words, at least in some respects, the earlier antiquarian tradition of scholarship on ancient Egypt was at least as encompassing as the more narrowly-defined academic field that replaced it. Even when it came to more sophisticated excavation, of which one particular programme is given sustained attention, antiquarian scholars such as Wilkinson were among the individuals who best understood its profound limitations. In making this argument, this thesis certainly does not seek to advance an extremist position that no scholarly progress or better understanding has developed since the early 19th century. Rather, to reiterate, it is to suggest that disciplinary histories have repeatedly followed an overly restrictive framework, which has obscured how several approaches, notably spatial ones normally considered as 20th century developments – followed by Wilkinson in both Egypt and the

Balkans – have a genealogical history stretching back to, and even before, the early 19th century.

1.2 Wilkinson's life and times

Wilkinson was born on 5 October 1797, either in Hardendale, Westmorland, or Little Missenden Abbey, Buckinghamshire (Shaw and Nicholson, 1995: 305; Thompson, 1992a). His parents were John Wilkinson and Mary Anne Gardner Wilkinson, who moved south and had their son baptized in Chelsea, then on the outskirts of London. Unfortunately for Wilkinson (who took on his mother's maiden name as his first name), John and Mary both died in the first decade of the 19th century, leaving their only child in the care of a Dr. Yates. Probably due to an accident of birth rather than an accident of geography, Wilkinson was sent to a nearby school, Harrow, in 1813. In 1816, he entered Exeter College in Oxford. Precisely what Wilkinson achieved at Oxford is unclear and it seems that he left without a degree. His preferred career choice was the army, and he attempted to purchase an officers' commission. Finding that the demobilization following the end of the Napoleonic Wars had reduced the number of places, he took time out to build up his surveying and technical skills, and also went on a Grand Tour of France and Italy.

A turning-point of sorts happened in Rome in 1820: Wilkinson met the antiquary William Gell, who was well-known for his research on Troy and Pompeii. Gell was also interested in Egypt, and he recommended Wilkinson to visit the country, as many scholarly questions – including but by no means only the deciphering of Egyptian hieroglyphs – remained unsolved. As Wilkinson's parents had left him an income, taking time out for scholarly activity was not overly problematic, and he left for Egypt, accompanied by a friend, Samuel Wiggett, in October 1821, arriving – under sail – a month later. The two men, having visited Rome before the growth of the modern city, were able to experience the same relatively small Alexandria, but went on to Cairo, Thebes and up the Nile. Wiggett returned home but Wilkinson, for reasons of his own, stayed on. He ended up remaining in the country for twelve years, an important formative time of his adult life.

In many ways, Wilkinson arrived in Egypt at an opportune time. Muhammad Ali, formerly an Ottoman soldier, was the ruler and had broken with Constantinople, making Egypt independent in all but name. Rather than heading for an isolationist foreign policy, Muhammad Ali sought to make Egypt a power in the region, and set in train an industrialisation programme (Dodwell, 1931; Sayyid-Marsot, 1984). Muhammad Ali, eager to secure industrial expertise, openly encouraged Europeans to settle in Egypt, with some success. Alongside engineers, many tourists and antiquaries, including Wilkinson, visited the country, some staying for extended periods. After the departure of Wiggett, Wilkinson joined up with another Briton, James Burton, who worked as a geological surveyor for Muhammad Ali (Thompson, 1992a). The two men travelled around Egypt, travelling up and down the Nile. They also made several forays into the Eastern Desert, which was then, to Europeans, relatively unexplored territory. In time, more Britons arrived in Egypt. Robert Hay, a Scottish aristocrat who travelled with a team of artists, including Joseph Bonomi, was directed by Gell to see Wilkinson. Edward William Lane, an engraver, took a more scholarly direction, into both ancient and modern Egypt (Thompson, 2010). Algernon Percy (Lord Prudhoe) and Major Orlando Felix travelled together on two separate occasions. With this network of men who often travelled in twos or threes and were widely dispersed throughout Egypt, Wilkinson was able to source more information than he would have been able to find himself, something which was invaluable for his topographic project.

One of Wilkinson's more unusual contacts was Osman Effendi – who, despite his name was in fact a Scot, who had converted to Islam after being captured as a prisoner of war (Thompson, 1994). Osman helped Wilkinson settle in, as he did with many of the other Britons who visited. One notable piece of advice that he gave Wilkinson was on how to dress: by donning Turkish costume, Wilkinson would not only be able to fit in – an advantage which Britons visiting Egypt in previous decades had found a necessity – but he would be able to align himself with Egypt's ruling class rather than the Egyptian peasantry (Thompson, 1992a; Schmidt, 2011; Rodenbeck, 2001). But in following this advice, Wilkinson found himself in conflict with Henry Salt, the British consul in Egypt, who pressed British travellers to dress as Europeans, in a drive to make the most of Muhammad Ali's reforms which were laying the foundations for Europeans to parade as a

privileged minority. Wilkinson also looked down on Salt for his involvement in buying antiquities, but one reason why he himself eschewed this – on several occasions he actively opposed taking antiquities to Britain – was that his independent income allowed him to stay aloft from such money-making schemes. Wilkinson's attitude to Salt was also typical of his classism – he saw himself as superior to most British men, whom he caricatured as heavy drinkers given to beating their wives. However, Wilkinson's attitude was extremely hypocritical, given his own indefensible act of allowing Osman to acquire for him at least one female slave from the Cairo slave market to attend to domestic duties. Early 19th century Egypt, like early 19th century Britain, had not abolished slavery (Powell, 2003).

Wilkinson continued to travel around Egypt throughout the 1820s, but his work seems to have increasingly focused on the ancient ruins at Thebes (across the river from modern Luxor). Following the example of the local inhabitants at Gurna, he partitioned the superstructure of a tomb for use as a house. From here, he was within trekking distance of many of the most important sites of New Kingdom Egypt, including the Biban el-Muluk (soon to be christened the Valley of the Kings by François Champollion), the Tombs of the Nobles, the mortuary temples of the west bank including the Ramesseum and Medinet Habu, and the grand temples on the east bank, Luxor and Karnak. The study of these was one of his major contributions to the scholarship on ancient Egyptian civilization. Wilkinson imposed some degree of order on the Valley of the Kings, by devising a numbering system, which enabled tombs to be referred to without resorting to lengthy descriptions. At the Tombs of the Nobles, one of which Wilkinson had taken as his house, Wilkinson studied wall paintings depicting the everyday life – work, games, sports and animal life – of New Kingdom times. In the mortuary temples, Wilkinson copied inscriptions and relief scenes of battles. Wilkinson's familiarity with the landscape was considerable, and he used this knowledge to produce a map, *The Topographical Survey of Thebes*, of the whole region on both banks of the Nile, with a level of detail that few, before or since, have equalled.

It may well have been at Thebes that Wilkinson began to draft several manuscripts, which were eventually published, as books and journal articles. Among the first of these was *Materia Hieroglyphica* (1828), a sustained attempt to establish a chronology of

Egyptian kings, as well as a preliminary exploration of ancient Egyptian religion. Another work – the publication of which was delayed as a result of the plague in Alexandria – was *Topography of Thebes, and General View of Egypt* (1835). As the name suggested, Thebes featured prominently, and Wilkinson intended it to be read alongside his map, a vision unfortunately held back by high printing costs. However, the book was much more than a description of Thebes, as it included many Pharaonic sites throughout the country. Some of these had become known due to the result of Wilkinson's own explorations, such as Tel el-Amarna, later to become renowned as Akhetaten, the city of the heretic Pharaoh Akhenaten, and his wife Nefertiti. Others were places which Wilkinson had learnt about from his friends. Wilkinson included a variety of other topics, including his work on hieroglyphs; in this he sided with François Champollion's system, which was still in dispute, especially amongst English scholars who felt, with some justification, that Champollion had plagiarised the leading scholar of their country, Thomas Young.

After his return to England in 1833, Wilkinson published an even longer study of Pharaonic Egypt: *Manners and Customs of the Ancient Egyptians* (1837). The genre was a standard one in proto-ethnographical circles, but Wilkinson's applying it to a past people was quite unusual, with only a few precedents. By giving attention to a brilliant range of aspects of ancient Egyptian society, Wilkinson was able to rehabilitate the ancient Egyptians from the negative image which the Bible had conveyed, and make them appear similar to modern Britons of the Georgian era, with recognizable industries, pastimes, games, and even objects including musical instruments, furniture and kitchen utensils. Partly from these factors, and partly from the many dozens of woodcut illustrations which featured throughout, the book was a commercial success: John Murray II reissued it in an expanded edition in 1841, in five volumes, using material which he had initially forced Wilkinson to cut. Nor was interest in ancient Egypt restricted to this somewhat detached perspective: increasing numbers of Britons were visiting the country, and Wilkinson and Murray capitalised on this by releasing an expanded version of Wilkinson's *Topography of Thebes, and General View of Egypt* as a two-volume work, *Modern Egypt and Thebes* (1843), which later became Murray's *Handbook for Travellers in Egypt* (1847).

In addition to his scholarship on Egypt, Wilkinson developed a new interest in the Balkans. In 1844, he undertook a trip to the region, visiting coastal towns such as Pola

(Pula) and Spalato (Split) which had formerly been part of the Roman Empire, and then the Venetian Republic, which had ceased to be an independent city-state in the year of Wilkinson's birth. To many other British observers, these regions were now simply another downtrodden part of the Habsburg Empire but, to Wilkinson, with his fascination for the ancient past, they were an opportunity for investigating the topography of Roman Dalmatia. As in Egypt, Wilkinson took a holistic attitude, encompassing antiquities and the natural environment. More adventurously, although hardly an intrepid expedition into unknown territory, Wilkinson made forays inland, including to Montenegro, a small independent state that had managed to retain its existence between the Habsburg and Ottoman Empires. Wilkinson also visited Bosnia in the Ottoman Empire. If his views of modern Egypt had been for the most part open-minded, Wilkinson's views of Islam in the Balkans were less tolerant, and his view of the Ottoman Empire and Albania, two Muslim-majority states, was overly coloured by his sympathetic attitude toward the plight of the Montenegrins, whom he saw through a classical lens, engaged in a David-and-Goliath like struggle against their neighbours.

Wilkinson's interest in the Balkans was not a passing one. For several years after his trip, he continued his research on the region, corresponding with local notables (Carrara, 1848). In 1848, Murray published the results of his research, *Dalmatia and Montenegro*, a two-volume work which was as definitive an account of the history, manners and customs of the people of this region as his *Manners and Customs* had been for the ancient Egyptians. In many ways, the work took advantage of the British enthusiasm for travel writing (Leask, 2002; Keighren *et al.*, 2015), and it contained a number of illustrations from a far larger selection that Wilkinson had produced while travelling through the region.[5] Due to its contemporary focus, and Wilkinson's sympathy for the

[5] "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. b. 2; "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. c. 1; "Sketchbook, 1844-5", Bodleian Library Wilkinson manuscripts, dep. c. 2; "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. d. 23; "'Journal, 1843-4'", Bodleian Library Wilkinson manuscripts, dep. d. 56; "'Journal, 1844'", Bodleian Library Wilkinson manuscripts, dep. d. 57; "'Journal, 1844-5'". Bodleian Library Wilkinson manuscripts, dep. d. 58; "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. e. 7; "Sketchbook, 1844-5", Bodleian Library Wilkinson manuscripts, dep. e. 8; "Sketchbook, 1844-5", Bodleian Library Wilkinson manuscripts, dep. e. 9.

Montenegrins, the work was also a political one, a dimension that was brought out even more by its being published during a time of great political upheaval in Europe.

Dalmatia and Montenegro was received positively and helped to cement Wilkinson's reputation as a scholar who was happy to undertake fieldwork (Anon., 1849), similarly to when he returned from Egypt to acclaim in society, when he was received into many London clubs, including the *Royal Geographical Society* and the *Athenaeum* in 1836.[6] The *Quarterly Review* (Anon., 1835) praised Wilkinson for single-handedly achieving as much as the Franco-Tuscan state-sponsored expedition had achieved over several years and at great expense.

In the 1850s and 1860s, Wilkinson's research interests remained wide, encompassing subjects as diverse as hieroglyphs, Venetian mosaics, cuneiform, medieval architecture and oysters (Wilkinson, 1851; 1852; 1857a; 1861; 1865). Not all of his projects were successful: an investigation of Greek vases got nowhere, while *On Colour and on the Necessity for a General Diffusion of Taste among all Classes* (1858) was less well received than Wilkinson had hoped: Ruskin largely ignored it, while others had a mixed reaction (Anon., 1858). Wilkinson retreated from active research into ancient Egypt, but remained a recognised authority on the subject, both among the new professional Egyptologists based at the British Museum, such as Samuel Birch, and the public alike: he wrote three more books on the subject, largely sourced from his earlier work, covering Pharaonic architecture and history (1850; 1854; 1857b). Wilkinson remained sufficiently up to date with current work in Egypt to offer expertise to others; he also corresponded with many other leading intellectuals of the mid-Victorian era (Thompson, 1992a). One of these links, to Lady Llanover, resulted in a development that surprised Wilkinson's friends: he married a woman called Caroline Lucas (who became Lady Wilkinson) in 1856. The two also moved from London to South Wales, where they bought Brynfield, a manor house on the Gower peninsula. Here, Wilkinson developed a new interest, in ancient British megaliths, which he investigated in Wales and while visiting friends in the Peak District (Briggs, 2008a; 2008b). This was Wilkinson's last act of intellectual branching out and he died in 1875.

[6] I would like to thank Jennie De Protani at the Athenaeum for providing me with this information (21.6.21).

1.3 Themes, aims and chapter outlines

Wilkinson is well-known amongst Egyptologists today and, due to the scale of his work, which took place across a vast array of sites, as well as his interactions with others, his name frequently appears in the literature, especially that produced recently, as archival work has become more widely used in the discipline (Malek, 2003; Bednarski, 2015). In part, this is due to the pathbreaking biography of Wilkinson's life and times, written by Jason Thompson (1992a), an account from which the preceding biographical overview was largely drawn, and which remains difficult to better, in its broad outlines. Most of the recent histories of Egyptology, especially those since the 1990s, and some of archaeology, typically devote several pages or even a whole chapter to Wilkinson (James, 1997: 101-126; Romer, 1981: 97-112; Wilkinson, 2020: 105-142; Naunton, 2020: 110-119). Even outside Egyptology, Wilkinson has been cited several times for his research into prehistoric megaliths in Wales (Naylor, 2003; Briggs, 2008a; 2008b) and to a lesser extent among cultural historians for his work on Venice (Laven and Damien, 2015). As such, although Wilkinson is less well-known than scholars of ancient Egypt such as Howard Carter, François Champollion and Flinders Petrie, it would be a big mistake to present him as a forgotten figure in need of recovering, in the manner that Robert Morkot (2017) has done for the obscurer figure of Thomas Lyme, or to suggest that a new biographical account is required.

Instead, this thesis seeks to show that the actual dimension that has been marginalised, or at any rate oversimplified, is the particular approach taken by Wilkinson: an antiquarian one, in which Wilkinson devoted equal emphasis to texts on the one hand, and material culture – including paintings, objects and monuments – on the other. This was an extensive way of working, as opposed to an intensive one, based on site history. As such, it breaks with existing accounts of Wilkinson's scholarly contribution, which overwhelmingly concentrate on and separate out his philological research into hieroglyphs from his artistic work, which culminated in his *Manners and Customs of the Ancient Egyptians* (1837; 1841). Both dimensions were crucially important to Wilkinson, but at the same time, their separation into two distinct projects (the latter being held as more successful) imposes 20th century disciplinary boundaries onto Wilkinson's work,

with the interrelationships underemphasised.[7] Additionally, both pay insufficient attention to the environmental context, often relegating this to a scenic backdrop, which Wilkinson only appreciated in picturesque drawings and sketches. However, there are strong grounds for saying that the landscape, in which Wilkinson lived and worked for over a decade, was – if not in any way determinative – a lens which he used to view ancient Egypt and which conditioned his ideas about its history.

As has already been alluded, reference to Wilkinson solely as an Egyptologist, even by a broad definition, is inadequate for an additional reason: his research interests extended well beyond Egypt's borders, into – besides prehistoric Britain, an area that has already been examined in detail in other studies (Briggs, 2008a; 2008b) – the Balkans, especially the Roman, Medieval and Early Modern periods: eras to which antiquaries gave much prominence. Though it has been suggested that, when not in Egypt, Wilkinson was "an Egyptologist abroad" (Briggs, 2008a), it is important to recognise that Wilkinson's research in other regions was largely independent of his work in Egypt: it did not make use of hieroglyphs, nor did it draw on his knowledge of Pharaonic era material culture, nor familiarity with stylistics and the Nilotic environment that he had built up in that region. This is definitely not to deny that there were prominent commonalities, of which the most significant were underlying beliefs about global connectivity (notably between Egypt and India), an interest in ancient geography – based, as in Egypt, on reading classical texts directly onto the landscape and material culture including stylistics – and a consideration of the impact of the environment on human development (not to mention beliefs about the origins of Egyptian and Balkan peoples). Yet they were not simply subordinate to his interest in ancient Egypt, and it is for this additional reason that the term "antiquary" is more suitable than "Egyptologist". Three different research areas that Wilkinson investigated will be considered under this rubric, explained in the following chapter summaries.

Following a review of the extant literature (Chapter 2) and a methodological overview (Chapter 3), the fourth chapter**,** on Wilkinson's scholarly work in Egypt, will pay attention to how Wilkinson conceptualised the geographical extent of the ancient Egyptian empire

[7] The two modern approaches are Egyptology (with its philological/ epigraphic focus) and archaeology (with its focus on material culture).

during the New Kingdom, to the south into Nubia (modern Sudan) and to the northeast into Palestine and Syria (modern Israel/ Palestine, Syria and Iraq), and beyond. Wilkinson relied on classical authorities, such as Herodotus, Diodorus Siculus and Strabo, but also used material evidence which he encountered during his fieldwork, including boundary stelae (inscribed stones), stylistically Ramesside temple ruins frequently incorporated into later structures, huge fortresses which had a spatial layout suggesting Egyptian ownership, and temple relief scenes. The result was that, whereas Wilkinson's predecessors had assumed that ancient Egypt had been confined to the Nile Delta and northern reaches of the Nile valley, Wilkinson demonstrated that ancient Egypt had been a truly mighty civilization in its own right – a conclusion which aligned closely with his rehabilitation of the ancient Egyptians as a people not dissimilar to modern Georgians, in his *Manners and Customs of the Ancient Egyptians* (1837). Not all of his conclusions are still supported, but they constituted a significant advance on previous understanding. The combination of using textual sources and the remnants of material culture (ranging from objects to monuments) has been understood to constitute a typically antiquarian methodology: this chapter thus lays out evidence to back up the contention that Wilkinson was an antiquary.

The fifth chapter maintains a focus on Wilkinson's investigation of the size of the ancient Egyptian state, but introduces a new dimension: geomorphological research in association with ancient monuments. Unlike in the previous chapter in which attention is given to the ancient Egyptian state beyond the traditional boundaries of Egypt, the focus here will be confined mainly to the Nile Valley and Delta. Wilkinson's most important fieldwork sites were the harbour of Alexandria, Heliopolis (near Cairo) and the west bank of Thebes, though he also conducted work elsewhere. His work indicated that, in the past, the historic environment of the Nile valley had been radically different to the present day – a research topic which was seldom pursued by archaeologists who came after him, and was neglected until as late as the 1980s (Jeffreys, 2010). Wilkinson's own approach involved the excavation of some pits, though this was not archaeology in the traditional sense of site excavation – he did not look for artefacts unlike other archaeologists. Instead, Wilkinson's approach paid more attention to spatial relations and trying to reconstruct what the ancient ones may have been, by extrapolating backwards

from several sites, to the whole of Egypt. Wilkinson's approach was largely valid and it was due in part to his antiquarian background that he avoided making mistakes which some of the otherwise renowned early archaeologists in Egypt, such as Joseph Hekekyan, who pioneered stratigraphy, did make.

The sixth chapter shifts from Egypt to Dalmatia, but continues the theme of investigating ancient geography, in this case a province of the Roman Empire, using clues from the present day. These were both similar and quite different to those that Wilkinson had drawn on while in Egypt. One commonality was that Dalmatia possessed large standing monuments, such as a Roman amphitheatre at Pola, and the ruins of Diocletian's Palace at Spalato. Whilst Wilkinson was eager to study these, he also looked for the less imposing remains of the towns of Roman Illyricum, including Salona and Burnum: essentially, he took the same topographical approach that he had in Upper Egypt, this time drawing on the Peutinger Table and a number of widely-dispersed classical references, as opposed to a single Roman itinerary. In addition to an interest in antiquities, Wilkinson also considered landscapes and ethnographic references (both material culture and cultural traditions) to establish a boundary between a classical Dalmatia on the one hand, and a deviant classical – almost Oriental – Montenegro on the other; the latter depended as much on allegorical references, associating the modern Montenegrins with the ancient Spartans or Hebrews, as realities. Overall, Wilkinson's approach to reconstructing ancient space in Dalmatia owed much more to visual culture, as well as the idea of travelling back in time that is often a prominent theme in travel writing, than it did to the more serious research that he had conducted while in Egypt, though ultimately the end result – of defining an ancient region – was similar.

With the focus on how Wilkinson's work makes sense under the rubric of antiquarianism, these chapters make no attempt to be a biography or to be comprehensive. The structure is thematic, and only roughly chronological: it appears that Wilkinson had drawn rough conclusions about the extent of the ancient Egyptian empire by the late 1820s, when his *Materia Hieroglyphica* (1828) was published, but he developed his ideas further after his return to England, and he returned to them again, on a return visit to Upper Egypt in 1848-49, two decades later. Although Wilkinson's most ambitious geomorphological research project at Thebes took place in 1830, his first, at

Alexandria, may well have taken place as early as 1821, immediately after his arrival in Egypt, and his last, a correspondence with Lyell on the Horner-Hekekyan investigation, took place as late as the 1860s. His research on the Balkans is more temporally constrained, taking place in the 1840s, although even here, Wilkinson maintained an active interest in the region for the rest of his life, both at a political level and at an academic one.

Chapter 2: Literature Review: Rehabilitated Antiquarianism and the antiquarian phase in Egyptology

Before considering Wilkinson's own antiquarian research in Egypt and the Balkans, which will be the subject of the following chapters, something first needs to be said about the relevance of the two terms "antiquary" and "antiquarianism" for characterising Wilkinson, given that both have accumulated a number of negative associations, and are still sometimes used negatively in non-specialist literature. First, consideration will be given toward how disciplinary histories of archaeology have characterised the antiquaries. Frequently, archaeologists have used antiquaries as a "defining other" to define their own disciplinary identity and wider imagined community.[1] In doing so, many have drawn on images from popular culture as opposed to considering the research contribution of the antiquaries themselves. Second, and much more extensively, consideration will be given to attempts by a range of scholars – a small number of archaeologists and a larger number of historians – to reconsider the contribution of the antiquaries, over the past century, and particularly the last two decades. The result has been what I refer to as a genre of "rehabilitated antiquarianism". Third, attention will turn to how "rehabilitated antiquarianism" is a potentially useful term to break an impasse which has developed in the history of Egyptology, with specific attention given to how it is relevant to Wilkinson.

2.1 Antiquarianism: Archaeology's defining "other"?

Broadly speaking, antiquaries have been characterised (and castigated) due to their supposed conformity to one of two stereotypes. One is that the interest of antiquaries – self-proclaimed or otherwise – was primarily that of a wealthy connoisseur who pursued an unscholarly quest for collecting beautiful objects or, at best, limited his interest to aesthetics/ taste. For example, Glyn Daniel, a prehistoric archaeologist, summarised "antiquarians and art historians" as "interested… in the curious, the beautiful and the old" (1975: 10). More recently, Alain Schnapp, in his consideration of the relationship of

[1] The term is of course borrowed from Benedict Anderson (1983), in his study of the origins of nationalism and nation states. For the British context, see Linda Colley (1992). Colley describes how anti-Catholicism formed a surprisingly large element of 18th century or Georgian patriotism/ nationalism.

archaeology and antiquarianism, suggests that "the antiquaries of the old school… were more concerned with the form of objects than with their evolution" (Schnapp, 1996: 305), and that theirs was an "age of random collecting and aesthetic pleasure for its own sake" (1996: 308). Some antiquaries were very wealthy collectors, and the history of antiquarianism has close ties with elites, but in many ways, this is far from being the whole story, as antiquarian interests extended down the social scale to some extent (Sweet, 2004; Gonzalez-Ruibal, 2017), a matter which will be mentioned again in due course.

Alternately, and very much contradictorily, antiquaries have at other times been characterised for being interested in objects utterly devoid of any aesthetic appeal, to the point of an unhealthy obsession.[2] This second accusation has been made on several occasions and is worth reflecting on at greater length due to its contradictory nature. One such example is a remark made by Clyde Kluckhonn, an American archaeologist active in the early 20th century, quoted without comment in the *Cambridge Illustrated History of Archaeology* (Bahn, 1996). Kluckhonn, referring to the history of Mesoamerican archaeology, saw an antiquarian phase which involved "a great deal of obsessive wallowing in detail of and for itself" (Bahn, 1996: 342).

Given that modern archaeology has staked its disciplinary identity largely around the study of all aspects of material culture – not just those that are aesthetically pleasing – details of which are logged meticulously to arrive at evidence-based conclusions and ideally allow future reinterpretation, Kluckhonn's characterisation of antiquarianism is strange. It would seem to bear a closer resemblance to a model for good practice, not something to be criticised. Ingo Herhlatz (2007) takes a slightly different perspective in attempting to explain why antiquarianism fell out of favour. However, he gives little explanation of precisely who stood in judgement over antiquarian research: there is the danger that the opinions of the non-scholarly, some of whom would likely be hostile to all forms of academic activity, are allowed to fill in for the absence of comments by scholarly successors of the earlier antiquaries.[3] One example of this scenario is in fact given by

[2] Numerous contemporary examples are given by Piggott (1989: 14-18; Sweet (2004: xiii-xiv) and Hill (2021: 26-29).

[3] For further details on the theme of the caricaturing of antiquaries, see Brown (1980) and Levine (1977). As Moshenska (2017) has shown, the public image of archaeologists is hardly flattering and bears little

Robert Cunnington, in his biography of his great-great grandfather, the Wiltshire-based antiquary/ archaeologist, William Cunnington. Here, the author gave an example from 1804 of a man called Joseph Cobb denigrating antiquaries for an unhealthy obsession with old coins, with those in worse condition being valued more highly. However, it is unclear whether Cobb had a genuine interest in the study of the past: Robert Cunnington described him only as "a businessman… who had been poking fun at cranky antiquarians" (Cunnington, 1975: 4). Whatever the exact details, William Cunnington was forced to write to the man and explain what his scholarship actually involved.

The same issue has been noted by the archaeologist and scholar of antiquarianism, Stuart Piggott (1989). In his consideration of the appearance of antiquaries in novels, plays and cartoons, he concluded that the only firm conclusions which can be drawn from such material are that antiquaries had a public presence; there are no grounds for drawing conclusions about incompetence. Not noted by Piggott, but nevertheless very relevant, is that George Cruickshank's cartoon of Lord Aberdeen's election to the Society of Antiquaries, which has often stood in for conclusions that meetings of the group were chaotic (Nurse, 2000), has a precisely similar equivalent in a *Punch* cartoon of a meeting of the renowned British Association for the Advancement of Science of a century later, featuring many eminent scientists behaving absurdly (Driver and Rose, 1992). Given that the latter scene is not seen as a summary of the level of competence of the BAAS, there is no reason to draw wider inferences from Cruickshank's cartoon.

Explaining why at least some archaeologists and historians have been willing to denigrate past scholars, not so much due to an assessment of the strengths and limitations of their research outputs, but through forwarding arguments which ultimately stem from non-scholarly or even outright anti-academic opinion and media, can be explained through considering them within the broader historiography of the history of archaeology and history, as it was written by archaeologists and historians in the mid to late 20th century. The focus below will be on archaeology.[4]

resemblance to what archaeologists do, and historians of archaeology would not draw on such sources for their own disciplinary histories – yet they do not hesitate to draw on such imagery when considering the antiquaries.

[4] Historians will feature in a later part of the literature review.

Several archaeologists – and the occasional non-archaeologist – have written histories of the development of their discipline, over the course of the 20th and 21st centuries (Ceram, 1952; Daniel, 1975; Stiebing, 1993; Bahn, 1996; Cline, 2017). Others have described and explained archaeological methodology (Petrie, 1904; Wheeler, 1954; Renfrew and Bahn, 2020). Inspired by the spectacular discoveries of the late 19th and early 20th centuries in the Near East in particular, but also by the bewildering and apparently ever-increasing number of novel techniques that have been developed to source new information about the past, nearly all scholars of archaeology have produced narratives of improvement over time, culminating in a modern professional discipline that is "scientific" and "systematic" (Ceram, 1952; Daniel, 1967; Renfrew and Bahn, 2020). It is undoubtedly true that the 19th and 20th centuries saw impressive innovations among archaeologists, the consolidation of archaeology at universities, and a considerable growth in the number of archaeologists and excavation projects.

What is more open to debate is whether these mainly measurable factors have produced more accurate knowledge about the past, or whether, as some historians of archaeology have suggested, the nature of the scholarly record does not allow for cumulative progress to be made, or that archaeologists are unable to avoid impressing their own biases onto their work to the extent that their conclusions derive more from (or at worst, solely from) these, as opposed to anything related to tangible evidence (Trigger, 1989; 2006; Edgeworth, 2006). Whilst these are very valid concerns for philosophers of archaeological method, the discussion below will mainly be concerned with a narrower subset of this Whiggish narrative of archaeology (to borrow a term derived from Herbert Butterfield's *The Whig Interpretation of History*): the undue level of focus in narrative histories of archaeology given to recent scholars, who frequently rediscovered techniques which had first been used decades before, and the active attempts to denigrate these earlier scholars.

An especially prominent theme in this progressive narrative of archaeology is the role of the "hero-discoverer", or "founding father", of a field or a particular subdiscipline who, as the name implies, founded a new discipline, or, also frequently, put an old discipline back on track, rescued and/ or professionalised it. One recent example, on underwater archaeology, is given by Eric Cline (2017), in his history of archaeology, which very much

subscribes to the progressive narrative of the discipline just outlined. Cline introduces Bass as "the father of underwater archaeology" (2017: 158) and explains that, in 1959, "Bass went out and conducted the world's first underwater excavation, on what is now known as the Cape Gelidonya shipwreck. This is why he is referred to as the father of underwater archaeology" (2017: 158). The rest of the chapter describes Bass's later successful career, but entirely omits mention of previous underwater excavations, such as the exploration of the Mary Rose by the Deane brothers in the 1830s, the Alpine lake dwellings from the 1850s and the Lake Nemi Roman barges (to name only one of many shipwrecks now on land) in the 1920s. In two earlier books on underwater archaeology, Bass in fact only appears a quarter and halfway through the narratives respectively, and much space is devoted to earlier pioneers (Burgess, 2000; Blot, 1996).

Not infrequently, this process was started by – and indeed much of the historiography simply takes it at face value – the practitioners themselves, in their memoirs: a particularly problematic dimension of "insider" histories (Livingstone, 1992) which, like institutional histories, underplay the role of people with whom the individual did not associate. An example, of most relevance to this study and also discussed below, is a narrative created by the Egyptologist Flinders Petrie, who presented previous investigators in Egypt as treasure-hunters. In his widely-read autobiography, *Seventy Years in Archaeology*, Petrie wrote that, before his arrival in Egypt, "the science of observation, of registration, of recording, was as yet unthought of; nothing had a meaning unless it were an inscription or a sculpture" (Petrie, 1931). Other scholars have automatically inferred that Petrie's sweeping statement referred to antiquaries. Margaret Murray's *The Splendour of Egypt* was dedicated to "Flinders Petrie [her tutor] who out of the hobby of antiquarianism created the science of archaeology" (1949: ix). Other histories of Egyptology and commentators on the subject published since have also frequently accepted Petrie's statement without reservation (Cottrell, 1956: 180; Wilson, 1964; Mertz, 1964; Thompson, 1992a), despite considerable evidence to the contrary that Petrie's predecessors had very much been interested in observing stylistic differences and recording, beyond inscribed or aesthetically-pleasing material remains – in some ways more that Petrie himself – as discussed below.

In a provocatively-titled article "Everything you've been told about the history of Australian archaeology is wrong!", Matthew Spriggs (2020) gives a fascinating case study of the factors which together create the myth of a "founding" father – in this case John Mulvaney – in a relatively short time. He argues that the historiography of Australian archaeology became seriously distorted, to the extent that there came to be a widespread belief, within and outside the discipline, that Mulvaney's excavations at Fromm's Landing marked a watershed moment when "amateurs" gave way to "professionals", with the latter using modern techniques, such as radiocarbon dating, for the first time. However, as Spriggs reveals, in a series of short biographies of early 20th century Australian archaeologists, even if the "amateurs" did not have the benefit of radiocarbon dating, they nevertheless used many of the latest cultural ecology approaches, following their contemporaries in Britain. In the process, they addressed complex issues, such as coastal change, its influence on people and the reconstruction of historic landscapes. Mulvaney, who had lacked a geological background, dismissed these techniques out of hand and instead presented his predecessors as engaged in "indiscriminate collecting of skulls and stone artefacts" (2020: 1), casting their research as unscholarly and ethically dubious. Spriggs suggests that he had a strong ulterior motive: Australian archaeology had hitherto been dominated by museums, but, in the 1950s, universities were undergoing a phase of expansion. Mulvaney saw an opportunity to consolidate archaeology within them, and set about "an act of distancing his own practice from that of previous generations, an act that was strategic" (2020: 1), in his prolific writing. Mulvaney's narrative proved additionally attractive for subsequent archaeologists, who copied it, as it allowed them, for a time, to disown some of the worst racist remarks made by their predecessors, with little academic cost to themselves. If Spriggs's article has a drawback, it is that he adopts the negative associations that Mulvaney attached to the word "antiquary". With the mechanism of how the work of previous generations can be discredited for social as much as academic reasons, it is therefore time to examine the work of scholars who have taken a more positive outlook on antiquarianism from the outset.

2.2 Rehabilitated Antiquarianism: Stuart Piggott, Archaeology and Natural History

Stuart Piggott, more widely known for his successful academic career as an archaeologist specialising in prehistoric Britain, was one of the most important 20th-century scholars on the history of British antiquarianism. Over a half-century between the mid-1930s and late-1980s, Piggott published a number of essays, chapters and monographs on the subject, ranging from Tudor antiquaries in the 16th century – following on from Thomas Kendrick's work on the preceding centuries – to the early 19th century. Piggott was keen, especially in his later work, to emphasise that he followed a "history of ideas" approach, contextualising antiquaries and evaluating their work by the standards of their time, rather than simply comparing them to the conclusions then favoured by modern archaeologists and branding them correct or incorrect.

Piggott's initial work on antiquarianism took the form of two articles (1935; 1937), later expanded into a book-length biography (Piggott, 1950), of the 18th-century antiquary, William Stukeley. Throughout, Piggott contextualised Stukeley's work against a broad background encompassing religion and ideas about the picturesque. As noted above, Piggott frequently evaluated Stukeley's work according to whether his methodology was sound, as opposed to whether it was still used in the mid-20th century. For instance, Piggott praised Stukeley's calculation of a "Druid's cubit", on the grounds that it was worked out from fieldwork, and for a specific reason, to ascertain whether a stone circle was pre-Roman – despite the method being written off by more recent research. Similarly, Piggott usually distinguished between cases of Stukeley's assessment of the age of earthworks agreeing with modern estimates due to use of a scholarly method and those cases where his was a "lucky guess".

Despite his largely positive outlook, Piggott's approach inadvertently has flaws. In several cases, Piggott praised Stukeley's ability to accurately record field monuments, and went as far as suggesting that when these were no longer visible, the "onus" should be placed on archaeologists to prove Stukeley wrong, rather than vice versa (1950: 181). While technically positive, this nevertheless gives the impression that the more scholarly intellectual work only came later and that the role of antiquaries such as Stukeley was comparatively passive: Piggott explicitly summarised Stukeley as "not a good scholar" (1950: 181). A further issue is Piggott's division of Stukeley's life into two parts, separated

by a "psychological disturbance" (1950: 181). Initially, Stukeley was a sound scholar, later he became unable to resist imposing religious ideas on his work, and became sufficiently gullible to be tricked into helping validate a new Roman geographical itinerary, which had been forged by Charles Bertram, a Danish student. However, as Haycock (2002) has shown, Stukeley's interest in religious matters was of long standing, not a sudden development. Piggott's assessment appears to owe more to his not appreciating that Stukeley had an agenda of his own, and would not have viewed his other interests as examples of eccentricity at best, or irritating distractions and even symptoms of a mental health episode at worst. In the case of Bertram's forgery, Piggott connects Stukeley's mistake to David Douglas's (1951) thesis that there was a decline in scholarship in the 18th century, but much of his discussion points to a quite different interpretation: Bertram's forgery was clever, and was only debunked well into the 19th century. Effectively, possibly due to a perceived need to distance himself from any misinterpretation that he was endorsing a forgery himself, it is possible that Piggott was reluctant to institute a paradigm shift, made by later scholars, but came very close to making one.

Overall, Piggott's biography of Stukeley constitutes an important landmark in attempting to trace the history of ideas in their historical context, although one with problems. It is not sufficient to summarise these, as David Haycock (2002) has, as resulting from Piggott's crossing from archaeology into another discipline, history, of which he had inadequate knowledge. Rather, the issue is that, while he successfully avoided writing a Whiggish narrative of linear scholarly progress in the 18th century, Piggott's presentation of Stukeley as simply a recorder of physical remains, and an uncritical reader of documents, unable to progress further, ends up reinforcing a narrative that his own discipline, archaeology, developing linearly, from a stagnant antiquarianism.

Piggott's later work, much of it collated in *Ruins in a Landscape: Essays in Antiquarianism*, furthered many of the themes that he first advanced in his biography of Stukeley, but encompassed a much larger cast of several dozen antiquaries, from the 16th to the 18th centuries. A number of Piggott's contentions on the development, course, and achievements of antiquarianism, of relevance to this study, will be considered below.

One of Piggott's key arguments was that, in the 16th and 17th centuries, several antiquaries, among them William Camden, John Aubrey, Edward Lhuyd and Robert Plot, made a conscious effort to entirely jettison certain textual sources which were seen as unreliable, such as Geoffrey of Monmouth's *History of the Kings of Britain* (Piggott, 1976; 1989: 26). In their place, they substituted material culture as a source for investigating Britain's ancient history. This marked a major change, as objects such as stone tools, pottery, coins and Roman paving had previously largely been viewed, if they had been considered at all, as "irrelevant trivia", of little use for the study of the past (Piggott, 1989: 23; Piggott, 1976: 1-21). The result of this change in approach was that antiquaries were able to study the remote past from new angles and ask new research questions.

Rather than viewing the new approach, which Piggott referred to as the "New Antiquarianism", as the result of an internal development by antiquaries alone, Piggott forwarded the interesting argument that it emerged from a dialogue with the natural sciences, in particular with the evidence-based inductive reasoning propounded by Francis Bacon, which later became a key defining feature of the Royal Society (Piggott, 1976: 16-20, 101-102, 110-111; 1989, 24). In an era in which disciplinary boundaries had yet to solidify, such intellectual exchange was rendered easier; many scholars of the time, such as Robert Plot and Edward Lhuyd, gave more or less equal attention to natural history and the human past (Piggott, 1989: 26). In some research areas, it was even unclear whether a given object was natural or artificial so the divide was non-existent. A notable example of this was a debate over whether chipped flints were of anthropogenic origin; William Dugdale and Plot were in favour, but Martin Lister, John Ray and Lhuyd were undecided (Piggott, 1989: 88-94). The result was that these men, and antiquaries in general, frequently resorted to a methodology that had first been pioneered in the nascent natural sciences, especially those which were later to develop into geology and palaeontology, consisting of "objective observation, description and interpretation, especially in the form of classification and ordering" (Piggott, 1989: 24), much in the way that natural curiosities, such as fossils, were being treated.

In practice, John Aubrey embodied this new approach, an observation that was first made by his biographer, Michael Hunter (Hunter, 1975: 192). Aubrey's landmark project *Monumenta Britannica* (written in the 1670s, but unpublished during his lifetime)

distinguished between numerous monuments in the landscape, including "barrows, pottery, burials in general, linear earthworks, road and trackways, Roman pavements" amongst others (Piggott, 1976: 16). Aubrey also included medieval architecture in his remit, and studied it in relation to documentary sources; he succeeded in building up a chronological sequence of styles for medieval doors and windows, to show change over time: essentially a typology of the sort for which archaeology would later become famous (Piggott, 1976: 17, 110; Piggott, 1978: 38-39). In 1817, Thomas Rickman published his own scheme, along similar lines, for the development of Gothic architecture (Piggott, 1976: 118). Although Piggott saw the development of interest in the Medieval period as a slow process, at other times he noted that antiquaries worked with little institutional support or university funding. As such, intellectual advances were correspondingly more piecemeal than those from the 19th century onward – a theme Piggott was elsewhere keen to emphasise.

Although Piggott sought to highlight the independence of men such as Aubrey from texts, it is at once clear that much of the best work, such as that by Aubrey, sought to combine both types of evidence (texts and deductive reasoning from material culture), for mutual advantage and maximum insight. Aubrey's use of textual sources to develop his medieval typology was one such instance. Other examples included interpretation of earthworks: it was possible to assign a pre-Roman date to earthworks bisected by a Roman road, equally it was credible for Aubrey to argue that Britain's megaliths were pre-Roman on the grounds that they were common in the uplands of Britain and in Ireland, which had been either loosely or unincorporated into the Roman Empire (Piggott, 1989: 114-115). Lhuyd argued along similar lines against a Scandinavian date for megaliths (Piggott, 1989: 115-116). In a similar vein, Robert Hooke made an interesting contribution to thinking spatially when he "elaborated an analogy between fossil shells, as marks of the former extent of the sea, and Roman coins as marks of the extent of an ancient empire" (Piggott, 1989: 26). One of Piggott's notable strengths is his willingness to consider influences on antiquarianism from other nascent disciplines. Also particularly worthy to note is Piggott's insight that, if the antiquaries had yet to develop techniques of central importance to archaeology such as stratigraphy, then they were nevertheless thinking in an advanced way about what the distribution of antiquities could suggest

about ancient geography, a dimension that was slow to develop in late 19th and early 20th century archaeology, which overwhelmingly focused on reconstructing the history of individual sites (Koelsch, 2013).

If Piggott's discussion of the interactions between antiquaries and the natural sciences in the 16th to early 18th centuries is insightful, his belief that the two later became irrevocably divided, from the mid-18th until the late 19th century, is unconvincing. Accepting David Douglas's thesis that English scholarship of the Medieval period (and possibly beyond) stagnated during the 18th century, and noting that G. L. Davies (1968) came to a similar conclusion for the earth sciences, Piggott did not hesitate to fill in the gap and conclude that the same was true for antiquarian scholarship in the same period (Piggott, 1976; 1989). In a manner reminiscent of his division of Stukeley's life into two phases, the second being one of regression, Piggott suggested that a turning-point in antiquarian scholarship came in the 1740s. The scholarly tradition bifurcated into two schools: some antiquaries continued to be interested in material culture, whilst others became solely interested in historical manners, customs and cultural development at an entirely philosophical level, the so-called "conjectural history". In time, this tradition, having mainly grown up in Scotland and France, came to dominate – although Piggott appears to mostly cite philosophers and economists who had little connection to antiquarianism – to the exclusion of the more-empirical scholarship in the Royal Society tradition (Piggott, 1976: 151-155; 1989, 150-152). As Piggott's argument continues, by the time antiquaries, who now filled the ranks of the county archaeological societies, felt ready to re-engage with the natural sciences, notably geology, the latter had become too specialised and consolidated as an independent science to allow a reunion. The result was that it was only in the late 19th century, after a lengthy human prehistory had become accepted, that a reconciliation could occur (Piggott, 1976: 174; Piggott, 1989: 158-159).

However, as will become evident in the fifth chapter of this thesis, Piggott's assigning of a reconciliation to the late 19th century is decades too late, if a division even occurred at all. As Susan Pearce (2007), Noah Heringman (2013), Emily Sloan (2019) and Anna Roos (2021) have shown, there continued to be close integration, including a far larger number of individuals than Piggott was aware, and even a shared language that united the

nascent natural sciences/ natural philosophy with antiquarianism, throughout the 18th and early 19th centuries. Henry Englefield, Hayman Rooke and Martin Folkes were just three such figures of whom Piggott was apparently unaware. Also crucial to note is the geological research of the Oxford school of geologists, notably William Buckland, in which antiquities and geological research were often closely integrated (Rupke, 1983; Rudwick, 2005; 2008). To Piggott, an archaeologist of prehistoric Britain, the establishment of human antiquity by Boucher de Perthes, John Evans and others in the mid-19th century, was inevitably a pivotal moment, but, as has been discussed by Martin Rudwick (2008), as well as by Donald Grayson (1983) and Bowdoin Van Riper (1993), it was only one of many research questions in 19th century geology, and only became the most important one in the 1850s. As will also be seen, in all three empirical chapters on Wilkinson's research in Egypt and the Balkans, Piggott's assumption that there was an irrevocable divide between the study of material culture and conjectural history, is untenable.

2.3 Alain Schnapp and the problem of rehabilitated antiquarianism in global perspective

More recently than Piggott, Alain Schnapp has made antiquarianism a major focus of study (1996; 2002; 2013). In a series of publications, Schnapp makes a commendable effort to define antiquarianism as a discrete category, independent of and distinct from archaeology. In a similar vein to Piggott, he argues that whereas previous generations of scholars, in Britain, France, Italy and elsewhere, at one time relied mainly on classical texts, sometimes supplemented by oral tradition which had varying levels of reliability, antiquaries elevated objects above all other sources and used them as a "starting point" (1996: 163-164). This challenge of relying on primary texts is encapsulated in Schnapp's citation of Jacob Spon, a 17th century French doctor who studied Dalmatian and Levantine antiquities. Spon maintained that "our antiques are nothing if not books, whose pages of stone and marble were written with iron and chisel" (1996: 182). As will be seen below, this rethinking of what the concept of "text" encompassed was also a sentiment shared by early 19th-century antiquaries, including Wilkinson. The cognitive leap of viewing antiquities as books to be read could be rendered particularly unproblematic as some, such as coins and inscriptions, literally could be translated and read, like ancient texts.

Schnapp also points out that other antiquaries saw the potential to source information from images. In time, these three approaches became known as numismatics, epigraphy and iconography (1996: 180-186).

In addition to inscribed objects, Schnapp points out that several techniques, now widely assumed to be archaeological innovations, were actually used several centuries earlier, by antiquarian scholars. For instance, as pointed out by Hunter (1975) and Piggott (1976), John Aubrey's comparison of architectural styles resulted in classifications based on chronology, or more precisely, upon specific variables which changed in a predictable manner (Schnapp, 1996: 188-192) – a typological method. Schnapp notes that other antiquaries went beyond describing and comparing, to investigating the function and use of objects. At its best, the "analytical method… of the antiquary" (1996: 197) involved chronological and topographical studies into entire regions of England. Thomas Bowne situated a discussion of what he presumed to be a Roman cemetery at Walsingham within this framework. In several instances, Schnapp emphasises that antiquaries were not confined to libraries, but – especially when it came to the need for finding new material – travelled, and were closely engaged with the landscape and setting of monuments. Occasionally, they went as far as excavating; in Sweden, in the late 17th century, Olof Verelius even drew stratigraphic sections.

Notwithstanding these examples of antiquaries carrying out exemplary work with well-defined methods, Schnapp ultimately takes a highly critical view of their overall intellectual contribution, as one of his principal aims is to demonstrate "the long route by which the curious became first antiquaries, then archaeologists" (1996: 13). Effectively, Schnapp puts the antiquaries on the lower stage of an evolutionary model, with archaeology, defined by a "trinity of principles": "typology", "technology" (ethnographic comparison to establish the function of artefacts) and "stratigraphy", as the more advanced stage (1996: 37). Given Schnapp's otherwise farsighted caution that the history of archaeology did not progress in a linear manner and that the antiquaries had already pioneered each of these approaches, this evolutionary model need not necessarily be negative (1996: 34-36). Nevertheless, Schnapp declares that the application of stratigraphy was the step required "to rid archaeology of the dross of antiquarianism" (1996: 37). This in turn created "a rigorous and exact science" (1996: 37). Schnapp's firm

line between antiquarianism and archaeology continues in subsequent chapters. He defines the archaeologist as "objective" and detached, in contrast to the antiquary, who he sees as emotionally-attached, and concerned with "random collecting and aesthetic pleasure" (1996: 39-40, 308). In later chapters, on the institutionalisation of archaeology as an academic discipline, Schnapp goes as far as suggesting that archaeologists had to make a distinct effort to "free themselves from the world of collectors and artists" and "the antiquaries of the old school, who were more concerned with the form of objects than with their evolution" (1996: 304, 305, see also 313). When it comes to defining archaeology, Schnapp sets up antiquarianism as an academic (or anti-academic) "other", which archaeologists can learn little from, and are in danger of being ensnared by. Such an about-turn, in contrast to earlier chapters on the methodological successes of antiquarianism, is regrettable; notably, it fails to appreciate that attention to aesthetics and stylistics could – and did – inform scholarship on typology (Sweet, 2017a; Schnapp, 1996: 322), as will be discussed in the fourth chapter.

The arguments that Schnapp forwards about the transition from antiquarianism to archaeology in *The Discovery of the Past* are largely repeated, with some further development, in a short essay published in 2002. Despite Schnapp's call for university-based archaeologists to abandon ideas about their having an "exclusive monopoly over archaeology" (2002: 135), he is reluctant to refer to anything that might be called a history of archaeolog*ies*, when referring to interest in the past before the mid-19th century, holding that archaeology should only be traced back to the mid-19th century – instead, he uses "antiquarianism" as an all-embracing term for interest in past cultures. For pre-19th century Europe, this approach is not particularly problematic, given that many people did self-identify as antiquaries. In other cases, though, it brings problems. For example, Schnapp refers to a Prince Khaemois, a son of the Egyptian king Ramesses II in the 13th century BC, who unearthed an ancient statue and used it for religious-political ends, to revive an ancient cult, as "without doubt" an antiquary, but remains undecided as to whether he deserves to be called an archaeologist (2002: 135). Although there are key defining traits of archaeology present – material culture and excavation for information – Schnapp appears to prefer to back-project "antiquary" rather than "archaeologist" on the grounds that the latter term is better-suited when the case study

in question has an ideological tinge, as if such issues are only relevant in the case of antiquarianism – something that Margarita Diaz-Andreu (2007) has shown is definitely not the case.

Schnapp's essay brings the danger that if "antiquary" is used as a flexible term and applied to other cultures, its meaning will potentially be diluted. Thus, while Schnapp on one occasion notes that literacy was of crucial importance to antiquaries, he also notes that "the existence of literacy is not necessary (though admittedly useful) to the antiquarian enterprise" (2002: 136). This potentially undermines the definition of antiquarianism as a synthesis of text and material culture, previously championed by Piggott (1950; 1976; 1989) and Schnapp (1996). If the textual dimension is dropped or marginalised, there is a danger than definitions of antiquarianism will revert to being based around material culture/ objects alone, and then to collecting – one of the two common caricatures of the antiquaries, discussed at the start of this chapter, before the initial attempts at rehabilitation.

In his most recent major work, an introductory essay to *World Antiquarianism: Comparative Perspectives*, a major multidisciplinary survey of approaches toward the study and celebration of the past in diverse cultures from an array of periods, Schnapp (2013) takes a markedly more positive tone toward the antiquaries. After acknowledging that the term antiquarianism has a bad or condescending meaning and that it has come to be associated with traders in antiques or antiquities, or "at the very best, a specialist in what is old" (2013: 1), he argues that "we cannot limit the significance of the antiquaries' activities or the importance of their influence to their roles as collector, textual editor, and adviser to the rich and powerful" (2013: 2) – a sentiment which is echoed by the author of the following essay (Murray, 2013).

However, despite this refreshing start, the main objective of Schnapp's essay, and the book as a whole, is to take a global history perspective of antiquarianism, provincializing the European tradition. However, although this is a worthy aim, and the scholarship taken by itself is highly interesting, in order to expand the geographical and temporal limits of antiquarianism from a very specific term to a broad theme for narrating global history, much of Piggott's work (and that of Momigliano, discussed in the next section) into defining the pioneering features and methodological complexity of the antiquarian

methodology, is jettisoned. Rather than considering the possibility that antiquaries asked research questions, or that they pioneered a novel methodology that later archaeologists overlooked, the authors largely settle around a lowest common denominator when they give definitions of antiquarianism, focused around collecting. When Schnapp comes to answering a sub-heading titled "what is an antiquarian?", he immediately falls back simply on this very theme (2013: 3). Many of the authors adopt a similar definition, and also accept Schnapp's model of antiquaries evolving into archaeologists, with a continual danger of backsliding (Faulkenhausen, 2013; Wendrich, 2013; Rojas, 2013). In several cases, "archaeology" is used to refer to the serious scholarly part of projects, while "antiquarianism" is used to refer to ideological or frivolous dimensions of the same work or else projects related to "identity politics" (Beaulieu, 2013; Faulkenhausen, 2013). However, such issues also permeate archaeological scholarship (Kohl and Fawcett, 1996; Diaz-Andreu, 2007).

2.4 Rehabilitated Antiquarianism: the historical perspective

If scholars with an archaeological background have tended to focus on how antiquaries pioneered the use of material culture and in the process played down the extent to which antiquaries used texts, then historians have for the most part attempted to see antiquaries as pioneers of using material culture alongside textual sources, as opposed to purely seeing a triumph of the former over the latter. In addition, as opposed to projecting an idealised "antiquarian" or "antiquarianism" onto the past, those who have approached antiquarianism from a historical perspective have frequently been more willing to discuss a broad array of key figures who considered themselves to be antiquaries or at the very least moved in antiquarian circles such as the London Society of Antiquaries or the Spalding Gentleman's Society, and to consider the broader contexts in which they existed, as well as what their priorities were: a perspective that builds on Piggott's (1950; 1976; 1989) approach, as opposed to Schnapp's (1996; 2013) quite different one of attempting to reinvent antiquarianism as a theme for global history.

Given that a substantial literature exists on this topic, and that which is most relevant for the present study has been written since the turn of the 21st century, the following,

with the exception of a section on Arnaldo Momigliano, will focus exclusively on the most recent scholarship, as opposed to attempting to track how historians have used the term "antiquarianism" over the second half of the 20th century. First, it will discuss Arnaldo Momigliano's pioneering essay, *Ancient History and the Antiquarian* (1950), a work with relatively little content of relevance to this study, but which, in terms of approach, was very influential in setting the scene for nearly all of the subsequent scholarship to rehabilitate the reputation of antiquarianism. Second, it will fast-forward several decades, to discuss the beginnings of an extended episode of renewed interest in antiquarianism. This started with poststructuralist approaches and subsequently broadened to take a history of ideas perspective, at the time of the tercentenary of the Society of Antiquaries of London in 2007. Third, attention will zoom in on the accomplishments of antiquaries in the 18th and 19th centuries, considered against wider concerns and politics – as well as a debunking of erroneous but widespread notions about precisely who did antiquarian research – primarily focusing on the work of Rosemary Sweet (2001; 2004) and Rosemary Hill (2021).

Arnaldo Momigliano, an Italian classicist whose career spanned, like Piggott's, the mid and late-20th century, made an attempt to understand antiquarianism on its own terms, in an essay, *Ancient History and the Antiquarian*, published in 1950, the same year as Piggott's book on Stukeley – although over a decade after Piggott's earliest work. As noted, Momigliano approached the subject from the perspective of a historian rather than an archaeologist. He also mainly concentrated on Italian antiquaries and over a substantially longer timespan – which one recent commentator has noted was sufficient "to intimidate most of his readers" (Herklotz, 2007: 127). Given the wide-ranging nature of his work, his relevance to the present thesis is mostly important because he influenced how subsequent historians conceptualised antiquarianism, than it is for establishing a direct precedent for Wilkinson's approach.

In his essay, Momigliano (1950) argued that Renaissance-era antiquaries played a key role in refining historical scholarship. Such a claim was noteworthy, because, in the 20th century, as has been noted, the term "antiquary" had negative connotations, and was associated with either unscholarly matters of fashion, or, somewhat illogically, with excessive amounts of scholarly detail which were deemed irrelevant to the political

history of the time. Although Momigliano did not use the term "antiquary" as a denigration, he nevertheless distinguished between two main scholarly traditions which he saw as extant between classical antiquity and the end of the early modern period. The more prestigious of these was political history, whereas antiquarianism – which gave attention to recording "manners and customs", including daily life and institutions, including laws, thus providing information on facets of daily life in antiquity which were simply too obvious for historians to mention – was its less well-regarded partner. The division was not an entirely intuitive one, and Momigliano blamed Thucydides, the ancient Greek historian of the twenty-seven-year Peloponnesian War (431-404BC), for establishing it, and subsequent scholars, such as the Roman historian Varro, who took Thucydides as a model, for perpetuating it. However, a change came in the 16th century: Momigliano saw a decline in the fortunes of the historians after they were seen to have become embroiled in political and religious disputes, which led to the widespread view that their writings were tainted by ideology and partisanship. For some "Pyrrhonists" such as Père Hardouin, literary sources in general became seen as unreliable.

However, whereas historians who relied on textual sources were deemed to have failed, Momigliano saw the reputation of the other group – the antiquaries – as having enjoyed a corresponding rise. Since antiquaries distinguished between original and derivative sources, and because they eschewed grand narrative or chronology in favour of the vernacular and smaller-scale, their work was seen as less ideologically-loaded. Their sources – material remains such as coins, statues and inscriptions – were seen as a quite different form of evidence which were deemed too numerous to be forgeries. The end result was that the work of the antiquaries was seen to be the antidote to a spreading historical Pyrrhonism, in the sense that reliable sources for writing history existed after all. Eventually a reconciliation occurred, and historians began to incorporate material culture: Gibbon embodied this approach. Essentially, Momigliano explained the passing of antiquarianism as a highly positive development, as it gave rise to a more encompassing and critical historical scholarship.

Given that Momigliano propounded a progressive evolutionary narrative and saw the early antiquaries as laying the foundations for the development of subsequent historical techniques, his perspective has been criticised as taking a "Whig interpretation of

antiquarianism", by Marcia Lolla (1999: 15), who lifts the phrase from Herbert Butterfield's attack on Macaulay's progressive vision of modern British history, *The Whig Interpretation of History* (1931). Lolla's concern is shared by Stephen Bann, the author of the preface of the volume in which Lolla's essay appears, *Producing the Past: Aspects of Antiquarian Culture and Practice, 1700-1850*. Bann notes that "Momigliano deserves the ambivalent credit of having taken the antiquaries seriously, and having at the same time determined the serious part of their enterprise to be a flattering mirror-image of the profile of the twentieth-century historian" (1999: xviii), or, to borrow a then-notorious political phrase, whether a previous scholar under consideration, such as William Camden, could be deemed "one of us" by historians (1999: xviii). In other words, Momigliano's fault is the same as Piggott's: both attempted to selectively annex choice parts of antiquarianism to their respective disciplines. Concern about Momigliano back-projecting modern concerns is also made by Ingo Herklotz (2007), who has raised several questions about Momigliano's claims, including whether the boundaries between antiquarianism and history, especially in the predisciplinary era, were as well-defined as Momigliano believed, or whether the antiquaries were indeed apolitical, and whether historians genuinely saw them as a model. Nevertheless, Herklotz's end verdict on the antiquaries – and the tone of the edited book in which his essay features – is highly positive and presents the half-century after Momigliano's essay as a renaissance for antiquarianism, given the questioning of the primacy of political history by the social history of the *Annales* school, as well as the move toward topical, as opposed to chronological, arrangements in modern museums.

One notable development from the historical reassessment of antiquarianism worth mentioning is that of a more inclusive definition than those given by archaeologists, who tend to emphasise material culture. In the same volume as Lolla and Bann, Lucy Peltz and Martin Myrone suggest that the hallmarks of an antiquarian approach were "their preference for systematic or structural analysis over chronological narrative, their investment of equal value in both visual and textual evidence, their insistent perhaps obsessive concern with details" (1999: 5). Peltz's and Myrone's highlighting of "systematic or structural arrangement" over "chronological narrative" owes something to Momigliano, whom they acknowledge. However, in viewing antiquaries as treating texts

and visual sources (whether architectural monuments or small portable artefacts) as truly complementary, their definition goes further, as it does compared to that posited by Schnapp and the authors of *World Antiquarianism*, who emphasise the primacy of material culture. Notably absent from their definition is the suggestion, made by several of the authors of *World Antiquarianism*, is that antiquaries are merely collectors.

Peltz's and Myrone's definition is very much borne out by a consideration of contemporaries who thought of themselves as antiquaries. This will be expanded considerably later on, but for the present time, two examples will suffice to show that even antiquaries renowned primarily for dealing with the material world nevertheless put great emphasis on texts. In his revisionist study of Stukeley, Haycock (2002) has shown that, far from Stukeley's career being divided into two phases, with archaeological fieldwork giving way to religious fundamentalism, Stukeley was inspired by perceived links between primitive Christianity and prehistoric Britain to undertake his fieldwork in the first place. As Lolla (2003) has shown, Johann Winckelmann, a German antiquary renowned for his interpretation of classical art, from Greek vases to sculpture, actually repeatedly subordinated classical art to textual interpretations. As Lolla (2003) has also shown, the reverse could be true as well: when the Society of Antiquaries undertook an inquiry as to whether it would be best to use printing or engraving (a much more expensive option) to create a facsimile of Domesday Book – a textual source running to hundreds of pages comprising land ownership during the reign of William I – the preference of the antiquaries was very much in favour of engraving the pages, as this would capture the full range of palaeographic variation in a way that no printed script could ever do. In other words, Domesday Book, which was seen by many simply as a text, was treated by the antiquaries as a material artefact in which the visual mattered just as much.

The Society of Antiquaries of London itself has been the focus of several scholars, in two multi-authored books, *Visions of Antiquity: The Society of Antiquaries of London 1707-2007* and *Making History: Antiquaries in Britain 1707-2007*, both published on the occasion of the society's tercentenary in 2007. Although space does not permit all aspects of these to be reviewed, a notable theme throughout both is the close congruence between antiquarianism and archaeology. As Susan Pearce notes, those

antiquaries who did collect objects or at least paid attention to collections of them – as will be discussed below the two by no means necessarily went together – frequently became sufficiently aware of differences to the extent that it was possible for them to construct taxonomic classification, via "a number of key, but changing, characteristics of form across a group of broadly similar objects", the arrangement of which allowed them "to construct a chronological scheme of growth, mortality and decay" (Pearce, 2007: 158). In his introduction to *Making History*, David Starkey (2007) goes further and claims that Fellows of the Society of Antiquaries did not just use now-crucial archaeological techniques as one-offs, but that the development of archaeology was driven by Fellows of the Society of Antiquaries. As he puts it, "the society was the natural pioneer of archaeology and played the key role in its perfection. Its Fellows turned excavation from random treasure-hunting to systematic exploration and recording" (2007: 13). Starkey also credits Fellows of the Society for the development of stratification and the discovery of prehistory.

Whilst there is a commendatory dimension to both books, both also feature less laudatory voices as well. In particular, Stephen Briggs (2007) has questioned whether it is really valid to see the Society of Antiquaries as a pioneer when it comes to developing archaeological techniques, especially those of relevance for investigating prehistory. He notes that, although the Society offered a stage for two of its Fellows, John Evans and John Lubbock, to make the case for a lengthy human prehistory as a result of new evidence uncovered in the late 1850s, it did not itself sponsor such research. Briggs also notes that many Fellows were reluctant to accept the new consensus and that the relationship between the society and the leading scholars who investigated the issue was extremely fleeting: they were much more associated with other societies such as the Geological and Ethnological Societies. Such issues are definitely worth bearing in mind, though even these potentially have qualifications. As Nathan Schlanger (2011) has shown, even if the Society of Antiquaries was not pivotal, antiquarian approaches could still be: Sir John Evans's numismatic research gave him a ready-made model and cognitive entry into classifying prehistoric stone tools. In other words, although the situation was somewhat more complex than that outlined by Starkey, antiquarianism more broadly can claim to have had an influence on the development of modern archaeology.

Inevitably other historians besides Starkey can be classed as potentially reading present-day concerns onto the past: antiquarian methodology has been viewed as foreshadowing poststructuralism by (Peltz and Myrone, 1999), while Ingo Herklotz's (2007) aforementioned contention that antiquarianism has made a comeback emphasises some aspects of antiquarianism above others: genealogy, a very important but frequently-overlooked aspect of the antiquarian endeavour, is once again popular, but it cannot be said to have converted many academics.

One of the most important historical contributions to rehabilitating the reputation of antiquarianism, and the most important of the early twenty-first century, is Rosemary Sweet's *Antiquaries: The Discovery of the Past in Eighteenth-Century Britain* (2004). As the title suggests, Sweet's concern is not simply in reinterpreting a single facet of antiquarianism: her book provides a reappraisal of antiquarianism for a century which previous scholars of antiquarianism saw as a period of decline. In doing so, Sweet revises a number of Piggott's contentions, and brings a fresh perspective toward considering how antiquarianism relates both to archaeology and history.

For Piggott, British antiquarianism was predominantly a scholarly phenomenon that flourished between the Reformation and the early 18th century. Piggott saw a decline in the decades after the 1720s: Stukeley impressed religious biases on his fieldwork and fell for Bertram's forgery, as did other antiquaries (Piggott, 1950). As well as a marked decline in the quality of research into prehistoric and Roman Britain, much of it an inference from the career of Stukeley, Piggott, following David Douglas, saw a corresponding decline in research into the Anglo-Saxon and Medieval periods, with over a century passing between the typological systems of Aubrey and Rickman. For Piggott, Richard Gough's revision of Camden's pioneering *Britannia* stood in for the quality of antiquarian scholarship at the time: it was a "failure" (1976) which paid inadequate attention to pioneering county histories.

Sweet's conclusions on the quality of antiquarian scholarship in the 18th century are considerably more positive than Piggott's. In an earlier pioneering article on Gough's contribution to late 18th century antiquarian scholarship, Sweet (2001) notes that for the Medieval period, much of the most important work appeared in larger works on county history, as opposed to standalone specifically antiquarian volumes, or in the Society of

Antiquary's journal, *Archaeologia*. In other words, she addresses Piggott's glaring contradiction that antiquarian scholarship in general, symbolised by Gough's incomplete work, was at a low ebb because it did not take into account other antiquarian work published in other outlets. Sweet suggests that the 18th century in general was a time of great progress in the quality of county histories. Whereas these had formerly focused mainly on heraldry and genealogy – to bolster the confidence of local elites – they increasingly incorporated information relevant beyond elites, on topics such as the local economy, natural history, manners and customs, and antiquities in the region – and Gough urged authors to do this (Sweet, 2001; 2004). For antiquarian research into the Roman period, Sweet does not hold back – as Piggott appears to do so – from a wholescale reinterpretation of the Bertram forgery: she notes that Bertram's manuscript provided over a hundred place-names for areas of Britain which were not known in other textual sources, yet which were yielding material traces of a Roman presence, whilst it did not greatly disrupt the existing framework and consensus (Sweet, 2004). Additionally, other Medieval manuscripts purported to be faithful copies of Roman ones and once they were translated and distributed, the end product was a mediated one in all cases. In other words, far from Stukeley and his contemporaries falling for an obvious forgery, Bertram "simply provided the antiquaries with what they wanted to hear, and in a format with which they were familiar" (2004: 177). Considering other episodes of historians being hoaxed in more recent times, such as the leading historian Hugh Trevor-Roper's mistaken validation of the fabricated Hitler diaries, and the extended saga of controversy surrounding the Vinland map (Monmonier, 1995), it certainly seems reductive for Piggott to allow a clever forgery to stand in for a whole scholarly tradition.

That Bertram's forgery was not representative of scholarship on Roman Britain is demonstrated by Sweet's (2004) extensive discussion of numerous other antiquaries who investigated an array of dimensions relating to the Roman occupation. In many ways, the whole of the 18th century was a golden era for Romano-British studies, and the period was consistently one of the most popular for antiquarian research, not least because Georgian Britons could easily read aspects of their own society onto the Romans. Alexander Gordon and John Horsley were two leading antiquaries of the early-mid 18th century, who wrote major works: *Itinerarium septentrionale* (1726) and *Britannia romana*

(1734) respectively. Of the two, Horsley's work was particularly impressive: he was able to build on Gordon's work due to his own extensive fieldwork. He assumed that inscriptions had rarely been moved from where they had originally been set up, so, unlike coins, they were a useful source for matching place-names known from classical sources with remains in towns or the countryside that had tentatively been associated with the Romans, which was a major area of interest. However, Sweet notes that Horsley's overall aim "was to trace out the chronology and geography of ancient monuments in order to confirm and illustrate the literature of ancient history" (2004: 170): it was an endeavour that was subordinate to texts, the discussion of which formed a large part of Horsley's work and as such took a quite different approach to archaeological approaches from the late 19th century onwards which prized excavation, to reconstruct histories of the occupation of particular sites. This emphasis on texts was a dimension that shed light on why Bertram's forgery was eagerly accepted by many (though by no means all): Scottish and Cornish antiquaries had found Roman artefacts and military camps in areas where existing texts did not reach, so Bertram's manuscript allowed these to be incorporated into an existing text-centric knowledge system.

As the 18th century progressed and more Roman remains were uncovered, it came to be appreciated that conclusions could be drawn from material culture alone. A major area of research was tracing the route of Roman roads: Francis Drake discovered several stretches running through and radiating from York, which reinforced the attestation of classical-era authors that the city was of great political importance. In 1769, Kennet Gibson achieved something similar for his home town of Castor, Northamptonshire, which became identified with Roman-era Durobrivis. A late 18th century development, and one which was very much driven by newly-unearthed artefacts (invariably from building work or agricultural improvement as opposed to excavation planned in advance), was the realisation that the Roman occupation had not simply been military: there were civil remains to be found, or a "domesticated Roman presence" (2004: 182). Partly driven by finds from Pompeii and Herculaneum which encouraged antiquaries to revise their conclusions about previously-known remains on the one hand, and partly by entirely new finds such as the remains of temples and a bathing complex in Bath on the other, antiquaries such as Thomas Reynolds, writing in 1799, turned away from the more

traditional focus on military antiquities such as roads and inscriptions, and took a more holistic approach toward encompassing the civilian dimensions of Roman Britain.

Another key area in which Sweet (2004: 250-254) provides correction to Piggott is in the development of typology and the ascertaining of age. Whereas Piggott assumed that the century and a half between Aubrey and Rickman was a vacuum in terms of progress in using stylistics for dating, Sweet (2004) shows that there was notable work in this area throughout the 18th century. Most notably, Charles Lyttelton was inspired by Aubrey's unpublished manuscript, *Chronologica Architectonica*, to try and distinguish Saxon from Norman and Gothic architecture. Though Lyttelton made assumptions about what Saxon architecture should have looked like from thinking about supposed Saxon characteristics, and also judged it inferior to classical architecture, he succeeded in devising a workable typological system that allowed him to date many buildings, and even specific building phases, without using texts, an approach that met with disbelief from other antiquaries: one, George North, assumed that archival research was the only valid approach. Although Lyttleton unfortunately did not publish a guide, sufficient correspondence exists to suggest that his ideas circulated among other antiquaries, including William Borlase, who attested to being able to see Medieval buildings in a totally new way. Lyttelton was not the only one who became interested in the minutiae of Medieval architecture: Horace Walpole had been one of the first to do so, while Lyttelton shared his interest in distinguishing the Saxon from the Norman with Smart Lethieullier. Such work also had wider implications. As Sweet notes in a subsequent essay, once specific styles were defined, "later additions and restorations could be correctly identified – and found offensive" (2017: 66). The culmination of this phenomenon were John Carter's and Richard Gough's championing the preservation of the fabric of several cathedrals, from James Wyatt's work which, though well-meaning, wrought considerable damage (Sweet, 2004: 288-301).

A last notable area in which Sweet's (2004) work corrects Piggott's research is in the realm of conjectural or philosophical history. Whilst Piggott (1976; 1989) envisaged an unbridgeable divide opening between antiquaries interested in material culture and those interested in theoretical models of human development, with Thomas Pownall, who was especially active in the 1770s, among the very few to bridge it, Sweet has shown

that antiquaries frequently considered the two together. Meyrick and Smith's *Costume of the Original Inhabitants of the British Islands* (1821) was one such endeavour for the earliest years of British history. Amongst other antiquaries, there was debate as to how civilized the Anglo-Saxons had been, and this drew on material culture: the presence of coins (which became accepted) and the presence of stone as opposed to wooden architecture (which did not) were two notable areas of debate (Sweet, 2004: 189-230). As well as antiquaries being inspired by such models, theorists such as Lord Kames and John Millar, frequently drew on raw material supplied by antiquaries, and this was especially true for the Anglo-Saxon period. In other words, for many, material culture and theories of human development went together to shed light on past cultures and were not at odds as Piggott (who occasionally appears to have read the heated debate over processual archaeology onto the past) thought. Sweet's argument can also be taken further: although generally not labelled as such, Winckelmann's ideas about the progress and regress of art, which became widely taken-up, was also a conjectural model of sorts and one which was firmly based on material sources, even if he occasionally attributed Greek sculpture to the Roman period and vice versa (Lolla, 2003).

The most recent attempt to rehabilitate the reputation of antiquarianism is Rosemary Hill's *Time's Witness: History in the Age of Romanticism* (2021). In some regards, Hill continues the narrative where Sweet leaves it: she considers the period between the French Revolution and the 1850s, a time when use of the term "antiquarian" may well have been at its most popular (Levine, 1986; Evans, 2007: 300; Gamble, 2015: 149-150). Hill also shares several of Sweet's background assumptions about the nature of antiquarianism, including her refusal to use "antiquary" and "collector" as synonyms and her insistence that the remit of the antiquaries covered both textual and material sources – with the latter including images, artefacts and monuments.

Two cases in which this worked particularly well were in the study of the Bayeux Tapestry and of Medieval manners and customs. For the Bayeux Tapestry, two British antiquaries, Hudson Gurney and Charles Stothard, noticed that the finer details of the embroidery could reveal clues as to when it had been produced. Assuming that the medieval people who had produced the image would have shown dress and architecture that they were most familiar with, they concluded that the embroidery had been made

soon after the Norman conquest because of the presence of Norman architecture, and the absence of the pointed arch, and other features which were associated with later centuries, such as heraldry. To La Rue, the leading authority on the embroidery at the time, this was a new approach: he was a historian and assumed that an absence of references to the Bayeux Tapestry in contemporary records was sufficient evidence to assign it to a later period; he also dismissed architecture and dress history as insufficiently serious sources.[5] As well as using such source triangulation for dating, Charles Stothard, and his wife Eliza, sought to gain information from the images too. As Hill notes, "it was clearly a narrative and the Romantic generation were predisposed to read it as a Romantic narrative, assuming that it dealt with events in terms of the motives and experience of the individuals" (2021: 122). From this perspective, they concluded that the climax of the Battle of Hastings featured multiple depictions of the last moments of King Harold, unfolding like a modern graphic novel. At roughly the same time, Joseph Strutt realised that, since contemporaries would have portrayed people in dress typical of their time, illuminated manuscripts, effigies and wall paintings could be combined with written sources such as chronicles and poems, to build up a picture of medieval dress; he extended his interest to sports and pastimes, and his research was sufficiently well-received so that other antiquaries, including Joseph Strutt and Samuel Rush Meyrick, followed him.

As well as straightforward scholarly concerns, Hill's narrative history devotes more space to romanticism and the "lived relationship" to history that many antiquaries literally dwelt in, including Walter Scott, who wrote highly successful recreations of medieval and post-medieval atmospheres. One notable area in which scholarly and popular antiquarianism came together was the growing cult interest in Shakespeare. The basic details of the playwright's life were known, but antiquaries, such as Robert Bell Wheler, were able to fill in many of the details and add colour, through studying historical documents, and relics which survived in Stratford-upon-Avon church: from this they uncovered more biographical details, and, to their satisfaction, a likeness of the man and his birthplace. Although such activity, as well as that of Scott, could lead to uncertainty as

[5] The difference in opinions was not simply English vs French: La Rue ignored the approach of Montfaucon, a French antiquary who had been active in the mid-18th century, who took an approach similar to that which the Britons effectively rediscovered (Haskell, 1993).

to how much of the past was being reinvented, Hill notes that Scott drew the line at the Sobieski Stuarts' production of a fabricated document on Highland Clan tartan which nevertheless became extremely popular.

Lastly, an area that both Sweet (2004) and Hill (2021) have devoted attention to is the identity of those who considered themselves to be antiquaries. Whilst there has frequently existed an assumption that all antiquaries (especially in their popular, but erroneous guise as collectors) were members of the aristocracy or the wealthiest sections of society – it is forwarded by several of the authors of *World Antiquarianism: Comparative Perspectives* – Sweet's and Hill's investigations of the individuals responsible for antiquarian publications in the 18th and 19th centuries suggest that the reality was more complex. At one level, Sweet notes that, in the 18th century in particular, an interest in antiquarian subjects was dependent on education and leisure time, both of which were the preserve of gentlemen and the wealthier sections of society. However, authors were rarely titled: instead, they tended to be members of the professional classes, minor gentry, or clergymen: the latter formed a quarter of the Society of Antiquaries and even higher proportions of provincial antiquarian societies. Hill's study suggests that the situation in the early 19th century was broadly similar: she even goes as far as to suggest, based on the large number of Catholic antiquaries – who were subject to widespread discrimination as a result a patriotism based in part on anti-Catholicism (Colley, 1992) – that "a remarkable number of the antiquaries were socially marginal" (2021: 38). Although Hill's reference must be considered in terms of the relationship of antiquaries to the wealthiest sections of society as opposed to the wider population, there is some limited evidence that their readers could genuinely belong to some of the more marginal classes: Sweet notes that John Cannon, an excise man, was able to access antiquarian publications through relatively cheap serial publications.

Alongside the representation of an array of different social classes, Sweet (2004) has also shown that antiquaries did not hold uniform political beliefs. While an interest in old objects and places, especially Medieval ones, did unleash feelings of nostalgia and a yearning for an old order for some Tories, this was by no means exclusively the case. Some antiquaries took a more radical approach: in legal antiquarianism, men such as Thomas Oldfield, in his *History of the Boroughs* (1792) attempted to make the case for a

broader franchise based on old charters, while John Blackner, a radical newspaper proprietor who published a history of Nottingham in 1815, presented the Medieval Gothic as a monument to English labour. Other scholars have arrived at similar conclusions for other types of antiquarian material: Stephen Daniels (2017) has commented on how John Britton's map of Marylebone, which featured vignettes of antiquarian subjects, including churches, carried a political message which suggested the area was a mature and coherent economic unit and which deserved parliamentary representation, which it had achieved in 1832. In a somewhat less radical vein, Andrew Kennedy (2003) notes that Samuel and Nathaniel Buck frequently produced views which juxtaposed ancient ruins with modern estate landscapes, giving an impression of improvement, while Dale Townshend (2017) notes that ruined castles and medieval abbeys were seen by some as symbolising the triumph of a Protestant constitutional monarchy over absolute monarchism and Catholicism.

Overall, Sweet and Hill make a convincing counter-argument to Piggott's contention that "antiquarianism" was a redundant term by the early 18th century. Instead, it remained relevant and was used as a term by scholars who carried out academic research up until at least the 1850s, well into Wilkinson's lifetime. Inevitably, neither Sweet's nor Hill's studies are comprehensive. Sweet, although she considers the Egyptian Society of London briefly, confines her study to the British Isles, and predominantly to England and Scotland. Whilst Hill's account contains an extremely interesting account of British antiquaries studying and expropriating medieval antiquities from northern France, she gives very limited coverage to areas any further afield. Whereas in geographical remit both therefore follow Piggott, neither develop Piggott's (1950; 1976; 1989) contention that antiquarianism borrowed a methodology from the earth sciences to any great extent. Sweet notes that this was the case in the Reformation, but makes only a very limited number of references to it for the 18th century, while Hill only notes that Rickman and Meyrick were influenced by typology used in natural history to develop their ideas about Medieval architecture and dress respectively, without further elaboration. To some extent, both of these themes are addressed by Noah Heringman's (2013) *Sciences of Antiquity*: Heringman discusses how antiquarian themes featured in early geological and natural history research in Italy and the south Pacific. In a shorter study, C. J. Wright

(2011) discusses how, under the direction of Richard Gough, the Society of Antiquaries briefly took an interest in Indian antiquities, before the Royal Asiatic Society became the main forum for such discussion.

2.5 Antiquarianism, Egyptology and John Gardner Wilkinson

The first full-length biography of Wilkinson, by Jason Thompson, was published in 1992. Besides giving a detailed overview of Wilkinson's life – at a standard difficult to better in its broad outlines, and which the biographical sketch at the start of this thesis relied upon heavily – Thompson sought to place Wilkinson in his broader intellectual context. One positive outcome of this approach resulted in attention being given to Wilkinson's colleagues in Egypt, among them Robert Hay, Edward William Lane, James Burton, Joseph Bonomi and Henry Salt. Few of these men – who had not made major discoveries like their predecessors such as Giovanni Belzoni or distant successors such as Howard Carter – had previously featured in histories of archaeology; while C.W. Ceram's widely-read history of archaeology, *Gods, Graves and Scholars* (1952), ignored them altogether in favour of a concentration on the scramble to collect antiquities and the race to decipher hieroglyphs. Even many histories of Egyptology omitted them, as the 1820s was seen as the decade of decipherment, following Champollion's "Lettre *à* M. Dacier" (1822), although in the years preceding Thompson's biography, Victor Wolfgang von Hagen (1968), Leila Ahmed (1978) and Selwyn Tillett (1984) published biographies of several of Wilkinson's colleagues, Frederick Catherwood, Edward William Lane and Robert Hay respectively. Overall, by discussing their work on a wide range of topics – the decipherment of hieroglyphs, the establishment of chronology, the understanding of now well-known episodes in Egyptian history such as the Amarna heresy and the method of pyramid construction – Thompson's contention that "for a time [the 1820s and early 1830s] Wilkinson and his friends were in the very forefront of Egyptology" (1992: 82) is convincing, and his restoration of a lost circle of British students of Egyptology to prominence is a crucially important scholarly contribution.

In addition to the above considerations, Thompson's biography of Wilkinson was a study in the history of ideas, or intellectual history, in which he sought to clarify the

complicated matter of Wilkinson's identity. Especially prominent is the question of whether he was an "Egyptologist". This was a term which previous writers, including the authors of earlier entries for Wilkinson in the *Dictionary of National Biography* (Margoliouth, 1900) and *Who Was Who in Egyptology* (Dawson, 1951) had little problem in bestowing on Wilkinson. Later scholars have also done so: Briggs (2008a) refers to Wilkinson as "an Egyptologist abroad" in his study of Wilkinson's work on megalithic remains in Wales while the revised entry for Wilkinson in the *Dictionary of National Biography* also uses the label (Thompson, 2004). However, in his biography of Wilkinson, which contains more in-depth discussion of how Wilkinson saw himself in relation to wider society and scholarship, Thompson is more ambivalent and notes that whilst Wilkinson has been termed "the founder of Egyptology in Great Britain", this "attribution would have surprised Wilkinson", because the word "Egyptology" had only been coined towards the end of Wilkinson's life, in the second half of the 19th century (1992: xi).

Instead of "Egyptologist", Thompson suggests that Wilkinson saw himself as an "antiquary" or a "traveller". Both terms were used by Wilkinson in letters and many of his contemporaries used them too: Thomas Young bestowed the accolade on Wilkinson when the latter was beginning to be seen as making a scholarly contribution. Other positive examples of the term "antiquary" can be found in Wilkinson's books such as his *Topography of Thebes, and General View of Egypt* (1835). Despite this, Thompson narrowly dismisses the term, noting that "if one defines an antiquarian as someone who is solely interested in facts about the past and who does not selectively arrange those facts and interpret them to answer questions about the past, then Wilkinson was much more than an antiquary" (1992: xvii). In this, he followed the judgement of Selwyn Tillett, who classed Robert Hay and his colleagues, Wilkinson among them, as "something more than antiquarians" (1984: 3). Later on, Thompson implies that, when middle-aged, Wilkinson underwent a period of statis, which he ascribes to his antiquarian outlook. As should be evident to the current reader, Thompson's view of the remit of antiquarian activity closely resembles the reductive remarks frequently made by scholars during much of the 20th century, and, bar a reference to Levine's *The Amateur and the Professional* (1986) – one of the first works by a historian to present the antiquaries in a more sympathetic light – he was writing prior to the reassessment of the term, which

predominantly took place during the fifteen years following the publication of his book. Had Thompson been writing later, it is not impossible that he would have taken on a more positive tone in this regard. Though in the first of his three-volume *Wonderful Things: A History of Egyptology* (2015), published two decades later, Thompson does not revive the term (nor does he discuss Levine), his usage of the term is neutral as opposed to loaded. Before turning to consider how useful the concept of rehabilitated antiquarianism is in Wilkinson's case, it is necessary to give brief consideration to the other term that has proven popular, of "Egyptologist".

The question of whether Wilkinson should be regarded as an Egyptologist is a complex one, because the definition of "Egyptologist" has changed over time, and, above all, because the term appears to have been coined towards the end of Wilkinson's life: as noted by Thompson (1992a), it is far from clear whether he used the precise term "Egyptology" or "Egyptologist" (but see below). For much of the 20th century, the term "Egyptology" has been synonymous with the study of ancient Egyptian linguistics, or hieroglyphs (James, 1997; Thompson, 2015). At the level of whether Wilkinson was an Egyptologist based on the philological dimension, or language qualification, there is a relatively straightforward answer: Wilkinson was indeed skilled in hieroglyphs, and Thompson (1992) has suggested that, for a brief time, in the early 1830s, immediately after the death of Thomas Young and François Champollion, Wilkinson was the most skilled scholar of hieroglyphs in the world – although at the same time it must be admitted that interest in the subject was muted and he was soon superseded by others, most notably the Prussian scholar Karl Lepsius; nor should Ippolito Rosellini be forgotten.

More recently, it has become commonplace to use the term "Egyptologist" (and "Egyptology") in a more democratic sense, to refer to anyone, past or present, who has made a contribution to the study of ancient Egyptian civilization, regardless of whether they possessed relevant philological knowledge. In part, this revision addresses the issue of somewhat unhelpful academic hierarchies in modern-day scholarship which present epigraphic research as higher than research into material culture, especially of the earliest phases of Egyptian history and prehistory. It also addresses the potentially problematic issue, pointed out by Thompson (2015: 3) that the most renowned investigator of ancient Egyptian civilization, Howard Carter, was not, under the linguistic-

based definition, an Egyptologist: he was an artist or copyist. As research into ancient Egyptian civilization has taken into account a much broader array of approaches, including reading Egyptian relief scenes like graphic narratives and reconstructing ancient landscapes through geomorphological and remote sensing research, a broader definition of "Egyptology" has, to some extent, become a necessity (Baines, 2020). The expanded definition of "Egyptology" has also been beneficial in that it has meant that the contributions of those without academic qualifications, but who still nevertheless made a contribution, can be considered.

In some regards, the lowering of the qualification threshold was started by Warren Dawson in the first edition of *Who Was Who in Egyptology* (1951): alongside scholars, Dawson included entries of antique-collecting businessmen who happened to reside in Cairo. Importantly for present-day considerations, Dawson also pushed back the temporal threshold to as long ago as the early 18th century. Subsequent editions of the *WWW* have both pushed back the temporal limit by two additional centuries, to 1500, and have continued to take a more inclusive approach to "who counts": this has resulted in a substantial increase in the number of entries, especially of women, many of the earlier ones being wives or daughters of male scholars. The considerably enlarged later editions (e.g. Bierbrier, 2019) also reflect a growing willingness amongst Egyptologists to reconsider the divide between the professional and the amateur and bestow recognition on the latter, and the rapid rise in the number of professional Egyptologists who attained employment during the post war expansion of the university sector, especially in Europe and North America. Other histories of Egyptology, such as Thompson's three-volume study, have taken a similar approach: one third of the first volume is devoted to the years before 1800, and the narrative extends back to the work of Khaemwaset, a son of Ramesses II, who was active in the 13th century BC. Overall, there has not been a move toward speaking of "Egyptologies" in the manner that practitioners of other disciplines routinely refer to "histories", "geographies" and so forth (e.g. Carruthers, 2015; Ogborn and Withers, 2004) to describe the dimension of not everyone following the priorities set by museum and university-based professionals (and that these traditions are not homogeneous). However, there is an increasing sense that the history of Egyptology is not synonymous with the work of a handful of researchers at a small number of mainly

western universities. Perhaps the most important research work in recent years in this regard is Okasha el Daly's *Egyptology: The Missing Millennium: Ancient Egypt in Medieval Arabic Writings* (2005), which has devoted considerable attention to indigenous Egyptian scholars of Egyptology, who are frequently given only a few paragraphs of attention in histories of scholarly interest in ancient Egypt. Applying this approach to Wilkinson is also fruitful. Given that Wilkinson was always renowned – by contemporaries and modern historians of Egyptology – for his studies of aspects of daily life and material culture, which were the main theme in his *Manners and Customs of the Ancient Egyptians* (1837), and not for his linguistic contribution, this expanded definition of "Egyptologist" allows Wilkinson's most important work to be placed in the category "Egyptology". Under the narrower definition, Wilkinson qualified as an "Egyptologist" almost accidentally, for a less important aspect of his work.

Although appealing, this more capacious approach nevertheless has profound problems. Most obviously, however outward-looking, it is a back-projection, usually undertaken by disciplinary insiders to establish a lineage and therefore legitimise their discipline by giving it historical respectability, resulting in anachronisms which are potentially misleading. The issues at stake are succinctly noted by William Carruthers, in his introduction to the multi-authored *Egyptology: Interdisciplinary Measures*. Referring specifically to the WWW, discussed above, Carruthers notes

> the volume's encyclopedic thrust… establishes a heroic disciplinary genealogy by mobilizing figures from many other worlds into a story of "Egyptological" achievement. If conducted by the right sort of individual, then, the volume implies that the discipline's work (whatever that work is) will usefully and unproblematically proceed, even if the individual involved does not identify [or did not identify] themselves as an Egyptologist. The publication co-opts various individuals for the discipline's purposes, and in the process, it makes the discipline more real as a unified entity (2015: 5).

In reality, the study of ancient Egypt is not unified, and has been deeply contested. Although the most recent edition of the WWW is not light reading – it consists of over 500 pages of notes on individuals, in alphabetical order and is not a narrative that is intended to be read cover-to-cover – Carruthers's contention will be understood by those

who have read even a sample. In some respects, expanding the definition of Egyptology meets some of the same issues as those which run throughout *World Antiquarianism: Comparative Perspectives* (Schnapp, 2013) – a historically-discrete term is expanded and loses much of its original meaning. In his book on the interrelations between the study of ancient Egypt and religion in the period between 1822 and 1922, Gange (2013) comes to a similar conclusion, and notes that the form of Egyptology that became consolidated as a discipline in the late 19th century followed one direction out of many that were then available. Caution is therefore needed before annexing earlier scholars to current Whiggish histories of Egyptology and archaeology.

Such objections are of considerable importance and will remain so for all subsequent considerations of the history of scholarly research into ancient Egypt in the 19th century. However, they potentially overstate the extent to which there was a break between earlier traditions of research into ancient Egypt, and later ones after disciplinary consolidation took place. Although not meant by the two aforementioned scholars, the suggestion that "Egyptology" should either be dropped or limited to the late 19th century onwards is similar to T. G. H. James' (1997) preference for limiting the usage of the term to linguistic researchers, before the broader definition of Egyptology's remit became popular. In their introduction to the recent textbook, *A World History of Egyptology* (Bednarski, Dodson and Ikram, 2021), the authors adopt the solution of retaining the existing narrative of key events before the disciplinary consolidation took place under the rubric "prehistory" of Egyptology, as opposed to a more conventionally-titled "history" of the enterprise. Whilst this may appear teleological, it balances nomenclature against the desirability of acknowledging earlier scholars. It is also worth noting that in his chapter of the book, Aidan Dodson (2021) notes that even if the term "Egyptology" appeared relatively late in Britain, there were earlier terms such as "Egyptologer" which were in use several decades before.

This is a dimension which is of some relevance to Wilkinson. In his biography of Wilkinson, Thompson records two occasions of Egypt being used to define Wilkinson's identity: in society circles, he was referred to as "the Egyptian traveller" (1992: 79), or, simply "the Egyptian" (1992: 165). From a modern perspective, the usage of the latter phrase in particular undoubtedly carries connotations of cultural appropriation but, to

stay on topic, his encouragement of it suggests that he saw his studies of ancient Egypt as constituting a major part of his (academic) identity. In using the term, Wilkinson may well have been consciously emulating his French rival, Champollion, who was also known as "the Egyptian" (Ceram, 1952). More distant predecessors include Stukeley's Society of Roman Knights, in which group members took on names of figures associated with Roman Britain, and the short-lived Egyptian Society, in which the members, most of whom had visited Egypt in the early/ mid-18th century, took on Egyptian or Arabic names, and had some scholarly pretensions (Sweet, 2004). In 18th century societal circles, the architect James Stuart was known as James "Athenian" Stuart, after his preferred style (Soros, 2006). It is also worthwhile to note that it is not unusual for scholars to refer to themselves or their opponents in shorthand terms lifted from their subjects of study. In the early 20th century, with reference as to whether Minoan Crete had colonized mainland Greece or the reverse had been true, American classicist and archaeologist Carl Blegen had no compunction in referring to his opponents, led by Sir Arthur Evans, as the "Cretans". Despite doubtless being fully aware that Evans would never have identified as such – he maintained an aloofness from the native Cretans (Powell, 1973; Wood, 1998) – Blegen still felt that using the term in this way adequately stood in for an intellectual current in prehistoric Aegean scholarship. To return to Wilkinson, in a reply to a request for information from the geologist Charles Lyell, Wilkinson referred to "we in Egypt", suggesting that he felt some affinity to the museum-based Egyptologists of the day in terms of a shared intellectual enterprise. As well as casting himself as a regional expert, Wilkinson appears to have been regarded as one more broadly: he received at least one letter in which an interested member of the public referred to him as the expert in "Egyptian archaeology" – a term which was in use from at least the late 18th century and which suggests that contemporaries saw the study of Egyptian history as a distinct scholarly project, albeit a very niche one (Blumenbach, 1794).[6] Given that Wilkinson had several associates, who might be termed collaborators in his Egyptian research, it would seem overly hasty to dismiss the seriousness of research into ancient Egypt before the late 19th century.

[6] S.F. Jarvis to Gardner Wilkinson, 27.4.1846, "Letters, 1828-71", Bodleian Library, Wilkinson manuscripts, dep. d. 133.

To reconcile both of these perspectives, this thesis argues for the use of the contemporary term "antiquarianism": that is to say, Wilkinson was an antiquary, and he approached the study of ancient Egypt ("Egyptology") using a typically antiquarian methodology, which gave equal emphasis to texts and material culture, usually taking into account the wider landscape context (which, as has already been suggested, was equally true for his research in the Balkans) – a theme which has been discussed extensively in the aforementioned overview of scholarship on rehabilitated antiquarianism, mostly in a British context. Although several historians of Egyptology and archaeology have used the term "antiquary" or "antiquarianism" in a negative way, the majority of such histories date from the mid-20th century, and even these predominantly relate to activity before the 19th century (Von Hagen, 1950; Ceram, 1952; Greener, 1966**;** Murray, 1949, McIntosh, 1986; Daniel, 1967; Wortham, 1971). More recent examples made by academics are surprisingly few in number and most usages of the term in more recent works are relatively neutral, although they do tend to position antiquarianism within an evolutionary framework in which archaeology emerges as a significant advance (Vercoutter, 1992; Naunton, 2020). In other words, while the revisionist framework recommended here is not mainstream, it seeks to bring about a shift in emphasis as opposed to a total reconceptualization of the term under discussion. Whereas several previous historians of Egyptology have mostly confined antiquarianism to the 16th, 17th and early 18th centuries, this thesis seeks to follow Sweet's (2001; 2004) and Hill's (2021) lead and use the term in reference to Wilkinson's scholarship during the first half of the 19th century.

This repositioning of Wilkinson and the history of research into ancient Egypt in the roughly half-century before the emergence of Egyptology as a consolidated discipline in the late 19th century brings wider insights into the history of the subject than those currently available, which will now be surveyed. Conventionally, histories of Egyptology, and chapters on Egyptian archaeology in narrative histories of archaeology, overwhelmingly emphasise two themes which stand out above all others: philological research and the collection of antiquities.

The first of these is well-known: it consisted of a race between François Champollion and others (notably Thomas Young) to decipher hieroglyphs, which, of course, marked a

huge step forward in the study of ancient Egyptian civilization. This element features prominently – to the exclusion of nearly all other subjects – in both Hall's (1915) and James's (1997) consideration of Wilkinson. It also features in Thompson's (1992a) biography of Wilkinson, albeit to a lesser degree of dominance. An idea of the extent to which Egyptology has been seen as synonymous with hieroglyphs can be glimpsed from Stocking's (1972) review of John Wortham's *The Genesis of British Egyptology* (1971). Unusually for such histories, Wortham included material on geomorphology and prehistoric archaeology, but was criticised for doing so: Stocking advised readers to consult Eric Iversen's (1961) *The Myth of Egypt and its Hieroglyphs in European Tradition*, a far more narrowly-focused book.

In addition to, but somewhat separated from philology, the second dominant theme is that there is a corresponding narrative of progress for the history of Egyptian archaeology. In this narrative, the early 19th century saw a scramble for antiquities between European consuls in Egypt, such as Henry Salt and Bernardino Drovetti, along with their colourful agents such as Giovanni Belzoni, which gave way to professional Egyptology and scientific excavations, as the 19th century progressed. Turning-points in the transition to serious Egyptology are variously placed in the 1840s (Karl Lepsius's wide-ranging expedition), the 1850s (Joseph Hekekyan's pioneering stratigraphic excavation), the 1860s (Mariette's crackdown on antiquities collecting and the excavation of multiple sites) and, above all, the 1880s (the establishment of the Egypt Exploration Society by Amelia Edwards and R. S. Poole, and the arrival of Flinders Petrie). Initially Petrie acknowledged his intellectual debt to Wilkinson and even saw the visual focus of Wilkinson and his colleagues as a possible specialism in the subject that he could develop (Drower, 1985: 202-203). However, he soon constructed a narrative that there were no serious scholars active before him, and his suggestion that "the science of observation, of registration, of recording, was as yet unthought of; nothing had a meaning unless it were an inscription or a sculpture", has been widely cited as if it was made by a neutral observer of the state of scholarship, as opposed to a determined attempt by an individual to rewrite history in his own image (1932: 20).[7] After this, as the traditional narrative

[7] The degree of Petrie's selectivity can be appreciated from his not even mentioning Carter's discovery of the tomb of Tutankhamun in his autobiography.

continues, archaeologists, with their techniques perfected, were able to make the big discoveries, culminating in the discovery of the tomb of Tutankhamun in 1922, before broadening their horizons to consider the interrelationship between ancient Egypt and the environment, in the late 20th century. Thereafter, the number of techniques multiplied.

Whilst the philological dimension is important, even if scholars were much slower in their attempts to comprehend hieroglyphs than is sometimes assumed – Champollion's breakthrough did not allow him or anyone else to read whole sentences in the ancient script – the narrative on material culture is highly simplistic. A good demonstration of just how problematic the narrative is can be appreciated from examining the above-quoted contention by Petrie, which is incorrect at every single level. Wilkinson and his antiquarian colleagues, such as Robert Hay, Edward William Lane and Joseph Bonomi, excelled at observing and recording. Whilst their attention was piqued at the sight of sculptures and inscriptions, it was also directed to any accumulation of rubble, even piles of bricks, as such sites could potentially be stations of the Antonine Itinerary, the reconstruction of which was a major project. Petrie's implication that his predecessors were connoisseurs also sets up an artificial dichotomy: in order to tell what was Pharaonic and what was Ptolemaic or Islamic, antiquaries were forced to devise typological systems, based on stylistic evidence of temple columns and other architectural remains. Sometimes this could be based on whole buildings, at other times it was based on fragments (this was inspired by a long tradition in which typology had been used for Medieval buildings and much else). Whilst it would be highly disingenuous to suggest that his work made no advance on that of his predecessors, Petrie's methods were anticipated by Wilkinson and others several decades before he arrived in Egypt, and his work was possible mainly because it built on the sophisticated work of these earlier researchers.

In addition to anticipating Petrie, other dimensions of what are generally supposed to be defining elements of modern Egyptian archaeology are present in Wilkinson's work and sometimes that of his contemporaries: Wilkinson obtained spatial information from Pharaonic monuments such as the Ramesseum and Medinet Habu, which he cross-referenced with texts and his knowledge of landscapes in the Levant. In the Nile valley, he

undertook geomorphological fieldwork, the results of which reinforced images of the state of development of Egyptian society which he saw in Egyptian relief scenes and tomb paintings. Even when it came to stratigraphy, Wilkinson was able to comprehend the flaws in what was vaunted as a highly-sophisticated new excavation programme, in correspondence with the eminent geologist Sir Charles Lyell. In addition to these themes appearing in Wilkinson's work, decades before they are commonly placed in disciplinary histories of Egyptology, each of them depended at least in part on the investigation of problems raised in texts (usually classical as opposed to as-yet inadequately-understood Egyptian ones), or in using texts as vital sources, in a manner quite different to that of Petrie and late 19th-century pioneers of disciplined Egyptian archaeology. In other words, Wilkinson's work involved the unification of textual and material culture as sources, which have previously been frequently separated in extant histories. While antiquaries were once cast as stumbling around Egypt, intent on collecting with little thought for the surrounding landscape except perhaps for scenic views, the revised image of Wilkinson repositions them as careful observers who gave weight to different sources, and with constant concern about the landscape context in which they were found. If antiquarianism is a prominent theme for Wilkinson's work in Egypt, it was equally the case for Wilkinson's work in the Balkans, although in this case romantic antiquarianism emerges as a stronger influence on Wilkinson in addition to his traditional approach.

Chapter 3: Methodology

3.1 Intellectual History

My research into the work of Gardner Wilkinson takes a primarily "intellectual history" or "history of ideas" approach: I attempt to investigate the lineage of Wilkinson's ideas, and to identify those by whom he was directly influenced, in order to understand why he drew the conclusions that he did; and in some cases, trace the reception of his conclusions.[1] As opposed to seeing Wilkinson as a standalone "great man" who developed hypotheses and conclusions in isolation, I seek to draw out precisely whose work he built upon and whose ideas he borrowed from, whether or not he explicitly referenced them.

An "intellectual history" approach deliberately avoids teleology. In several cases, aspects of Wilkinson's scholarship indeed seem to anticipate, by decades or even a century or more, later ideas like uniformitarianism in geology, the Neolithic Revolution, processual archaeology and modern landscape archaeology. However, in all cases these pursuits had antecedents reaching back to the early 19th century and even before. Acknowledging and exploring these allows for a less radical, albeit far more realistic assessment of Wilkinson's scholarly contribution to be built up, and for generalist statements that he was "ahead of his time" to be scrutinised. By situating Wilkinson within his intellectual context, he appears more representative than unique. "Catastrophist" geologists followed a uniformitarian methodology in most cases, while Scottish and French philosophers devised models of human development that bear striking resemblances to those later favoured by prehistorians. Similarly, antiquarian topography, which in Wilkinson's case took place in Egypt and the Balkans, was based on meticulous fieldwork, while that work which resembles interdisciplinary landscape archaeology was entirely typical of the "predisciplinary" context in which he worked.

Nevertheless, despite such superficial resemblances, an "intellectual history" approach necessarily goes beyond simply evaluating whether Wilkinson's conclusions were "right" or "wrong" when compared with those favoured by modern scholarship, or selectively choosing and emphasising cases in which Wilkinson's work seems to agree with modern

[1] See Whatmore (2016); Whatmore and Young (2016).

orthodoxy. As Gange, in his recent study of 19th century research and popular interest in ancient Egyptian history has put it, "the question of whose guess [about Egyptian chronology] was 'most wrong' is hardly relevant. More interesting are the reasons that various scholars gave… the type of evidence they wished to use, and the remarkable… differences in method and results" (Gange, 2013: 31). It follows that my present concern is with how Wilkinson's research involved evidence-based reasoning (often from a very inadequate evidence base) and what his methodological approaches were. In one case, in the second half of the fourth chapter, it will become clear that Wilkinson's conclusions differ significantly from the modern consensus – but it will also become clear that he followed a coherent methodology to arrive at his conclusions, which were by no means outlandish given the nature of his sources.

By paying attention to intellectual debates, no attempt is made to imply that Wilkinson or any other scholar worked in a cultural vacuum, unaffected by personal or political biases. In the forthcoming chapters, many of these will be noted, and, it would be more surprising had Wilkinson abandoned all of his preconceptions on arriving in Egypt, Dalmatia, Montenegro and the other places in which he worked. However, he did not do so, and these inevitably influenced how he approached topics, and the conclusions that he drew.

At the same time, as Peter Rowley-Conwy has cautioned, in the introduction of his study of the uneven uptake of the Three Age System for prehistory, it is important to avoid crude deterministic explanations which make "aspects of 'social context'" such as nationalism "paramount, and… more or less the sole object of investigation" (2007: 13).[2] As well as being influenced by them, individuals such as Wilkinson acted outside of them. For example, Wilkinson's ideas about the Egyptian empire in Africa and Asia were conditioned by modern British imperialism in India, and modern Egyptian empire-building in Africa. However, both of these factors fade in importance next to the challenge of interpreting ambiguous passages of classical authors such as Herodotus and Diodorus

[2] Early attempts at writing the history of archaeology, such as that made by Glyn Daniel (1975), attempted to explain the enthusiasm for the Three-Age System in Denmark as being driven mainly by nationalism, which is reductive and ignores the role of key individuals. Similarly, Sweet (2001; 2004) acknowledges that nationalism/ patriotism was present in British antiquarianism, but explores other factors, such as the role of individuals who were genuinely interested in their research topics. Briggs (2007: 255) has noted that neither John Lubbock nor John Evans were constrained in the debate on human antiquity.

Siculus, and triangulating between these and a novel source, Egyptian relief scenes. Similarly, Wilkinson held religious beliefs, but, contrary to the suggestions made by some scholars (Thompson, 1992a; Klaver, 2010; Gange, 2013), these were not the key determining factors which conditioned his work on Egyptian chronology, or his geological research: in both, other factors, explored at length in this thesis, were more important. To reiterate, an intellectual history approach deliberately avoids one-liner explanations and acknowledges that, as in the case today, academic debate existed over a range of issues.

With such theoretical approaches noted, the remainder of the present chapter seeks to give an overview of the range of sources, on which the arguments and conclusion rest. As far as published sources are concerned, Wilkinson's lesser-known works such as *Materia Hieroglyphica* (1828), *Topography of Thebes, and General View of Egypt* (1835) and other works, notably *Dalmatia and Montenegro* (1848) are discussed, alongside his well-known *Manners and Customs of the Ancient Egyptians* (1837). Similarly, Wilkinson's articles, notably his little-known but important "On the Nile, and the present and former levels of Egypt" (1839), are considered. In addition to printed sources, I have attempted to triangulate between Wilkinson's extensive manuscript collection in the Bodleian Library, and his library, at Calke Abbey.

3.2 Wilkinson's books and journal articles

Wilkinson was a prolific author: he wrote ten major books, several of them multi-volume works, mostly published during the two decades after he returned from Egypt, and some thirty-seven pamphlets and journal articles, spread across five decades, from the mid-1820s to the mid-1870s (one was published posthumously).

The main books that this study has drawn upon are Wilkinson's *Materia Hieroglyphica* (1828), *Topography of Thebes, and General View of Egypt* (1835), *Manners and Customs of the Ancient Egyptians* (1837; 1841), *Modern Egypt and Thebes* (1843), *Handbook for Travellers in Egypt* (1847) and *Dalmatia and Montenegro* (1848). Several of Wilkinson's works were a commercial success: his *Handbook for Travellers in Egypt* became the most widely-read book among 19th-century travellers to Egypt, while an abridged version of his

Manners and Customs appeared in his lifetime which would have allowed a relatively broad spectrum of British society (if not the majority) to access parts of his work. Whilst publishing by subscription, or at the author's own expense, had been commonplace in the 18th century, Wilkinson worked with the publisher John Murray, renowned for travel, natural history and related subjects (Keighren *et al.*, 2015), as was becoming increasingly common in the 19th century as production costs for books fell (Keighren *et al.*, 2015).

Although widely-read, Wilkinson's books were nevertheless very important scholarly works. His *Materia Hieroglyphica* (1828), published while he was in Egypt, contained, alongside an extensive study of ancient Egyptian religion, a pioneering attempt to construct a chronology of Egyptian kings, as well as an interesting attempt to use temples and their relief scenes to reconstruct Egypt's political and military history. Wilkinson took this approach considerably further in *Topography of Thebes, and General View of Egypt* (1835). Although the main feature of this work was, as the name suggested, an overview and commentary on the sites associated with the Pharaonic era – the remains of towns, tombs and so forth – it also advanced a very bold argument about the development of ancient Egyptian civilization, and forwarded a thesis that ancient Egypt had extended deep into Africa, as well as a considerable distance into southwest Asia. It also featured a chapter in which Wilkinson attempted to extract information about the daily life of the ancient Egyptians, from material sources such as everyday objects and tomb paintings. He amplified his findings into the three-volume *Manners and Customs of the Ancient Egyptians* (1837), which was a considerable success: it was reissued in an expanded edition four years later, and led to Wilkinson receiving a knighthood.

Compared with these publications, Wilkinson's *Dalmatia and Montenegro* (1848) was a very different work. A reading of letters that Wilkinson wrote to his publisher, John Murray – Murray's letters to Wilkinson do not appear to have survived – strongly suggests that Murray pressed him to conform to the increasingly popular genre of travel writing.[3] In this, he was partially successful: unlike any of Wilkinson's previous books, *Dalmatia and Montenegro* was centred around a trip through the Balkans, much of it narrated in the first person. Wilkinson began this voyage in Istria and travelled south,

[3] Wilkinson to John Murray, John Murray Archive, National Library of Scotland, Ms. 42177, fols 124-151, 1843-44.

between the major towns on the Dalmatian coast, visiting the classical-era remains and antiquities. An excursion into Montenegro (and more briefly to the Ottoman province of Bosnia) formed the culmination of Wilkinson's narrative. Murray was not entirely successful in his attempts to direct Wilkinson's writing, as the published work contained substantial sections of narrative history, some extending for dozens or even a hundred pages at a time, such as his multi-faceted account of the history and culture of the formerly-independent republic of Ragusa.

Strictly speaking, although Wilkinson's published output appears vast, not all of it was new work: he recycled material across his Egyptian publications. For instance, many of the ideas rehearsed more extensively in *Topography of Thebes, and General View of Egypt* (1835) first appear more briefly in *Materia Hieroglyphica* (1828). Most notably, *Modern Egypt and Thebes* (1843) was really only an expanded version of his *Topography of Thebes, and General View of Egypt* (1835). Similarly, he converted his two-volume *Modern Egypt and Thebes* back into a single volume for his *Handbook for Travellers* (1847), but with the addition of useful material on travelling from England to Egypt, and directions to notable sites. However, a notable advantage of this phenomena is that, given that the publication dates provide a "latest" date for Wilkinson holding an idea, it is occasionally possible to track Wilkinson's ideas changing through time, as he encountered new evidence and reconsidered complex issues. Notable examples are his initial dismissal, then eager acceptance, of the existence of Egyptian fortifications in Nubia, and his identification of the mysterious "Shepherd" or "Pastor" kings (now known as the Hyksos), who he initially identified as Hebrews (1828), before changing to favour Assyrians (1837) and, finally, Scythians (1841). However, given that Wilkinson wrote most of his books in fairly quick succession, there are limits to the extent to which this is possible for most issues under discussion.

Wilkinson also published several dozen articles in academic journals. The number of these proliferated in the 19th century; new scholarly clubs and societies helped to advance a general idea of education as an improving pastime, while there was also a background of concern that Britons needed to catch up with their European counterparts in various scholarly fields, and, of course, the possibility that such research might advance Britain's concerns abroad (Sweet, 2004; Cameron, 1981). Although Wilkinson would not

have earned money from these, unlike some of his more successful books, publishing in journals was at least financially risk-free and enabled him to circulate ideas with his name attached to them. The most important of Wilkinson's articles to be discussed here is his "On the Nile, and the Present and Former Levels of Egypt", published in the *Journal of the Royal Geographical Society of London*, in 1839. To Wilkinson, the newly-established and wide-ranging RGS would have seemed a logical place to publish his research into a theme associated with ancient Egyptian history and geography. At the same time, Wilkinson's choice of outlet may well account for his work being subsequently overlooked: the journal covers the whole world, and as such it is not an obvious source for those interested in the history of scholarship into ancient Egypt (see Twidale, 2004).

As a result of the closure of the Bodleian library for much of 2020 and 2021, due to the global Covid-19 pandemic, the present study draws particularly heavily on Wilkinson's published material, although it is important to note that many of the issues discussed have not previously received extensive attention, despite featuring in relatively easily accessible publications.[4] I obtained original volumes of Wilkinson's landmark *Manners and Customs*, albeit in a heavily damaged and essentially worthless condition.[5] In other cases, I used digital copies: nearly all are accessible through online libraries, notably the *Internet Archive*, and others such as the *Hathi Trust* and *Gallica*. I favoured first editions over abridgements or later edited collections, which frequently selectively choose parts which agree with modern scholarship, and in doing so omit large sections. The only exceptions to this are when I attempted to trace changes through subsequent editions, and in the special case of the images from *Dalmatia and Montenegro* (1848).

[4] Booth (2018) notes that some collections have been seen as amateurish and not taken seriously by past scholars. Although this has not been the case since Thompson's publications, this may well have been an issue during the 20th century, especially for the parts that I have focused on.

[5] I have encountered physical copies of most of Wilkinson's books at one time or another, although issues of abstraction and questions surrounding materiality have not been major research questions. The major exception is in relation to images. For these, I have, due to the low quality of the illustrations in the otherwise excellent copies of Wilkinson's *Dalmatia and Montenegro* available via the Internet Archive, had to rely on those from a 1971 reprint of the book.

3.3 Wilkinson's manuscripts

Alongside an extensive array of published material, my study has used unpublished manuscripts from the Gardner Wilkinson papers in the Bodleian Library, as well as elsewhere. Before discussing the content of these papers, a brief note will be made about their curatorial history. Prior to 1981, many of the Wilkinson manuscripts were kept in the Griffith Institute (University of Oxford), and researchers who consulted these papers encountered "56 bound volumes, folders and notebooks", mainly relating to ancient Egyptian subjects (Clayton, 1982). However, the complete collection is in fact well over twice this size. As Thompson (1992b) explains in a short article, all of Wilkinson's manuscripts and books were originally bequeathed to his cousin, Sir John Harpur Crewe, at Calke Abbey in Derbyshire – a place Wilkinson had visited on multiple occasions. However, the collection was split in 1925, as a result of the Egyptologist Francis Griffith taking a portion of the manuscripts for study; these were retained, with permission of the Harper-Crewe family, in Oxford by the Griffith Institute, for researchers to consult (Thompson, 1992b). The manuscripts were reunited after ownership of Calke Abbey passed to the National Trust in 1985, and all of them were deposited in the Bodleian Library (but remain the property of the National Trust).

The material in the Bodleian Library consists of "journals", sketchbooks, correspondence and papers related to Wilkinson's academic projects (Fig. 3.1). All of these have been used to a greater or lesser degree. The collection was expertly catalogued by Sarah Flynn in 1997 (but see below).

One prominent category of manuscripts are Wilkinson's 45 travel "journals". Despite the name, the category is highly heterogeneous, and covers material including diaries, notebooks and travel journals: some of the latter category appear to have been written over an extended period of time, though others may have been written up on return. These "journals" cover Wilkinson's sojourn in Egypt, as well as his other travels, although the majority – about four-fifths – are of Egyptian subjects. Alongside details of where Wilkinson went, these contain a range of material, including descriptions of antiquities and local customs, as well as the occasional line-drawn illustration. As previous researchers have noted (Thompson, 1992a; James, 1997), Wilkinson's handwriting is often very difficult to read, and this has been a challenge in using this source.

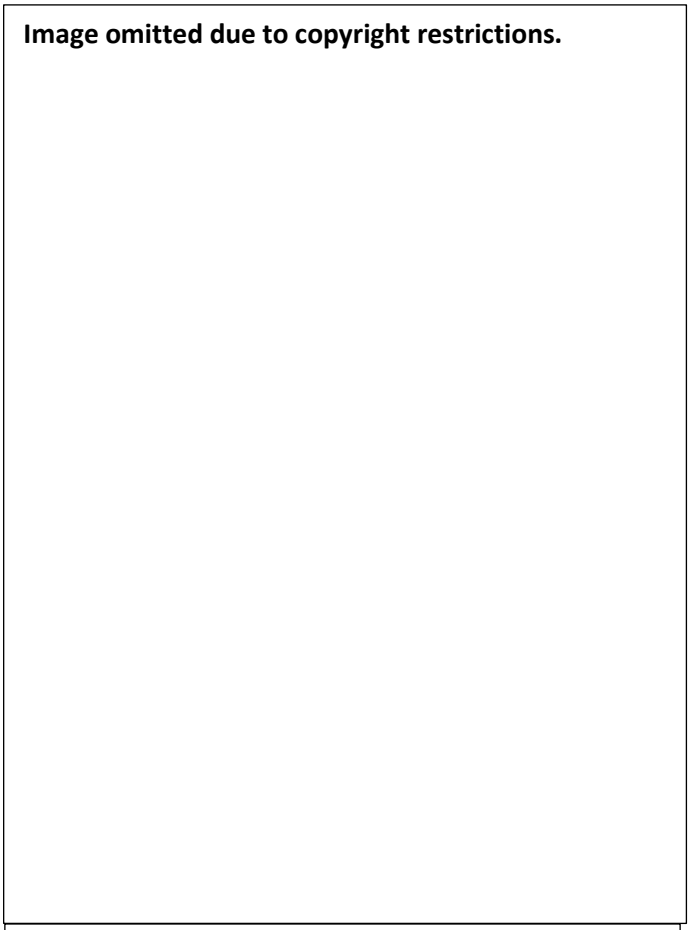


Fig. 3.1: A typical example of one of Wilkinson's notebooks, in the Bodleian Library, consulted in November 2018.

Another prominent source are the images in Wilkinson's collection of sketchbooks which have, as with his other manuscripts, survived in a unified collection. There are 83 sketchbooks, and they essentially consist of notebooks of a range of sizes from under A5-size to about A3-size, often of about forty pages in length, with different types of images, from crude line drawings to impressive watercolours. Of these, about a quarter are of places and sites in England, nearly half are of modern European subjects (including ten on the Balkans) and the remainder are of Egyptian, European and British antiquities and historic sites. The sixth chapter will discuss a selection of Wilkinson's sketchbooks, those that he filled during his trip to the Balkans in the early 1840s. These contain several hundred images of landscape scenes, local architecture, ruins, and, especially, costumed figures. Like many of his contemporaries who travelled on the continent after the

Napoleonic Wars, Wilkinson appears to have produced many of his amateur artistic pieces whilst in the field and for his own enjoyment, possibly while waiting for his accomplices to negotiate access or to arrange transport means (Piana *et al.*, 2018a). Whilst some of these scenes doubtless "capture… landscapes at a precise moment in time" (Piana *et al.*, 2018b: 130), it is interesting to note that on publication, in Wilkinson's *Dalmatia and Montenegro* (1848), several were subtly altered. Although it was not unusual for engravers or even artists to introduce features that were not present in reality (Koivunen, 2009; Piana *et al.*, 2018c**;** Piana and Watkins, 2020), it is notable that this manipulation is mainly confined to the images of Montenegro, which both Wilkinson and John Murray, his publisher, wanted to depict in a particular way. With these issues in mind, this thesis will take a critical perspective toward the images discussed; this is equally applicable to maps and other diagrams, which are inevitably highly selective in their detail (Monmonier, 1995; Harley, 2001) – as will be discussed in the fifth and sixth chapters – and which Wilkinson employed for rhetorical purposes.

A third group of manuscripts in the Bodleian library consists of correspondence between Wilkinson and various associates: many of these have been used by Thompson (1992a) in his biography of Wilkinson. Some of the most important to be considered in the forthcoming are Wilkinson's correspondence with the British antiquary William Gell, who was based in Naples and Rome. Although James (1997) conjectured that Gell frequently ignored Wilkinson's letters due to his unclear handwriting, it is clear that the correspondence was not one-way, and both men corresponded over scholarly (and occasionally other) topics, throughout Wilkinson's time in Egypt. The most important series of correspondence that this study considers is one between Wilkinson and the eminent geologist Sir Charles Lyell, widely-known today for his energetic propounding of uniformitarianism in geology. This latter correspondence runs to approximately 8,000 words and forms the basis to the second half of the fifth chapter. Unusually, it is kept at Calke Abbey, and is referred to at this point as it is in manuscript form and as such is clearly associated with the other manuscripts presently under discussion.

A final category of manuscripts held at the Bodleian library are those related to Wilkinson's scholarly work: these include drafts, proof copies and unpublished material which he later reworked into subsequent books and articles. Of particular importance to

this study is a manuscript account of a journey in Upper Egypt, which Wilkinson drafted in the mid-1820s, with an intention to publish, but which was not taken up. However, it is of great relevance to the present study on account of it showing Wilkinson's methodology and working assumptions, in greater relief than his subsequent published work on the same topic. As several scholars (Haycock, 2002; Rudwick, 2008; Sweet, 2012) have noted, unpublished manuscripts frequently contain a more faithful indication of a writer's views than those that they published, either because they lacked confidence to propound ideas with insufficient evidence, or because societal and/ or social pressures encouraged them to self-censure. As Keighren *et al.* (2015) and Bridges (2002) have noted, substantive changes were frequently made by publishers to ensure that narratives would appeal to a broad audience. These factors mean that consulting Wilkinson's original sources is an imperative.

At the start of this project, access to the Weston library branch of the Bodleian Library was a straightforward affair, and I made several visits, including a pilot visit in 2017, and several visits through the first and the start of the second year of research. I could order up to ten items in advance, and on-the-day orders were also possible, enabling substantially more material to be consulted if some items turned out to be less valuable than expected. While at the library, I signed the relevant forms to enable me to make copies of any of the Wilkinson material for study, which was very useful, as it enabled me to study material ex situ – a considerable advantage which I appreciate from previous archival work, including in collections which still heavily restrict photography, such as the Royal Geographical Society Archive.

Unfortunately, in March 2020, the Covid-19 pandemic led to the closure of the Weston (and the other branches of the Bodleian) library to all researchers. Key sources in Edinburgh, London, Calke Abbey and elsewhere also became inaccessible at the same time. This resulted in me having to forego plans for extended visits to Oxford (and shorter-duration ones for elsewhere), planned for Easter and Summer 2020. Although the Bodleian library reopened to researchers during the second half of 2020, it closed again in January 2021 (with exceptions for researchers based at the University of Oxford) and remained closed until the middle of the year. When the Bodleian library reopened, reduced staffing and "social distancing" meant that stringent limits were placed on what

could be ordered: it became possible to order five items every two days. [6] In practice, this translated to ten per week: less than a fifth of what it was previously possible to see before the pandemic. The result of these closures meant that I was able to consult fewer of Wilkinson's manuscripts that I otherwise would have been able to look at during 2020-21. Although the Bodleian library catalogue made it easier than it otherwise would have been, the level of detail for many items in the collection was occasionally inadequate for the specific subjects that I was interested in, and it was not possible in every case to find the original manuscripts that I looked for in the time that was available.[7] This issue was worsened in some regards by the Wilkinson manuscripts being re-catalogued in 1997: it is only possible with great difficulty (usually by relying on dates, which are generally given as a very broad range in the catalogue, adding yet another layer of difficulty) to match references in Thompson's biography with manuscripts listed in the catalogue. Although such issues are very common in archival research, they became more difficult than they otherwise would have been on occasions in which I had to make a decision of what to order from a number of items with similar descriptions and identical date ranges. On some occasions I was able to address issues, but on other occasions, month-long waits for manuscripts proved disappointing.

To an extent, my lack of access to manuscripts for extended lengths of time was offset by the Bodleian library's mediated copying service, which was offered from late August to late December 2020, and from May to December 2021. With the falling number of infections in 2021, archives and libraries reopened, and I was able to acquire substantially more material than I had been able to acquire over the preceding year. Although such issues are not normally discussed in methodologies, to omit them would render it impossible for future readers of the following thesis to grasp the difficulties involved, and

[6] "Social distancing" was a practice which required people from separate households to avoid personal contact, either by a metre or two metres, and was combined with wearing face masks; the idea being to reduce the opportunity for the airborne virus to spread from one person to another. It was officially mandated for much of 2020 and 2021, and unofficially for particular places, such as the Bodleian, for longer.

[7] This issue is partly why the first empirical chapter is largely unillustrated. From the *Topographical Bibliography***,** I know that Wilkinson definitely sketched the relief scenes of the Ramesseum and Medinet Habu. However, sketchbooks which the catalogue labels as "Thebes" are insufficient to identify these particular ruins. I have seen other relief scenes and paintings that Wilkinson sketched, but have had to rely on other sketches and photographs of these relief scenes (from which it is possible to trace what Wilkinson was seeing in every case).

the frequently extreme limitations placed on what became possible, for much of 2020 and 2021.

3.4 Wilkinson's library at Calke Abbey

Like many other Georgian and Victorian gentlemen scholars, Wilkinson possessed an impressive personal library (Figs. 3.2-3.4). Unusually, this survives in its entirety, at Calke Abbey, Derbyshire. Like Wilkinson's manuscripts, his library of nearly 1,600 works was acquired by the National Trust along with Calke Abbey (Purcell and Thwaite, N.D.). Given that thorough attempts have been made to reconstruct the libraries of scholars such as Abraham Ortelius and Francis Willughby (van den Broeke, 2013; Poole, 2016) in order to establish what work they most likely consulted and were influenced by, the survival of Wilkinson's library is potentially a very useful resource in a study of his work from a history of ideas perspective.



Fig. 3.2: Wilkinson's library at Calke Abbey (October 2021).

The range of subjects represented in Wilkinson's library is extremely diverse, covering ancient history (Egypt, Greece, Rome and beyond), linguistics, natural history, modern politics, and many other subjects. Given access limitations, again imposed by the pandemic, I have only handled a small number of volumes at Calke Abbey, though have occasionally been able to note that Wilkinson referenced a particular notable work, as a result of having access to a complete catalogue of the contents of the library.

Image omitted due to copyright restrictions.

Fig. 3.3: Books from Wilkinson's library at Calke Abbey.

Given that nearly all of the books in Wilkinson's library – or at least those that I have considered in this study – are available to read, and are often downloadable, through online libraries such as the *Internet Archive*, my main intention in consulting Wilkinson's books was to look for signs of readership, especially marginalia, the study of which is now of major interest, as it can give insights as to how contemporaries interpreted books and what they sought to gain from them, neither of which are necessarily obvious from a gap of several centuries (Jackson, 2001). Other unique features of Wilkinson's books are the occasional letters and sketches in some volumes, mentioned in the catalogue of Wilkinson's books. In practice, of the sample of books that I viewed, all were either

unannotated or virtually clean, although in one case (Buckland, 1837), a very short annotation revealed that Wilkinson's objection to a geological issue was indeed based on what were, at the time, widely-held ideas within the geological community, and not fringe ideas as has previously been supposed. Of considerably more use was the series of letters, mentioned above, which Wilkinson and Lyell exchanged over 1861-63, which were pasted into the back of Lyell's *The Geological Evidences for the Antiquity of Man* (1863), which he gave to Wilkinson. If these letters had not been physically attached to the book, it is likely that they would now be in the Bodleian library. As will be explained in the below, the existing interpretation of Wilkinson's correspondence with Lyell is highly erroneous: from consulting only Wilkinson's letters to Lyell, Thompson (1992a) concluded that Wilkinson was motivated to make contact due to religious objections to modern geological research. However, the survival of Lyell's side of the correspondence, which only exists at Calke Abbey (which was then inaccessible to Thompson), reveals that Lyell contacted Wilkinson rather than vice versa, and that he did so with regards a major archaeological controversy of the time. As the correspondence progressed, Wilkinson demonstrated impressive knowledge about the Egyptian landscape, towards which Leonard Horner, the interpreter of the excavation, took a reductive view. Religion was only briefly discussed, and Wilkinson was not dogmatic.

Although Wilkinson's library has potential, it is nevertheless important to be aware of the limitations inherent in using both it and the catalogue of it. One issue is that Wilkinson's library is incomplete in the sense that it does not contain all of the books that he consulted for his research, especially that regarding Egypt, much of which he did when he was younger. Instead, the contents of his library date disproportionately to the later stages of his life, when he possessed both a sufficient disposable income to buy them, as well as, after his move from London to South Wales, space to store them.

An appreciation of the extent to which Wilkinson depended on other sources for books can be acquired from comparing references in his books to those which appear in his library: notable works absent in the latter include books by scholars such as Voltaire, Champollion and James Bruce, as well as the French *Description de l'Egypte*, to which Wilkinson frequently referred. Subscription libraries were one way to acquire access to books. Another library that Wilkinson definitely made good use of was that of the

antiquary William Gell: he allowed Wilkinson to draw on the material relating to ancient Egypt, which meant he was well-informed when he first visited Egypt in 1821. One of Wilkinson's notebooks from his stay with Gell shows evidence of his extensive reading; he was particularly interested in William R. Hamilton's *Aegyptiaca* (1809).[8] Technically this book exists in Wilkinson's library – Hamilton sent Wilkinson a copy as Wilkinson's reputation grew – but it is unannotated, and as such might indicate that Wilkinson had not been influenced by it, when this was, as will be seen, decidedly not the case. Even in Wilkinson's later life, there are gaps between what he read and what exists in his library: his correspondence with Lyell shows that he read two notable papers in *Philosophical Transactions*, which will also be discussed in due course, but these are not present in his collection, because Lyell requested their return.

Whether Wilkinson had read all of the books in his library is also a difficult question to answer and this is a much-debated issue at the broad scale. Sweet (2004) has noted that subscription lists of elite patrons in antiquarian works should not be read uncritically as lists of interested parties, as many subscribed so as to gain cultural capital from being associated with advancing learning and understanding of British history. On the other hand, Purcell (2017) has warned that there is a need for wariness before assuming that the libraries of elite Britons were filled with decorative, but unread volumes of books. One clue for ascertaining reading is the presence of marginalia, but it should not be assumed that all readers would have wanted to scribble over their books. Despite being largely unannotated, none of the books that I was able to physically handle was still uncut, which at the very least demonstrates an intention to read them. Undoubtedly, as with reading practices today, a balance probably lies somewhere in between. The above overview of possible issues can be briefly summarised. First, it should not be assumed that Wilkinson was unaware of a particular book if it was not in his library. Second, without marginalia it is not possible to say whether Wilkinson read a particular book, but the possibility definitely exists.

[8] "Extracts, notes and sketches on Egyptology, hieroglyphics and related subjects, *c*.1821", Bodleian Library Wilkinson manuscripts, dep. e. 55.

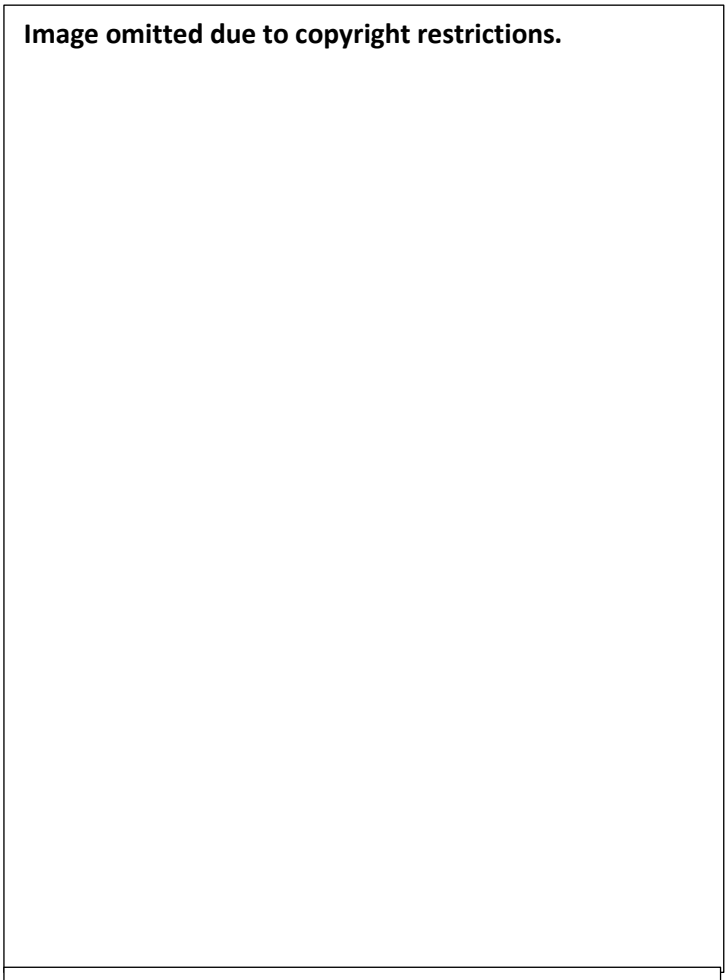


Fig. 3.4: Wilkinson's armorial book plate, which features on the inside cover of most of his books. It very much encapsulates his interest in human and natural history.

Chapter 4: Wilkinson's investigations into the extent of the ancient Egyptian empire

4.1 Introduction

One research question that Wilkinson was particularly interested in was what the territorial extent of the ancient Egyptian state had been: he addressed it in his first (unpublished) book-length manuscript, as well as in two of his earliest books, *Materia Hieroglyphica* (1828) and, far more extensively, in *Topography of Thebes, and General View of Egypt* (1835). Over a decade after this work, he returned to the question in a short journal article, after new evidence came to light.

At one level, the existence of an ancient Egyptian state had long been known in Europe, due to the Bible (Wortham, 1971). Prominent parts of the Old Testament; notably the narrative of *Exodus*, were largely set in the country, and it provided a narrative of sorts for at least a part of Egyptian history. However, there were profound limits to the usefulness of the account for ascertaining questions about ancient Egyptian geography. Whilst it was clear that the state had been centred in northeast Africa, around the Nile Delta, there was very little indication of how far it had extended up the Nile into Africa, or whether it had extended to the northeast, into southwest Asia. Two classical Greek authors, Herodotus – who visited Egypt in the mid-5th century BC and devoted a book of *The Histories* to the territory (Pritchett, 1993) – and Diodorus – who devoted attention to the region in his universal history written in the 1st century BC – suggested that ancient Egypt had once controlled an empire of immense proportions. For others, there was an assumption that ancient Egypt had not extended far under the Pharaohs, and that it had achieved its greatest extent following its incorporation into the Roman Empire. To a large extent, this was backed up – and indeed informed by – the Biblical narrative: to many, the most familiar reference to Egypt was the flight of the Hebrews from the country. Far from demonstrating their military prowess in this episode, the Egyptians had been defeated on their home territory, with their king possibly drowning in the Red Sea. There was thus the possibility that Egypt might only have been powerful whilst able to make use of the free labour of the Hebrews.

Wilkinson took a very different view of the nature of the ancient Egyptian state, and, at the very start of his *Topography of Thebes, and General View of Egypt* (1835) – a book

which was far more than a topographic record of historic sites – he advanced a bold thesis. According to this, the Egyptians had transitioned from a hunter-gatherer to a pastoral and subsequently to an agricultural mode of life at an extremely early time, long before their neighbours (with the possible exception of Assyria, which also possessed an advantageous environmental backdrop in at least some of its territory). After the Egyptians became farmers, they produced a food surplus, which enabled people to take on other roles, such as administrators, priests and soldiers. This resulted in the development of a state. This state, with its professional army, was consequently able to dominate and plunder its neighbours in wars of conquest; it therefore became a wealthy empire. In its broad outlines, the model resembled the concept of the Neolithic Revolution, forwarded in the 20th century but, far from anticipating this model by a century, it is in fact overwhelmingly likely that Wilkinson was inspired by philosophers, conjectural historians and economists, many of whom were associated with the 18th century Scottish Enlightenment, such as Sir John Dalrymple, Lord Kames and Adam Smith (Meek, 1976; Piggott, 1976; 1989). These Scots, and their French counterparts, forwarded a number of models – all highly speculative but frequently drawing on the ethnographic observations of peoples in the South Pacific and the post-contact Americas, as well as, occasionally, ancient material culture – which held that humans had initially been primitive, but had improved over time, with civilization (or, in some models, decadence) the end result. Whilst following the broad general outline, Wilkinson differed by adding evidence from material sources, which in turn backed the contentions forwarded by Herodotus and Diodorus.

Having painted an impressive image of ancient Egypt as a mighty state from early on, Wilkinson drew his readers to consider more recent parallels. One was that the Pharaoh Necho II had maintained an empire which stretched to the Euphrates, in Mesopotamia, while a couple of centuries later, Ptolemy Euergetes' troops had apparently reached Bactria, in Central Asia. At home, Egypt was renowned by the Greeks as a centre for learning – philosophy, medicine and the sciences – yet by this time it had supposedly been in decline for centuries. Wilkinson pointed out in a footnote, added to his manuscript later on, that modern Egypt, with its diminished resources, was in the process of reconquering these areas (1835: xi). Overall, Wilkinson effectively urged his readers to

consider that, if Egypt was so powerful and influential in later times, then it must have been overwhelmingly so in earlier times – especially if its enemies had been comparatively weak or at a low level of economic development. Wilkinson included the same discussion about Egyptian geography, and how the ancient Egyptians had possessed a resource base that would have enabled and encouraged them to build an empire, at the very start of the first volume of his landmark *Manners and Customs* (1837) as well, which reiterates the importance of the topic to him.

Aside from such apparently common-sense reasoning, or what might have appeared to others as speculative extrapolation, Wilkinson arrived at this broad historical narrative only after conducting extensive research in Egypt; his approach which allowed him to reach such conclusions is well worth further consideration. He gave equal emphasis to the authority of textual sources (although as usual he valued some in some scenarios over others: in many ways he came to favour classical Greek sources over inferences drawn from the Old Testament) on the one hand, and to material culture on the other. He then considered both in association with the Egyptian landscape. In other words, Wilkinson followed a typically antiquarian approach which encompassed a range of methodological techniques. To trace the extent of the ancient Egyptian empire to the south, he tested the claims made by Herodotus and Diodorus against material evidence on the ground: stylistics, boundary stones and border fortifications. Denied access to Sinai and the Levant due to Egyptian military activity in the region, Wilkinson came up with an ingenious methodology to reconstruct an ancient geography by using relief scenes of temples as proxies for actual landscapes.

Somewhat surprisingly, given a recent surge of interest into western and indigenous Egyptian colonialism in Africa (Powell, 2003; Kennedy, 2013), there has been relatively little work on examining how early 19th-century scholars approached the question of ancient Egyptian imperialism. Even histories of ancient Egyptian imperialism, such as George Steindorff's and Keith Seele's *When Egypt Ruled the East* (1957), have entirely neglected early 19th century interest in investigating this issue. Instead, Steindorff and Seele substituted the familiar narrative of the decipherment of hieroglyphs. Strictly speaking, the decipherment of hieroglyphs was a necessary prerequisite for translating the Pharaonic sources that research into Egyptian conquests overseas drew upon, but the

overall impression is that their choice of writing a historiographical backdrop about linguistics is not the most appropriate one in a book which focused on space. More recent histories of ancient Egyptian imperialism tend to follow a similar pattern. For example, one prominent recent Egyptology textbook, in which the Egyptian empire is a prominent feature across several chapters, draws attention to the history of the decipherment of hieroglyphs, and gives only very brief attention as to how scholars came to believe that Egypt had once possessed an empire, the main exception being a very brief note on the discovery of commemorative stelae, which were in many ways one of the least convincing forms of evidence (Van De Mieroop, 2011).[1] As has been noted in the preceding literature review, histories of Egyptology (as opposed to the aforementioned histories of ancient Egypt) tend to focus on the discovery of tombs, lost cities and other novelties, as opposed to addressing complex scholarly problems.

There is no obvious explanation for why there has been a reluctance amongst historians and Egyptologists to acknowledge an early 19th century antiquarian contribution to the discovery of ancient Egyptian geography. One possible reason might be an inkling that the question is too obvious to be worth asking: as more travellers gradually reached further and further south, the discovery of more Pharaonic monuments would show that ancient Egypt had once had a presence there.

As will be seen though, the question is more complex, and the simple concept of "discovery" is in many ways insufficient to describe what was a contested and far from straightforward enterprise. In the case of the ancient extent of Egypt into Asia, political difficulties rendered it difficult to travel through Sinai, meaning that fieldwork was not an option, and alternative sources had to be found: the ones that Wilkinson turned to would be treated with much more caution today (Shaw, 2019).

Even where fieldwork was possible, there were a number of difficulties. Although Europe had experienced several waves of crazes for the Egyptian style, most recently the use of what were presumed to be stylistically Egyptian motifs in furniture (Curl, 2005), it was still unclear as to what the absolute defining features of the Pharaonic style were, in the event of a need to delineate it from other types. This meant that if a European

[1] Admittedly there are limits as to how much space can be devoted to disciplinary histories in textbooks.

traveller encountered a structure, there was no way to definitively confirm that it was from the Pharaonic era: there was the possibility that it could be later. With such a state of affairs, it was possible for a scholar in Britain to dismiss reports and even images provided by travellers who were themselves convinced of an ancient Egyptian presence on the upper reaches of the Nile. The reverse problem could also occur: an important Pharaonic-era site could be walked over, camped on and even sketched, without realisation of what it was, or its broader significance, even by attentive travellers – something that in fact happened to Wilkinson at Semna in Nubia (see below).

Even beyond such eminently practical concerns, Europeans brought preconceived ideas, cognitive systems and knowledge structures, which could both help and hinder their work (namely, Philhellenism, the Antonine Itinerary and evolutionary ideas about human development). The overall result was that, far from waiting until all of the evidence had come to light – or at any rate until the most obvious unexplored areas had undergone some sort of preliminary investigation – mental maps and conclusions were drawn up according to a mixture of scholarship based on textual and material sources, and conjecture.

4.2 The extent of ancient Egypt into Nubia

Upper Egypt (a territory occupying the southernmost two-thirds of modern Egypt and so-called due to its being located upstream of Lower or Northern Egypt; also commonly referred to as the Thebaid due to its being dominated by the town of Thebes, or present-day Luxor) and Nubia (roughly modern Sudan) loomed large in the debate over the geographical extent of Ancient Egypt to the south by the beginning of the third decade of the 19th century. This was a relatively new development. A generation earlier, and even for non-specialists in the 1820s, scholars had seen little reason to question the classical authors who had given precise information as to the southern boundaries of ancient Egypt. Herodotus had recounted that "according to the Oracle of Ammon, all those who drank the water of the Nile and lived to the north of Elephantine were Egyptians" (Wilkinson, 1843: 215). In contrast, he held that "the country inhabited by Ethiopians commenced beyond Elephantine to the south" (Wilkinson, 1835: 464). British and

European scholars took it for granted that Elephantine, an island in the cataract region adjacent to the modern garrison town of Aswan, had always been the southernmost extremity of ancient Egypt.

In the 18th century, few European travellers had gone much further upstream, so this picture was unchallenged. To reach Abyssinia/ Ethiopia and the source of the Blue Nile in the 1770s, James Bruce left the Nile valley just south of Aswan – and consequentially missed the remains, such as that of the great temple of Abu Simbel, that he would have encountered had he continued to follow the river. The British traveller William George Browne had intended to travel up the Nile valley past Elephantine in the 1790s, but local unrest had prevented him from doing so.

Frederick Lewis Norden, a Dane, and W. R. Hamilton, who travelled with a detachment of a British expeditionary force in Egypt, were among the closest to glimpsing some of the country beyond, in 1737-1738 and 1801-1802 respectively. Norden journeyed as far south as Derr before being forced to turn back, but returned to Europe and published a book, *Voyage d'Egypte et de Nubie* (1755) which recounted his travels; it even featured images of some ruins which he interpreted as Pharaonic temples, to the south of Aswan.[2] Nearly half a century later, Hamilton made his trip to Egypt, which he recounted in his *Aegyptiaca* (1809). Prevented by wartime conditions from travelling more than slightly upstream of Aswan, Hamilton closed his chapter on his journey to the far south by noting intelligence reports of "a very large temple" at "Calaptshi" [Kalabsha], in the region immediately south of Aswan. At the same time though, he admitted that "the accounts we received were extremely confused and contradictory" (1809: 37), so much that he was led to conclude that, beyond Aswan, "there is every reason to believe… the banks of the Nile… would have afforded us very few objects of interest" (1809: 39). Though Hamilton was aware of his predecessor's journey, he viewed Norden's sketches of diminutive temples – in stark contrast to the grandeur of Thebes to the north – as merely confirmative of his belief that the whole region to the south had always been materially poor. To him, the only possibility of it ever having been home to any sort of civilization

[2] This was translated into English too, as Norden (1757). *Travels in Egypt and Nubia*. Lockyer Davis and Charles Reymers: London.

was “during the reigns of the Ptolemies, or under the dominion of the imperial Eagles of Rome” (1809: 40).

In the 1810s however, this simplistic picture of ancient Egypt ending at Aswan and traces of it fading away began to be seriously questioned. The neat idea that ancient Egypt’s boundaries were identical to those of modern Egypt was most notably brought into question by Johann Ludwig Burckhardt’s discovery, in 1813, of the colossal temples at Abu Simbel, far to the south of Aswan and only just above the far-distant Second Cataract of the Nile. These temples were unmistakably of the Pharaonic style. As more travellers journeyed up the Nile as far as the Second Cataract and occasionally beyond, further remains, mostly of a far less impressive style and scale, continued to be found by travellers, including Thomas Legh, William Bankes, Giovanni Belzoni, Charles Irby, James Mangles and Henry Beechey, amongst others (Morkot, 2017; Usick, 2002; Clayton, 1982). Upper Egypt and Nubia became increasingly accessible, to the extent that it was even possible for an early aristocratic touring expedition, led by Lord Belmore, to reach as far as the Second Cataract, as early as 1817 – just 4 years after the famed temple discovery by Burckhardt, and at the same time that Belzoni had visited and cleared them, as described in his *Narratives* (1820).

Within a year of his arrival in Egypt in November 1821, Wilkinson had made his first trip up the Nile into Nubia/ Sudan (Thompson, 1992a); a fact which may say something about just how important and how much of a draw the question of how far ancient Egypt had extended to the south was for him and his contemporaries.[3] Such was Wilkinson’s level of interest, he devoted a third of his first book-length (but ultimately unpublished) manuscript, *Journey in Nubia; the Eastern Desert, and to Behneseh*, to the Pharaonic-era antiquities and ruins of the region.[4] With a full decade of ventures in the region already having taken place before his first trip there, Wilkinson’s work as far as Wadi Halfa cannot be regarded as truly pioneering simply in terms of the ground he covered. Rather, the

[3] I use the names Nubia/ Sudan more or less interchangeably below, as they correspond very closely. The main civilization of relevance in ancient Nubia/ Sudan was that of the kingdom of Kush. Wilkinson’s first visit to Nubia took place in 1822, according to a footnote in his *Topography of Thebes, and General View of Egypt* (1835). The same footnote also says that this was his only visit to the region, while another implies that he had been prevented from returning to the region due to illness.

[4] “Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3”, Bodleian Library Wilkinson Manuscripts, dep. c. 13.

significance of his work was that he identified – and also collated plenty of the disparate observations of his contemporary travellers – the many less aesthetically impressive, less imposing and less obvious traces of ancient Egyptian remains in Sudan into a single volume for the first time (in English), his *Topography of Thebes, and General View of Egypt* (1835). Notwithstanding the fact that much of this material was not technically new, his work was still useful at a scholarly level. To borrow a slightly oxymoronic phrase used by Bartholomew (1979: 290) to describe a highly influential geological book published in the same decade as Wilkinson's, it could be termed a "pioneering survey work". The book was also more than the sum of its parts: by collecting all of the references to Pharaonic-era antiquities to be found in northern Sudan, Wilkinson's work helped to support the growing body of evidence that there had been a sustained Egyptian presence in the region, as opposed to a more fleeting one of the sort that Hamilton had envisaged in the first decade of the 19th century.

In the rest of this chapter, attention will be given to the different facets of Wilkinson's enterprise. First, brief consideration will be given to the previously-accepted manner of proceeding: through using the Antonine Itinerary, a key classical source. Second, attention will move beyond classical sources to considering indigenous Egyptian sources, which Wilkinson was particularly keen to use, more than Hamilton and other less scholarly travellers had been, but at a similar level to contemporary travellers in the 1820s. This, divided into three parts, will form the bulk of the discussion. It will move from the obvious – large buildings with aesthetically Egyptian motifs and features, and inscriptions, for which Wilkinson acted more in the manner of collator and intellectual intermediary – to the more obscure: Wilkinson pioneered a novel way of looking at Egyptian temples to recover additional information. For these two facets, Wilkinson's two books, *Topography of Thebes, and General View of Egypt* (1835) and *Modern Egypt and Thebes* (1843) will be the principal sources. The third facet of this discussion will fast-forward three decades, to Wilkinson's return trip to Sudan, to study newly-discovered features which he seized as confirmation that ancient Egypt had extended further to the south than hitherto believed.

4.2.1 The Antonine Itinerary: a starting-point

Before the decipherment of hieroglyphs, the only textual sources available for studying ancient Egypt were classical-era ones. However, many of these, including Herodotus, Diodorus and Strabo – the three most useful – largely focused on Lower Egypt and the northernmost parts of Upper Egypt, thus leaving scholars with little to go on for regions further south. One of the more detailed classical-era sources available for investigating ancient Nubia was the Antonine Itinerary. This source essentially consisted of a record of the routes of over 200 Roman-era roads from the 2nd or c. 3rd century AD, between a myriad of settlements and military stations, throughout the Roman Empire (Reed, 1978; Sweet, 2004). Crucially, the distance between the sites was given, meaning that, if an observer followed the Itinerary on the ground carefully enough, then it was possible to match place-names in the text with ruined sites in the field. There was also the possibility for previously-unknown sites to be discovered, between previously-known ones. Although theoretically relatively simple, there were difficulties in interpreting the document: the distances given might not have been accurately copied from the original manuscript(s) and even if they were, there was confusion as to whether they recorded the shortest straight-line distance between two sites, or whether they reflected the road mileage between them, and even whether the place-names were discrete sites or larger bounded administrative provinces (Hamilton, 1809); neither of these unknowns even begins to enter into the debate over the precise function of the document (Reed, 1978). Despite these drawbacks, the source was the best fine-grained source available to scholars investigating the ancient history and sites of Nubia.

From a modern academic standpoint, the point of attempting to match the place-names mentioned in routes in the Antonine Itinerary to extant sites is not necessarily clear at first sight. Ironically however, this confusion is in part due to the success with which several generations of antiquaries and other scholars went about this research enterprise; the legacy of which consists of many – if by no means all – of the attributions still being accepted – and in some ways taken entirely for granted. The results of this enterprise might be regarded as even more impressive on the grounds that it was almost entirely uncoordinated.

Whilst it would be deeply chauvinistic to suggest that Britons or Europeans were either the first or only people to take an interest in their past (Anderson and Rojas, 2017), attempts at matching classical texts up with extant sites was an approach that was entwined in the British antiquarian tradition, extending back to the 16th century. Shortly after the Antonine Itinerary was brought to light, the antiquary Robert Talbot, an associate of John Leland, sought to correlate the places mentioned in it with towns in Britain (Sweet, 2004). The number of antiquaries who followed in his footsteps was initially restrained. However, the pace quickened in the 18th century, due to two inter-linked developments: the increasing number of antiquities being found during improvement works in rural and urban areas, and the interest by elites in the subject. The latter development resulted in patron-client relationships being established, with elite members of society supporting work by typically lower-class antiquaries. The resulting scholarship included milestones such as William Stukeley's *Itinerarium Curiosum* (1724), Alexander Gordon's *Itinerarium septentrionale* (1726), John Horsley's *Britannia Romana, or The Roman Antiquities of Britain* (1732), several new editions of Camden's *Britannia* (up to Gough's edition, in 1789) and Thomas Reynolds' *Iter Britanniarum* (1799). Attempting to definitively identify the settlements and stations of the Antonine Itinerary formed a key part of each of these projects, and it depended on fieldwork as much as research in libraries. To Horsley, the ideal level of proof required for ascribing an ancient name from the text to a place was an appropriate inscription: he presumed these were mainly found in the towns where they had first been set up, and had not been transported around the country like coins. However, notwithstanding a preference for inscribed material, antiquaries frequently had to be content with far less aesthetically impressive remains, including distinctive Roman brickwork, masonry and pottery. Literally any of these material remains could potentially be vital evidence for identifying a minor Roman site located between larger ones, especially if it lay on the course of a Roman road. Effectively, the Antonine Itinerary functioned as a framework, onto which discussion of epigraphic evidence, art and monuments could be attached.

The later 18th and early 19th centuries saw a great expansion in terms of areas investigated by classical scholars, with topographic research carried out across the whole breadth of what had once been the Roman Empire, concomitant with Britain's expanding

reach and involvement in the Mediterranean (Gunning, 2009; Holland, 2012). There was also a parallel expansion in the range of classical-era sources used. However, despite the enlarged scale, the underlying justification for the research project(s) remained the same as that which it had been several decades earlier: whereas it was obviously possible to read classical texts on their own, they made substantially more sense when the reader had even a basic knowledge of the ancient geography, such as how one city was related to another. This was recognised by many contemporaries, such as Thomas Arnold, a scholar of Thucydides and the Peloponnesian War, who essentially saw geography as synonymous with classical geography, and this as an aid for understanding ancient history; his view was shared by the military topographer William Martin Leake and the classical topographer William Gell (Koelsch, 2013; Wagstaff, 1987). Beyond military matters, research into the topography of the Holy Land, especially Palestine, was an area of considerable interest to many, and pioneers who attempted to locate sites mentioned in the Bible included Edward Robinson and Eli Smith (Silberman, 1982). The views of these men were representative of their time; this particular vision of geography was one particularly favoured by those involved in educational establishments, and in North America as well as Europe (Koelsch, 2013).

The implications of this are that, by the time Hamilton arrived in Egypt, accompanying the British army which had been sent to expel the French following its defeat at Aboukir Bay, a methodology involving attempts to match classical sources with extant sites was orthodox for antiquarian research. The Antonine Itinerary, the source which was relevant to Hamilton in Upper Egypt, had been the subject of much scholarly interest in Britain itself and as such was regarded as a trusted source for reconstructing the furthest extent of the Roman Empire in Britain, notably for the sites along Hadrian's Wall. As mentioned above, Hamilton was prevented from journeying further south, meaning that the Antonine Itinerary became not only his sole textual source, but his sole source for finding out about the ancient geography to the south of Aswan. In his brief discussion of the region, Hamilton suggested that Egypt (or Aegyptus, to follow his title) had probably reached its furthest extent in Roman times; he refused to consider any possibility that Pharaonic Egypt might have been more extensive. These were the limits of his attempt to incorporate the region into universal history.

Whilst the Antonine Itinerary had been the end-point of Hamilton's exploration of ancient Egyptian topography, it was in many ways the starting-point for Wilkinson: a notebook which Wilkinson appears to have kept while he stayed with William Gell in Naples, before he set out for Egypt in 1821, contains copious notes from Hamilton's *Aegyptiaca* (1809), especially regarding the Nubian part of the Antonine Itinerary.[5] As Wilkinson's unpublished book draft of 1822-23 makes clear, he was also keen to check Hamilton's identifications and add his own observations, even before he advanced beyond Hamilton's furthest south at Aswan.[6] However, once he passed Aswan, Wilkinson gained a new source base which Hamilton had been denied: material evidence on the ground. The significance of this dimension was crucial: as with antiquaries investigating Roman Britain, Wilkinson was able to attempt to match place-names in the Antonine Itinerary with mounds of brick and masonry on the ground. By paying careful attention to the distances between these sites, they could be linked up with the distances recorded in the Antonine Itinerary. Without a name, even a large mound of bricks and masonry could not be reconciled to universal history. With a name though, the place could be better appreciated in relation to other sites, which in turn linked into the better-known parts of the classical world, and with accounts of Roman historians of notable events that had taken place in the vicinity.

For investigation of the Antonine Itinerary to work best, ideally each successive place should have been located before the next one was sought, otherwise there was a potential worst-case scenario – if a very unlikely one – for a single mistaken identification to render all subsequent identifications incorrect.[7] The result of this was that, in order to assign large artificial mounds their proper identification, even small ones, or simply isolated pieces of Greek and Roman masonry, took on a new significance which they had not previously held: they could mark the site of a settlement listed in the Itinerary, or they could be worth noting as an alternative site to challenge an existing identification.

[5] "Extracts, notes and sketches on Egyptology, hieroglyphics and related subjects, *c*.1821", Bodleian Library Wilkinson Manuscripts, dep. e. 55.
[6] "Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3", Bodleian Library Wilkinson Manuscripts, dep. c. 13.
[7] It is unlikely that errors would have compounded as in a triangulation, as the antiquary would presumably have taken into account the missing place. However, missing out one site made the subsequent one more difficult to locate, and errors in rounding would amplify over time.

An appreciation of Wilkinson's attention to detail comes across in several examples. South of the temple at Dabod, Wilkinson noted that "a wall projecting into the river… perhaps marks the site of Tzitzi", while slightly further south, he noted that "the plain… is strewed with broken fragments of cornices, mouldings & most of them of a late date… none of these ruins can boast of an earlier date than the Caesars": this seemed to indicate the ancient town of Taphis.[8] Other examples abound: Wilkinson noted the probable location of ancient Phthicus based on "the many sculptured blocks of stone & columns", while at Serra, he deduced the location of ancient Phthamis based on the remains of an old quay.[9] In one case, at the relatively well-known island of Philae, he even noted the presence of a Greek or Roman era settlement on the grounds of pottery sherds featuring Greek script – the most obvious sign of occupation to late 19th century archaeologists such as Flinders Petrie – scattered over the ground.

It is worth reflecting for a moment on how this research enterprise differed from activities which it superficially resembles in terms of material used, but not in ultimate intent. Sweet's (2012) observation of the tendency for Grand Tourists to look out for any Greek or Roman remains, however small and unimpressive, and Gregory's (1999) classing of the activity of later 19th century British visitors as taking a "scripted" approach – that is to say that there was a mania for copying inscriptions as though they were souvenirs as opposed to for further research purposes – describe an entirely different set of cultural practices of a decidedly less scholarly nature. This remark is not intended to delegitimise their activity by any means, but merely to underline that what antiquaries such as Wilkinson (or Hamilton, had he had the opportunity) were doing needs to be conceptualised differently and on its own terms, as a concerted and systematic attempt to reconstruct ancient Egyptian geography for a specific region, based on both texts and stylistic recognition.

In addition to considering material culture to the south of Aswan, Wilkinson dramatically parted company with Hamilton by considering Pharaonic-era Egyptian

[8] "Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3", Bodleian Library Wilkinson Manuscripts, dep. c. 13, pp. 23, 25.
[9] "Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3", Bodleian Library Wilkinson Manuscripts, dep. c. 13, pp. 59, 61.

remains in the region.[10] The addition of this new indigenous source base, which is the principal focus of the remainder of this chapter, introduced a dramatic new dimension for antiquaries and other scholars. Whereas Hamilton had been interested in showing the extent of the Roman domination of northeast Africa, Wilkinson argued for a Pharaonic-era domination of northeast Africa and beyond. As has been seen, this had become a hot topic in the second half of the 1810s, immediately before his arrival in 1821. Although the Antonine Itinerary could supply very little in the way of an accompanying textual guide for sites which were relevant to this new research question, it would be a mistake to write the source off as purely decorative or relict. The source had been useful as a powerful communication tool by scholars interested in Roman Britain and this remained the case for the Egyptian context, even if Wilkinson was interested in a predecessor civilization. The perspective opened up by the Antonine Itinerary – of a civilization centred on Alexandria or Memphis (Cairo) once having controlled an empire stretching deep into Africa – survived at a cognitive level in this new enterprise: it was just a case of Rome being replaced with Pharaonic Egypt. As such, there are both continuities and discontinuities when it comes to the influence of the Antonine Itinerary on the later antiquarian research carried out by Wilkinson, and the former should not be overlooked.

4.2.2 Stylistically Pharaonic: the medium-sized temples

The most important elements of the new Pharaonic-era Egyptian source base to emerge in Upper Egypt and Nubia, alongside the discovery of Abu Simbel in 1813, were the many temples which resembled those found north of Aswan, which travellers had long ascribed to the ancient Egyptians, even when they were surrounded by, or sometimes incorporated into, later structures. With the intuitive assumption that the same Egyptian civilization which was responsible for structures to the north of Aswan had also built those to the south, there was interest in finding these smaller-scale traces of ancient Egypt in the region. Due to their sheer visibility in the landscape, Wilkinson was not the first to see any of these temples: all of the ones which he described had been

[10] Technically the "Pharaonic era" continued after the Greek and Roman conquests: the Ptolemaic dynasty retained the ancient religious iconography, as did the Roman emperors, despite most of the latter never visiting Egypt. Strictly speaking, "acculturation" would be a valid term.

visited and described by travellers before he published in 1835, and many of them had been before he even arrived in Egypt: Giovanni Belzoni and Robert Richardson (a member of Lord Belmore's party who wrote a travel account of the expedition) were two who did so (Belzoni, 1820; Richardson, 1822).

Notwithstanding his relatively late arrival in the region, Wilkinson's work was important because it was a systematic survey (as 18th and early 19th century topographic works often were) of all the known monuments to the south of Aswan, and because he applied a consistent methodology. He paid attention to hieroglyphs, and to stylistic similarities with the temples he had previously seen at Thebes and elsewhere, which entailed a close examination of the relief scenes and architectural motifs found in each temple. He would take this to a new level in his later *Manners and Customs of the Ancient Egyptians* (1837). A notable result from this approach was that Wilkinson was able to assign rough dates to many of the temples, even if he ultimately became frustrated in an attempt to arrange them in chronological order. In theory, the easiest way to date a temple was to cross-reference a royal name – which were distinguished by cartouche-enclosed hieroglyphs – with the chronologies of Egyptian kings which were being pieced together with increasing confidence over the course of the 1820s.

Hieroglyph-based dating was a method which Wilkinson relied upon on multiple occasions, but there was potential for running into difficulties: occasionally, names were too damaged to read, or they were unknown in chronologies. In these cases, stylistics provided a possible solution. From noting the distinct architectural styles and decorative motifs which occurred alongside Pharaonic buildings which were associated with a known reign, it became possible – without too much danger of circular reasoning as more examples became known – to assign rough dates based on stylistics alone, to temples which lacked an obvious king's name. Occasionally, he was even able to use stylistics as a check on hieroglyphs: he noted that one traveller ascribed a "Christian" or late Roman date to the temple at Philae, on the grounds that a hieroglyph of the Emperor Theodosius had been found there. However, the architectural style was clearly Ptolemaic. Both approaches enabled Wilkinson to date some temples, such as Beit el-Wali and Gerf

Hussein, to the Ramesside period, while he was able to ascribe others, such as Kalabsha, Debod, Dendur and Dakka, to the Graeco-Roman period.[11]

Again, Wilkinson was not the first to have achieved something like this. Other researchers active in the 1820s frequently shared their working hypotheses, and independently came to similar conclusions at the same time as a result of working on the same material: Alessandro Ricci had achieved something similar while journeying through the region with William Bankes, as had François Champollion, who journeyed to Egypt after deciphering hieroglyphs (Usick, 2002; Salvoldi, 2018; Champollion, 1868). As such, whether Wilkinson should be awarded absolute precedence for assigning a date to a given temple is not the main issue. Rather, it is important to note that his interpretive approach, based on combining hieroglyphs and stylistics, which was becoming increasingly used, was a step-change from approaches that had been common over the preceding twenty years, in which very vague aesthetic ideas alone were invoked to assign dates. At the end of the 18th and start of the 19th centuries, savants who accompanied the Napoleonic expedition concluded that the Dendera Zodiac, which was then attached to the ceiling of the Ptolemaic temple of Dendera, was over ten thousand years old, based on its apparently graceful proportions (Buchwald and Josefowicz, 2010). A decade later, William Bankes (Usick, 2002) still assigned tentative dates based on whether a monument was in good or bad condition; he assumed that the older monuments were less well-preserved as a result of wear and tear over time. Although it would not be entirely accurate to say that Wilkinson was entirely opposed to such reasoning – he subscribed to Johann Winckelmann's ideas that the rise and fall of civilizations was reflected in the style of their artwork, and he classed Roman remains as "barbarous" (1843: 313) as opposed to "chaste and simple" (1843: 295) Pharaonic ones – he gave far less weight to it than many of his predecessors. Alongside this change, Wilkinson's other contributions were to bring corroborative evidence and in a few cases revisions, and, as a reviewer of his book alongside others on the same subject noted, to make them available to a wider audience (Anon., 1835).

[11] The best reference to Wilkinson explicitly outlining his approach is actually given in a case in which he was unsure: "Neither the names nor the style of sculpture have enabled me to affix any date to this excavation" (Bodleian Library, dep. c. 13, p. 59).

In some ways, this methodology of dating structures according to their architectural style and motifs recalls Thomas Rickman's classification of British Gothic architecture as Norman/ Early English/ Decorated/ Perpendicular, published just four years before Wilkinson set out for Egypt (Sweet, 2004) – the main difference being that the date of these structures was known, whereas Wilkinson had no supporting textual evidence. As with reflecting on how Wilkinson used the Antonine Itinerary compared with later visitors to Egypt mentioned above, it is interesting to note here the relationship between this approach and those that came later. Flinders Petrie, an Egyptologist who excavated in Egypt and the Near East from the 1880s to the Second World War, is usually credited as a founder of modern archaeology on the grounds that he devised an innovative system, typology, which classed artefacts according to form, with the idea that these changed gradually, enabling a newly-uncovered artefact to be instantly ascribed a date. In many ways though, this was actually just a variant – though a clever one – on techniques which had been pioneered by Wilkinson and earlier generations of antiquaries, rather than something radically different.

4.2.3 The invisible temples of Upper Egypt and Nubia

In addition to the reasonably straightforward features of distinctive Pharaonic motifs in reliefs and architecture, and hieroglyph inscriptions, Wilkinson also referenced more complex evidence to back up his claim that the pre-Ptolemaic Ancient Egyptian state had extended far into Nubia. On repeated occasions in *Topography of Thebes, and General View of Egypt* (1835), and even more so in *Modern Egypt and Thebes* (1843), he argued that several Ptolemaic or Roman-era temples actually occupied the sites of long-vanished Pharaonic-era temples. As Wilkinson's primary aim in Upper Egypt was to show that the earlier Pharaonic state had maintained a sustained presence in this region from early times – as he put it, "the king who excavated the Temples of Abousambel [Abu Simbel], Derr, Saboua, & Girshe [Gerf Hussein], must have been *firmly settled* there" – evidence of

more Pharaonic-era temples would have backed this contention; later Ptolemaic or Roman-era temples clearly could not.[12]

Whether the concept of much older temples, which were to most observers invisible, was an innovation of Wilkinson's is open to doubt: it is not outlined to any great extent in his draft manuscript, although it is of course conceivable that he could have devised it over the course of the following decade. Instead, it appears that Champollion was the first to make the case, and he did so at some length in his *Lettres écrites d'Égypte et de Nubie en 1828 et 1829* (Champollion, 1868), a posthumously-published collection of letters which he had written to his brother while leading the Franco-Tuscan expedition in Egypt. As a result of work carried out over the preceding 15 years, Champollion was aware of the uneven distribution of Pharaonic-era and classical-era temples along the Nile: Pharaonic ones were abundant between the Delta and Thebes, and upstream of Seboua, but there was a 350km gap upstream of Thebes in which they were almost entirely absent. Here, Ptolemaic and Roman ones predominated. Champollion's solution to the problem was that, following the Persian conquest, the invading army systematically destroyed temples on its journey upstream, before it departed from the river at Seboua. This explained damage at and upstream of Thebes, as well as the survival of a cluster of New Kingdom temples between Seboua and Abu Simbel (Champollion suggested that the Persians had overlooked the temples of Beit el-Wali and Gerf Hussein which were the sole surviving ones in this region, as they were rock-cut rather than freestanding). He suggested, after the end of the Persian occupation, the Greeks and Romans (as they had eventually done for the Athenian Acropolis) had subsequently rebuilt the Egyptian temples on the same sites. Although it was highly speculative, there was evidence at several sites to support the contention: at Esneh, he recorded finding a granite door jamb inscribed with Thutmose III's name, while at Thebes he explained non-alignments as evidence of successive building stages.

Lacking any attempt at an explanation to explain the absence of Pharaonic temples between Thebes and Seboua, Wilkinson's claims were no more outlandish than

[12] "Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3", Bodleian Library Wilkinson Manuscripts, dep. c. 13, p. 35 [My italics].

Champollion's. His own fieldwork at Karnak Temple, Thebes (Luxor), recounted at length in his *Topography of Thebes, and General View of Egypt* (1835) also suggested that succeeding kings demolished the monumental temples of their predecessors, or in some cases carved their own names into earlier ones. Wilkinson provided evidence for this earlier on in the book too. On the island of Philae, he provided convincing evidence of a sort with which modern Egyptologists would be comfortable (e.g. Bryan, 2003), backing up his claim that there was an earlier temple on the same site, by saying that an attached chapel and an inscribed stone on one of the pylons featured a cartouche of Nectanebo, of the 30th Dynasty.

Further south, the most convincing evidence that Wilkinson supplied was of a similar type. He pointed to sculptured blocks with inscriptions, or more often those with parts of architectural features that he was familiar with from the Theban temples, to argue that later mainly Ptolemaic temples were built from the masonry of older ones. The most convincing of this type of evidence is actually in Wilkinson's *Modern Egypt and Thebes* vol. 2 (1843) as opposed to his book from eight years before. Describing the temple of Ibrim, which he somewhat conjecturally ascribed to the Romans, Wilkinson noted that there was an inscription of King Tirhaka, a Pharaoh who ruled a couple of centuries before the Greek-Macedonian conquest, on one of the blocks of stone incorporated into it. At the same time, he mentioned the interesting observation that it incorporated architectural fragments, such as an "Egyptian cornice", a projecting slab and what he assumed were holes for flagpoles (Wilkinson, 1843: 323).

Returning to Wilkinson's earlier work, *Topography of Thebes, and General View of Egypt* (1835), some of his claims rested on less firm ground. His reasoning for claiming that the Kalabsha temple, a building which he dated to the early years of the Roman occupation of Egypt, succeeded an earlier temple, is a case in point. Rather than highlighting any obviously earlier blocks bearing the name of kings incorporated into the structure itself, his claim that "the stones employed in its construction had belonged to an earlier edifice" appears to rest solely on "a granite statue lying near the quay before the entrance" (Wilkinson, 1835: 480). As this had Thutmose III's name "traced" on it, Wilkinson extrapolated that the same monarch had built an entire temple, which had been entirely destroyed to build the present structure.

Explaining why Wilkinson chose to make this more tenuous argument which could have been heavily censured, when he had ample evidence in the form of other Ramesside-era temples and boundary stones (see below), is an interesting question. One answer is the simple positivist one: Wilkinson was convinced by the evidence he found on his fieldwork alone that the examples he found were unproblematic. At the same time though, there is also the possibility that inscriptions and small Ramesside temples (Abu Simbel being a large exception) could only be taken so far as convincing evidence for a sustained Egyptian presence in Nubia: they did not necessarily imply outright annexation and incorporation into Egypt. Compared with Greek and Roman era temples, the latter of which had apparently been built in a relatively short time period, these mostly small-scale remains did little to help Wilkinson's image of a mighty Pharaonic-era Egypt – not least in any comparison with Graeco-Roman Egypt; Norden's images of them had not convinced Hamilton. To demonstrate his image of a powerful Pharaonic Egypt, more substantial evidence, on the same scale as the rock-cut temples of Abu Simbel was needed. Wilkinson supplied this in the form of a set of theoretical, now-vanished temples which had been built by a whole range of Pharaohs, constructed by several rulers of the 18th and 19th dynasties, and not just one great conqueror such as Ramesses II.

4.2.4 Filling in the gaps: boundary stone evidence for ancient Egypt in Nubia

Beyond classical texts and stylistically Pharaonic temples, some of the most important primary sources that Wilkinson used to build his case that ancient Egypt had extended far into Nubia were inscribed stelae, or boundary stones. In antiquity, these were frequently erected by Pharaohs to celebrate – and potentially intimidate locals if the memorials of other colonial states are considered – the reach of the Egyptian state, or, at the very least, the place that a victorious military expedition had reached (Clayton, 1994; Fletcher-Jones, 2020). As far as Egypt was concerned, one of the earliest of these to be discovered was that located on the island of Argo (Hagar e'dahab), which marked the maximum extent of Thutmose I's invasion of Nubia (Wilkinson, 1835: 472). This had been brought to Wilkinson's attention by two of his fellow scholarly travellers, Algernon Percy and Major Orlando Felix, who briefly visited Egypt in the 1820s (Thompson, 1992a; Ruffle, 1998): although Felix wrote a work on hieroglyphs, neither he nor Percy seem to have been

interested in writing up the discoveries made on their travels, so in some ways Wilkinson filled the role of intellectual intermediary in bringing their findings to a wider audience. Over the course of the 1820s, more stelae were found gradually further south, suggesting that the campaigns of succeeding kings had reached increasingly further, culminating with Thutmose IV and Amenhotep III at El Berkel, far into modern-day Sudan. In a footnote at the end of the section, Wilkinson noted that "Diodorus, Pliny, and Strabo extend the conquests of Sesostris as far as the vicinity of the modern Berbera, beyond the straits of Bab-el-Mandeb" (1835: 474). [13] Had such a statement been written two decades earlier, it would have been regarded with incredulity, by 1835 it would have been significantly more credible, even if it might have seemed slightly exaggerated.

At their most useful, boundary stones could be treated as similar to a floodmark – albeit horizontally on a map view as opposed to vertically – and be used to understand the reach of the apparently ever-expanding reach of the Pharaonic military. Ultimately though, they were more problematic for determining whether ancient Egypt had maintained a sustained presence in Nubia: they could be interpreted simply as records of fleeting visits or one-time raids. This was not Wilkinson's opinion: he implied that Herodotus was incorrect in claiming that the Egyptian king "Sesostris was the only Egyptian monarch who ruled in Ethiopia" on the grounds that "several other Pharaohs not only extended their conquests, but erected temples and other buildings in that country" (Wilkinson, 1835: 473). [14] The key word here is "ruled" and the key idea is that it is evidenced by material traces of the Egyptians' efforts to develop the country. As has already been seen, Wilkinson believed that Egyptian kings would only have built such temples – which were civic as opposed to military installations – had the Egyptians been "firmly settled" in the region.[15] As such, whilst boundary stones might have been ambiguous sources on their own, they were useful to Wilkinson because they provided

[13] Bab-el-Mandeb is the name of the entrance to the Red Sea from the south.
[14] Sesostris is not now regarded as having been a historical figure, but instead a composite representation of the militarily successful Middle Kingdom rulers.
At Wilkinson's time it was conventional to refer to Nubia/ Sudan as "Ethiopia"; this name can also be seen on other maps as referring to equatorial Africa and not the country on the Horn of Africa today. The word "Sudan" was used in a similarly broad manner, to refer to a large swathe of Saharan and sub-Saharan Africa.
[15] "Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3", Bodleian Library Wilkinson Manuscripts, dep. c. 13, p. 35.

textual records – confirming the general thrust of the classical ones – which provided assurance that the stylistically Pharaonic buildings in the region had indeed been built by a conquering state, as opposed to being the product of acculturation. However, in the event, so far as this theory of an ancient Egyptian conquest was concerned, in some ways Wilkinson need not have bothered to make such elaborate arguments. By the time of his return visit to Egypt, what seemed like firm evidence of Pharaonic Egypt's presence in Nubia, the region that Wilkinson had conjectured might have contained older Pharaonic temples at Kalabsha, Ibrim and elsewhere, was emerging in the form of gigantic brick structures in the desert either side of the upper reaches of the Nile.

4.2.5 Egyptian Border fortresses in Nubia

Wilkinson's return visit to Nubia in 1849, which took him as far as Gebel Berkel/ Napata in what is now central Sudan, took place nearly three decades after his first visit. Whereas in 1822 – as elaborated at the start of this section – the history of the western exploration of this region of the Nile valley was less than a decade old, leaving opportunities for Wilkinson and his contemporaries to uncover genuinely new sites and material, by now the ancient history of the area had been gone over much more thoroughly. Notably, there had been two enormous state-sponsored expeditions, a Franco-Tuscan one led by Champollion in 1828-1830 and a Prussian one led by Karl Lepsius, in 1842-1845 (Thompson, 2015). The result of this was that there was substantially increased knowledge of the region by the time of Wilkinson's second visit, which had uncovered entirely new features, including a series of enormous fortresses. If they were proved to be Egyptian in origin, then they would demonstrate the power of the pre-classical era Egyptian state in the region in a way that nothing else could.

The destruction of most of the fortress sites due to their submergence below the Lake Nasser reservoir in the 1960s and 1970s has meant that current research on the sites relies, with only a few exceptions, upon reinterpretation of older evidence as opposed to newly-collected data (Kemp, 2018). This, together with the lack of surviving contemporary or even classical-era documentation, has meant that the function and extent of the fortresses in the wider landscape (i.e. whether they were stand-alone or

were connected by smaller-scale structures) has been inadequately understood, although a source known as the Semna Dispatches has provided some insights as to how they functioned (Callender, 2003). Nevertheless, there is consensus that the heyday of the fortresses was in the Middle Kingdom (Bourriau, 2003). Other than this, their precise function remains controversial, with debate as to whether they were largely symbolic, or whether they were designed to protect Egypt from the newly-emerging unified kingdom of Kush to the south (Lawrence, 1965; Adams, 1977, cited in Yare, 2001; Kemp, 2018).

If anything, confusion in Wilkinson's time was even greater. If any additional contemporary evidence is required to emphasise that the "discovery" of the extent of the ancient Egyptian empire in Nubia was a complex enterprise, it can be found in the history of the scholarly investigation of these fortresses, and Wilkinson's engagement with them in particular. Before Wilkinson's arrival, William Bankes and several others visited two fortresses, Semna and Buhen, and drew images of them (Usick, 2002). In one way or another – the circle of Europeans in Egypt readily exchanged news about new discoveries – reports of fortresses in the region reached Wilkinson, and on his 1822 trip to Nubia, he drew a map of Semna with a label "Old Fortress? B[ric]k r[uin]s & Temple".[16] However, just as Hamilton dismissed the importance of Norden's report of Pharaonic temples in Nubia, Wilkinson also ultimately dismissed these reports after conducting fieldwork. He noted that "the ruins on the Eastern side of the river consist of a Temple in a platform or open area, once enclosed by a wall of crude brick or stone foundations; it may have been a "fortress", tho[ugh] I do not see any great reason for giving it that name".[17] In *Topography of Thebes, and General View of Egypt* (1835), the Nubian fortresses are a very notable absence. Only in his *Modern Egypt and Thebes* (1843) is there a vague reference to brick fortress structures between the First and Second Nile cataracts: Wilkinson noted that "opposite Dakkeh, on the east bank, are large crude brick remains, apparently of a fortress" (1843: 320), an observation which he did not develop further.

Nevertheless, once he became aware of their significance – he credited Karl Lepsius – and revisited them on his 1848-49 visit, Wilkinson made a bold attempt to describe the

[16] "Journal, 1821-2", Bodleian Library Wilkinson manuscripts, dep. d. 47, p. 55.
[17] "Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3", Bodleian Library Wilkinson Manuscripts, dep. c. 13, p. 63.

fortresses, and to ascribe them to the ancient Egyptian state (Wilkinson, 1853), and he wrote a paper focused mostly on the two fortresses at Semna, now known as Semna South and Semna West.

Wilkinson unequivocally stated that "a line of forts was constructed by the Egyptians, in very early times… to defend the frontier against the Ethiopians" (1853: 8). To preclude any objection that the reverse could be true – that they were constructed by the Nubians as defences against Egypt – Wilkinson supplied some evidence in the form of hieroglyphs on the side of one of the fortresses at Semna, which he interpreted as a record of high inundation events kept by the garrison in order to assess whether their defences could be in danger from flooding. Given that the colossal mudbrick structures had few other, if indeed any, distinctly Egyptian architectural traits which Wilkinson and others had relied heavily on for distinguishing Pharaonic-era temples beyond Aswan, the presence of hieroglyphs, presumably written by Egyptians, could be regarded as the next best thing for proving that they were actually built by the Egyptians.[18]

To further discredit the idea that anyone but the ancient Egyptian state was responsible for building the fortresses, Wilkinson next introduced an opposition argument, for the sole purpose of refuting it. He suggested that some scholars (who remain nameless) had previously suggested that the fortresses could have been built by "the shepherd invaders of Egypt" as places of "refuge", after they were forced out of Egypt at the start of the New Kingdom. This was not inconceivable: the Hyksos dynasty had allied itself with the kingdom of Kush, so surrounding Thebes (which would eventually conquer the Hyksos) to north and south (Bourriau, 2003); it could easily be imagined that the Hyksos, after being defeated, might have fled south and formed a client-state of Kush, on its border with Egypt.

Wilkinson's objection to this idea revolved around an impressive examination of the ground plan and spatial relationships of the fortresses. On the basis that both of the two fortresses were most heavily fortified on their southern side and had their "water-gate" (1853: 8), or most vulnerable point, on the northern side, with additional protection

[18] The sole piece of an Egyptian architectural element that Wilkinson found was the shape of the battlements, which he interpreted as being similar to a row of Egyptian shields.

provided by rapids in the Nile in the area chosen for the gate, Wilkinson's argument is difficult to argue with, given the underwritten assumption that the architects of the fortress were sane when it came to constructing effective fortifications. In addition to just looking at plans of the sites and how they aligned with the compass directions, and therefore their relative position in relation to the ancient Egyptian state, Wilkinson went even further. Noting that the fortresses "are so placed as to command the passes from the desert into the valley of the Nile" (1853: 8), with an additional fortress on an island in the river in case the main one fell, it is clear that he also took into consideration the broader natural topography, considering who the enemy was and where they were coming from.

The bulk of his article then discussed an array of military engineering techniques, including features such as projecting towers, counterscarps and ravelins. Wilkinson mentioned in passing that the Egyptian square towers were different from the Roman round towers, so dismantling a counter-argument that the fortresses could have been built during the brief Roman advance into Nubia. Whereas Wilkinson had earlier pointed to numerous sources of evidence – inscriptions, Ramesside-era relief scenes and vanished temples – to confirm a sustained Egyptian presence in Nubia, the realisation that immense border fortresses had also existed took conceptions of what had constituted the ultimate southern boundary of Ancient Egypt to an entirely new level. Far from being a gradual fade-out in Upper Egypt, the southern boundary of ancient Egypt was increasingly conceptualised as a heavily-fortified frontier zone, maintained in part to defend Egypt and in part as a base for further military campaigns in Nubia.

Though presumably Wilkinson's observations on topography and military engineering originated in his preparation for entering the army, this approach was frequently also a strong feature of contemporary antiquarianism. Stuart Piggott (1978) has observed that although a late 17th century plan of a Pembrokeshire hill-fort drawn by the antiquary William Jones compares unfavourably with a modern survey of the site, the older survey in fact demonstrates very good awareness of the spatial relations of the different lines of defence at the site. In contrast, Koelsch (2013) has observed that archaeology, particularly that of the 19th and early 20th centuries, is primarily concerned with reconstructing the history of sites as opposed to considering them in their wider

landscape context. It was only in the second half of the 20th century that this became a major feature of archaeological research – and more recently still where the border fortresses are concerned: many of Wilkinson's observations were effectively rediscovered anew in the late 20th century, which has again seen these sites situated in their wider landscape context (Vogel, 2010; Kemp, 2018).

4.3 The extent of ancient Egypt into Asia

As well as being interested in the extent of the ancient Egyptian New Kingdom empire to the south, into Nubia, Wilkinson was also interested in the possibility that the empire had extended in another direction: to the northeast, into Sinai, Palestine, Syria, Mesopotamia, and beyond. His reason for believing this was largely due to the same ancient Greek authors, Herodotus and Diodorus, who had attested to an ancient Egyptian conquest of Nubia: both claimed that a great warrior-king called Sesostris had also led campaigns deep into Asia.[19] Herodotus had noted that Sesostris had left distinctly-shaped boundary stones to mark his achievement; there therefore lay open the possibility that these could still exist and be used to reconstruct his advance, as those in Upper Egypt and Nubia allowed the campaigns of successive Egyptian kings to be tracked.

Wilkinson's suggestion that there had existed a mighty Egyptian empire was not entirely at odds with the authority of the Old Testament. The Egyptian state referred to in *Kings II* had not been confined to the Nile valley in northeast Africa, but had sent forces deep into Mesopotamia on several occasions. However, although it aligned with his argument, this text was not Wilkinson's primary reason for taking the stance that he did. Rather, he was influenced by classical Greek sources, especially Herodotus and Diodorus (Herodotus 2: 101-105; Diodorus 1: 53-58). Both of these authors had recounted that a mysterious Egyptian king called Sesostris had launched massive military expeditions into Asia and Europe, conquering substantial areas, and even establishing colonial settlements, including one at Colchis, on the shores of the Black Sea. Both authors also noted that Sesostris had left commemorative tablets, or inscribed boundary markers, in the

[19] Now regarded as Sesostris III, who reigned in the C19th BC; the passages in Herodotus on him are somewhat garbled.

territories that he had conquered: those with male images denoted peoples who had fought bravely, but those with female images denoted those who were deemed to have not done so.

Diodorus also suggested that there was a palace-tomb at Thebes, featuring relief scenes commemorating some of the hardest-fought battles, some of which had been against a people called the Bactrians, who had revolted against Egyptian rule and had subsequently been reconquered (1: 47-49). The Bactrians were not a people unknown to history: their home territory incorporated parts of what was then, and remains part of, Afghanistan, and they were one of the remotest people that Alexander the Great had subdued (Tarn, 1951; Holt, 1999; Holt, 2012). Towards the end of his rule, Alexander himself had faced and ended a rebellion launched by Greeks whom he had settled in Bactria (which possibly explains Diodorus's interest in this earlier phase of conquest), but quite why they featured on ancient Egyptian monuments was a mystery. Whilst Diodorus's description of the dimensions and structure of this monument were not easy to match up with any standing monuments, it was nevertheless detailed enough to investigate further: he referred to scenes of marching soldiers, an Egyptian king with attendant lion (either a pet or a symbol of the monarch's courage), mutilated prisoners, the spoils of war, religious offerings and much else.

Overall, this monument, if it could be identified, presented an interesting opportunity: it potentially held information about the nature and location of the war, involving a very unexpected people, that could be accessed by a researcher based in Egypt, as Wilkinson essentially was for over a decade. This was in contrast to the boundary stones: even if the unsavourily-decorated boundary stones had survived defacement or outright destruction by local people in the intervening three millennia, they probably would have been too widely-scattered for a single investigator to try and locate, especially given the fairly vague description given of them (Ober, 1995). In any case, these boundary stones were inaccessible to Wilkinson: he had planned a trip to Sinai – for which he had gone as far as to copy a map – and possibly beyond.[20] However, he was forced to abandon this, as a

[20] "Rolled papers, n.d. (c. 1821-33?)", Bodleian Library, Wilkinson manuscripts, dep. b. 8(R).

result of the unstable condition of what had formerly been a highly prosperous region, caused by Muhammad Ali's invasion of the Ottoman Empire's Syrian territory.

To Wilkinson, and to his contemporaries such as Burton and Champollion, the two leading candidates for the building that had impressed Diodorus were the Memnonium or Ramesseum (built by Ramesses II) and Medinet Habu (built by Ramesses III). These were the two most prominent temples on the western bank of Thebes, which survived in relatively good condition. Both had been built to memorialise successful campaigns of these respective Pharaohs against enemies of the Egyptian state, with images of the victorious Egyptian kings and their vanquished foes, along with those of battle scenes and religious rituals (Murnane, 1980; Wilkinson, 2000).

Wilkinson's approach to these monuments was singular. Effectively denied access to Sesostris's boundary stones as a source for reconstructing the extent of the king's conquests, he not only attempted to match the relief scenes to Diodorus's description. He additionally attempted to extract spatial information directly from the monumental relief scenes, so effectively mined the reliefs for information on topography in lieu of the real thing. In doing so, Wilkinson was not entirely unique: both Robert Richardson (1822) and his mentor W. R. Hamilton (1809) had attempted to interpret the scenes, but neither went as far as Wilkinson in the conclusions they drew. Wilkinson's approach was also a typically antiquarian endeavour: he treated ancient texts and field monuments (or artefacts) as equally important sources, with the end intention of writing a political-military history of ancient Egypt, to correct the greatly conflicting Egyptian chronology that was known from Herodotus, Diodorus and Manetho (in Africanus and Eusebius). As will be seen though, Wilkinson pieced together a narrative which went much further and established a dynamic for several centuries of ancient Egyptian history, besides ascertaining the ultimate reason for why the conquests were made.

4.3.1 Making Images into History

Wilkinson's desire to convince his readers that the Ramesseum and Medinet Habu relief scenes, which will be discussed below, had been created to recount the history of wars fought in Asia, is evident from his drawing attention to them in the introduction to

Topography of Thebes, and General View of Egypt (1835), and then his inclusion of dozens of pages of discussion in the very first (hundred-page) chapter of his book. However, he was forced to address a potential issue: pictures carved on stone blocks were not widely viewed as the ideal source base for writing history. Essentially, Wilkinson faced an issue for which earlier antiquaries had been caricatured (Sweet, 2004; Hill, 2021): his source base was unusual, and it was in his interest to present it as incontrovertible. Wilkinson was unequivocal in making his case, asserting that "monumental records… deserve as great a claim to credit as any national history" (1835: xix). He maintained that the images in question were straightforward to interpret, and if this was accepted, it was not possible "to consider their sculpture much inferior to the annals of an historian, except in extent and minuteness of detail" (1835: xix). In making this suggestion, he was in fact echoing Hamilton, who had declared "the monuments are nearly the only books from which much useful information can be derived" (1809: 58). Wilkinson saw his chosen source simply as providing "additional information", which had simply not been written up in the traditional way before, due to a conscious decision of the ancient Egyptians to record their achievements in visual format, as opposed to chronicles or epic poetry (1835: xix). To convince any of his readers who remained sceptical, Wilkinson urged that, had Rome produced no historians, then the greatness of the Roman Empire would have been difficult to grasp. With understanding of hieroglyphs still inadequate – which Wilkinson alluded to in his introduction – it could also be inferred that the intellectual world could be waiting for a long time if it was only prepared to accept the most familiar of sources, written histories. In the meantime, there were other sources which could be drawn upon to fill the gap, and these were waiting to be read.

Although Wilkinson derived many ideas from the monumental relief scenes in the 1820s, his case for the validity of this source base was helped by Champollion's *Lettres* (1868), which became accessible to him between the publication of his *Materia Hieroglyphica* (1828) and *Topography of Thebes, and General View of Egypt* (1835). Both Wilkinson and Champollion had been impressed with the quality of ancient Egyptian art, which they regarded even more highly than that of ancient Greece. Both admitted this privately (although Champollion's letters were posthumously published), and Wilkinson took a very sympathetic approach toward Egyptian art, which frequently lacked the key

dimension of perspective. In Upper Egypt, faced with an outsize figure that seemed to break from European artistic convention in several ways, he noted

> their reason for so introducing him is very natural: in order to represent the extensive scene of a battle, or even of a war, they were forced to make the figures small, while in order that their hero might be conspicuous, they allowed nothing to interfere with him, & left the rest in the distance, but whenever he seizes with his hand a captive foe, there will be found little or no difference of size between the 2 figures. Another proof of this is, that the arrows, which reach the enemy, are much smaller than those he holds in his hand.[21]

However, few of Wilkinson's or Champollion's contemporaries would have shared this opinion: Stephanie Moser (2006) has noted that the British Museum probably would not have purchased a collection of Egyptian antiquities had it been given the option of buying them as opposed to being presented them by King George III. Faced with such potential lack of interest in ancient Egyptian art, Champollion ingeniously referred to the relief scenes, which had vestiges of paint, at the Ramesseum and Medinet Habu as "historical paintings". Starting with a "premier tableau", these could be read as narratives of battles. Though Wilkinson had come to the conclusion that the relief scenes could be read in such a way before Champollion arrived in Egypt, he eagerly adopted Champollion's term; previously he had referred to the relief scenes simply as "sculptures". In his *Topography of Thebes, and General View of Egypt* (1835), he delighted in noting roof projections, as if they had been added to protect the scenes from rain. Effectively, ancient Egyptian images were being subjected to the same way of seeing as that of the audiences who viewed the increasingly popular historically-themed paintings on show at the Royal Academy (Morris and Milner, 1993; Smiles 1994). By presenting the relief scenes in this way, both men were able to place them among the ranks of other visual sources which antiquaries had interpreted as narratives, such as the Parthenon frieze, Trajan's Column and the Bayeux Tapestry – the last two being historical narratives of Rome's Dacian Wars and the Norman

[21] "Draft, entitled 'Journey in Nubia, the Eastern Desert, & to Bhnesh [sic] by J.G.W.', *c*.1821-3", Bodleian Library Wilkinson Manuscripts, dep. c. 13, p. 35.

Conquest of England (Haskell, 1993; Hill, 2021) – which had likely helped to condition the way in which they viewed the Egyptian scenes in the first place.

4.3.2 Ancient Egypt Illustrated I: The Ramesseum

The first set of reliefs that Wilkinson discussed were those on the walls of the Ramesseum, then called the Memnonium, a ruined temple on the west bank of Thebes which was better known for possessing a fallen colossus, one of the largest statues in Egypt. The decade before Wilkinson, the adventurer, scholar and antiquities-dealer, Giovanni Belzoni, had taken the uppermost part of another statue from the site; by this time, it was on display in the British Museum in London. One or possibly both of these huge statues had inspired Shelley to write his poem, *Ozymandias* (1818), meaning that one facet of the site had entered the public consciousness, even if the rest of the temple or even its name was practically unknown to most Britons. However, in addition to the statues, the Ramesseum also featured relief scenes, carved on the colossal blocks of stone which formed its walls, or what was left of them; these were the features that Wilkinson devoted his time and space to describing, in a lengthy section at the start of *Topography of Thebes, and General View of Egypt* (1835: 50-75).

The first half of Wilkinson's description summarised the remains of the scenes, starting with those on the north side of the entrance pylons. Wilkinson recounted a series of lively scenes to the reader, including an army marching in the field, captured towns, the punishment of bound captives, and the victorious king with the spoils of war. The lack of illustrations would have forced the contemporary reader to use their imagination and rely on their existing knowledge of other ancient sieges; Homer's descriptions of Troy in the *Iliad* probably would have loomed large in the minds of many (Davies, 2018; Gange and Davies, 2018). Wilkinson's inclusion of several references to Homer and the *Iliad*, going as far as suggesting that the poet might have been inspired by the scenes to write his evocative battle passages (1835: 19), took full advantage of this lack of familiarity: the public had been somewhat indifferent toward the aesthetic qualities of Egyptian antiquities which had recently been brought to Britain during the Napoleonic wars and

might have been less willing to believe Wilkinson had they seen the actual relief scenes (Leask, 2002; Moser, 2006).[22]

Another parallel with the Trojan War was that the enemy facing the Egyptians was an advanced one. Wilkinson, like many of his contemporaries, saw hunter-gatherers and pastoral nomads as less advanced than more settled agriculturalists and city-dwellers (Meek, 1976). The enemies of the Egyptians, with their walled towns (1835: 17), clearly fell into the latter category, as Wilkinson was pleased to point out at this stage: he carefully noted other distinguishing points of high civilization, including coinage, although he was unable to ascertain whether they were sufficiently technologically advanced to possess the arch: the Egyptians depicted their bridges from above, making it impossible to know anything about their structure (1835: 19).

If Wilkinson was disappointed in relation to such a specific detail, it was largely because he had been sufficiently impressed by the reliefs to form very high expectations of the sort of information that he could expect to extract from them. Wilkinson's initial commentary was similar to that which Hamilton and Champollion provided: all saw the scenes as depicting each manoeuvre of the army over the course of an engagement:

> In order to check the approach of the Egyptians, the enemy had crossed the river, whose stream, diverted into a double fosse, surrounded the towered walls of their fortified city, and opposed their advance by a considerable body of chariots; while a large reserve of infantry, having crossed the bridges, was posted on the other bank, to cover the retreat or second their advance; but, routed by the Egyptian invaders, they are forced to throw themselves back upon the town, and many, in recrossing the river, are either carried away by the stream, or fall under the arrows of the advancing conqueror. Those who have succeeded reaching the opposite bank are rescued by their friends, who, drawn up in three phalanxes, witness the defeat of their comrades, and the flight of the remainder of their chariots. Some carry to the rear the lifeless corpse of their chief, who was drowned in the river, and in vain endeavour to restore life, by holding the head

[22] Wilkinson himself helped to change views on Egyptian art with *Manners and Customs* (1837) and its many illustrations.

> downwards to expel the water; and others implore the clemency of the victor and acknowledge him their conqueror and lord (1835: 19-20).

Effectively Wilkinson attempted to put the events into a connected sequence, almost like a modern comic, graphic novel, or even the Bayeux Tapestry. At this stage he had either not fully developed his own way of reading reliefs, or chose not to do so, though he later used it to great effect, as will be seen below. In yet another scene, Wilkinson described the siege of a hilltop town in similarly exquisite detail, with the story unfolding in front of him (*Manners and Customs*, 1837, vol. 1: 362).

Despite the wealth of information about the course of the battle, a key detail remained elusive: Wilkinson was unable to ascertain the identity of the enemy. Whilst the names of captives and towns were frequently inscribed in hieroglyphs, often for each depiction, they could not be matched up with tribes or towns in southwest Asia with any certainty. As he noted in his book, the ancient toponyms of the region were still inadequately known and only further investigation could reveal more – Wilkinson was again forced to turn to consider other details. Informing his readers that he would not "take it for granted the Bactrians are here represented" (1835: 22) – the thesis he had advanced at the start of his book – he advanced a cautious argument that the battles had taken place outside the Nile Delta, on the grounds that, even during the reign of Necho, long after the height of Pharaonic Egypt's power, the Egyptian empire had extended across southwest Asia, to the Euphrates. Extrapolating backward from this, the ancient extent of the empire during the New Kingdom could conceivably, or at least intuitively should have, been much greater; given such a state of affairs, it would seem unlikely that the enemy was an invading army raiding Egyptian territory.

This dimension of Wilkinson's argument was highly conjectural, but he also referred to more empirical evidence. Somewhat strangely, he only referred readers to his earlier, and by then less accessible work, *Materia Hieroglyphica* (1828), to read an explanation of how the Ramesseum scenes could not have taken place in the Nile Delta – the part of Egypt that an invading army would first reach, and where the Egyptians would have been expected to attempt to repulse them. In this, he had pointed out that the hills and woods shown in the Ramesseum reliefs were wholly atypical of the flat and monotonous Nile Delta landscape: this could not have been the location of the battle. Some aspects of this

interesting observation – notably the descriptions of scenes of Egyptian troops laying siege to a fortified hill town – featured in *Topography of Thebes and General View of Egypt*, although strangely Wilkinson omitted all mention of the specific point, not even including a brief summary of his earlier reasoning, bar noting the specific page numbers of his earlier work.

More straightforwardly for his readers, and in many ways worthy of his approach in *Manners and Customs* (1837), whereby he extracted individual scenes, people and items from tomb paintings and other sources, Wilkinson noted that the prisoners captured by the Egyptians were portrayed in "a much lighter hue that the inhabitants of the Nile" (Wilkinson, 1835: 23-24). They were also shown in stylised foreign costume: it seemed to him that the ancient Egyptians saw the enemy as a different race from themselves. Wilkinson assured his readers that this as-yet unidentified foreign people were similar to the Assyrians portrayed on the monuments of the later Pharaoh, Tirhaka – it was an identification that could be believable, in the absence of anything more convincing. This was not in itself proof of the Egyptian army having fought in Assyria – let alone Bactria which Wilkinson distanced himself from at this point – but, coupled with the observations on the geography of the area, and the argument that New Kingdom Egypt must have been more powerful, it was an additional piece of evidence in favour of a campaign which had taken place some distance from the Nile valley.

4.3.3 Ancient Egypt Illustrated II: Medinet Habu

If Wilkinson was reluctant to fully commit himself into ascribing the reliefs at the Ramesseum to a military campaign in Central Asia along the lines suggested by Herodotus and Diodorus, he was notably more strident in his discussion of another set of reliefs, those of Medinet Habu. This palace-temple complex, built by the Pharaoh Ramesses III, a successor to Ramesses II, was similar to the Ramesseum, with reliefs of battle scenes carved on its walls (Murnane, 1980; Wilkinson, 2000). It was also situated close to it, on the edge of the desert, immediately above the cultivated land, on the west bank of Thebes. As such, Wilkinson discussed it after several pages describing the temples that lay between the two. However, there was one significant difference between the two:

whilst much of the Ramesseum had been destroyed, leaving only parts of the front pylon and several walls of inscribed blocks, the central monolithic part of Medinet Habu was very well-preserved. Wilkinson took full advantage of this to try out a mode of interpretation that he had only briefly been able to accomplish at the Ramesseum, namely, to reading multiple scenes as following on from each other, like a graphic novel, thus reconstructing a detailed history of the whole war.

After describing scenes of a religious nature, Wilkinson announced that "the commencement of the interesting historical subjects of Medeenet Haboo is in the south-west corner of this court, on the inner face of the tower" (1835: 65). The first scene featured Ramesses III leading the Egyptian chariotry against the enemy, shooting arrows at them, along with bound captives. Wilkinson saw the reliefs on the west wall as "a continuation of the scene" (1835: 66): here the hands of the captives were tallied, to measure the scale of the victory. Like at the Ramesseum, the name of the enemy tribe was recorded in hieroglyphs, here it was "the Rebo". To Wilkinson, the name recalled "the Rhibii of Ptolemy" (1835: 66). Unlike at the Ramesseum, it was possible to assign this people a rough geographical location, based on the information provided by the same ancient author: they had lived "near the Caspian and the north bank of the Oxus". The appearance of the people also seemed to back up this identification. Wilkinson described the people as wearing "flowing dresses, striped horizontally with blue or green bands on a white ground", while their physiological features consisted of "long hair" and "aquiline nose[s]" (1835: 66). This was in contrast to the depiction of Egyptian men, with their short hair and loincloths; there seemed to be congruence with the classical description.

For the present though, Wilkinson made no further connections between the reliefs and the purported extent of the ancient Egyptian empire in the classical sources – in fact, he noted that he had no intention to discuss the identification in greater detail – in a manner reminiscent of his distancing himself from them in his description of the Ramesseum reliefs – and he instead turned to subsequent scenes showing Ramesses III's return to Egypt, with great acclaim. A potential explanation for his persistent refusal to go the whole way may be that Wilkinson thought it would be better to introduce his novel finding – that the much-doubted passages in Herodotus and Diodorus were correct and that the Egyptian armies had extended far into Central Asia on at least one occasion – in

stages rather than all at once. Seen in this light, Wilkinson's passage on the reliefs in the Ramesseum was a "testing the waters" exercise, intended to establish the validity of a methodology, based on a source that his audience was likely unused to. Leaving the implications to one side for the moment, he persuaded readers to accept the validity of the methodological approach, without critiquing it on the grounds of pre-existing prejudices or sheer disbelief. Only with the audience accepting, or at least having previously been exposed to his novel methodology, did he turn to taking it to its logical conclusion, for the scenes on the outside walls of Medinet Habu.

On these walls, Wilkinson described another battle scene, which is by far the longest, and demonstrates his attempt to trace the chronology of a campaign by reading reliefs as sequential. The first of these featured Ramesses III in a chariot, with attendants. In the "second compartment", there was a similar scene of a marching army. However, Wilkinson attached a new meaning to it: he took the repetition of the scene to imply that the Egyptian army had marched a considerable distance before engaging with the enemy. The "third compartment" featured a battle in which Egyptian archers and chariots, alongside their "Asiatic allies", triumphed over the Rebo; Wilkinson did not spare his readers a description of the Egyptians collecting grisly war trophies, including severed hands, in addition to the enemy's weaponry. However, the campaign carried on, and the Egyptians continued to march further into enemy country, an observation that Wilkinson reached on the basis that Ramesses III, in a chariot, was still armed with "bow and sword in one hand and his whip in the other" (1835: 70).

The ensuing battle represented the climax of the campaign. Before detailing an account of the battle, Wilkinson noted the identity of the enemy. Champollion had assumed they were a people called "the Fekkaros", but this reading of the hieroglyphs made no sense to Wilkinson, as there was no trace of an "F" sound in their name. Instead, Wilkinson suggested that the leftover syllables resembled Ptolemy's "Tochari", a people who inhabited lands to the northeast of Bactria, close to the Rhibii; he noted that "if any of the sculptures of Thebes refer to the rebellion of the Bactrians, they are here" (1835: 70). The battle narrative featured the same chariots moving in all directions as normal, but quickly became faster paced. After the Egyptians and their native allies, the Shairetana and Sha, routed the enemy, identified by Wilkinson as the Tochari, they chased them in their

retreat, further into Asia. Surprisingly, the next panel consisted of an epic naval battle, which Wilkinson interpreted in great detail as usual. Central Asia was not near the sea and at this point Wilkinson chose to furnish his readers with much more precise topographical information: “the lake here represented is probably that of the Oxus [Amu Darya] and Iaxartes [Syr Darya], to the east of the Caspian, and now called the Aral Sea”.

The preciseness of the identification of the water body, and even the mention of an ancient Egyptian presence in Central Asia – in territory that was then being contested by the Russian Empire, closer to China than to Europe, Africa or the Near East – is undoubtedly startling but, given the build-up, not excessive; this is also the case due to another key factor. Wilkinson’s paradoxical attempt to extract geographical information, while simultaneously withholding it from the reader until the last moment, is more convincing than it otherwise would be because his narrative gradually leads the reader toward the locality, culminating with the presence of the Tochari, who Ptolemy placed as bordering China. With this people enabling the campaign to be pinned to a point on the map – the assumption of course being that they were not the invading empire striking into Egypt, which Wilkinson could safely dismiss given the absence of any information whatsoever for such a scenario – the identification of the water body as the Aral Sea (rather than one of the other large water bodies closer to Egypt, such as Lake Menzaleh, the Mediterranean, the Red Sea, the Black Sea, Lake Van or the Caspian Sea) was made more believable, as it was the closest: effectively, the two identifications could reinforce each other, independently of the underlying thesis that Egypt under the New Kingdom Pharaohs must have extended further than it did in the less prosperous times that followed.[23]

After the climactic claims on the location of the battle, Wilkinson drew his account of the campaign to a close and described the king returning to Egypt in triumph.

[23] Whether the non-specialist reader would have thought in such detail is doubtful; an otherwise-attentive reviewer (Anon., 1835) did not do so.

4.3.4 The Origins of an Empire: Egyptians, Hebrews, Assyrians and Shepherd Kings

Whereas Wilkinson had described the relief scenes in exquisite detail, he made only a rudimentary attempt to assemble them into the historical narrative that he had promised at the start of *Topography of Thebes, and General View of Egypt* (1835): the topographical approach did not really allow it. As has been seen, he mainly confined himself to using them to reconstruct an ancient geography, that of the route of a campaign to a specific enemy territory, and back again. However, in two of his other books, his *Materia Hieroglyphica* – published as early as 1828, just over halfway through his sojourn in Egypt – and *Manners and Customs of the Ancient Egyptians* (volume 1, 1837), he went into considerably more detail, providing information about when the foreign conquests happened, by whom they were led and fought against, and, perhaps most importantly, why – these were questions that the classical sources had answered imperfectly, or left unaddressed.

Wilkinson rehearsed many ideas in his earlier work, *Materia Hieroglyphica*, but his fullest exploration of them and their implications, are in the first volume of his *Manners and Customs* (1837), in which he attempted to establish a chronology of Egyptian rulers, and as such most of the discussion below, except where otherwise specified, will be devoted to this later work. In this chronological section, Wilkinson attempted to reconcile the chronologies provided by Herodotus, Diodorus and the fragmentary works of Manetho (which were preserved in Josephus), with the monumental evidence, including a newly-discovered king-list from Abydos. The end result was an inspired, if at times very conjectural and openly in-progress (in *Manners and Customs*, he freely admitted that his earlier thoughts had been incorrect on several occasions), attempt to reconstruct Egypt's history by incorporating indigenous Egyptian sources, as opposed to mainly relying on classical ones. Throughout this chronological section, the theme of Egyptian conquests outside the Nile valley is a constant, and one that even influenced his chronology.

In a nutshell, Wilkinson assumed that each Egyptian Pharaoh spent the first few years of their reign on campaign, before returning to Egypt, where they built commemorative monuments with their spoils of war – the Ramesseum and Medinet Habu were two of the best-preserved of these monuments, but they were far from being the only ones. However, on closer examination, all of these monuments – or at least the ones which

were inscribed – appeared to date only from the latest dynasties: no Pharaohs before the 17th dynasty were represented. Total destruction of the earlier ones was a possibility – Wilkinson witnessed the dismantling of several Pharaonic temples for building stone while resident in Egypt – but he became convinced that there was another reason. Namely, Egypt had initially been relatively peaceful and self-contained, but in its later history it became a highly militaristic society that had subdued its neighbours.

Ironically, Wilkinson came to believe that the catalyst for the growth of the Egyptian empire was an earlier invasion of Egypt itself, by a mysterious group referred to by classical authors as the "Shepherd kings" or "Pastor kings", apparently due to their distinctive mode of life – in contrast to the agriculture-based subsistence strategy that most Egyptians followed.[24] Josephus recorded that this group had successfully conquered Egypt, and he referred to Manetho, who recorded several dynasties of this people, and several dozen kings, who had ruled Egypt, or at least the Nile Delta region in the north, for several centuries. Eventually though, the Egyptians had risen against them and forced them to leave Egypt. In many ways, this represented the beginning of a narrative rather than the end of it: Wilkinson suggested that the leaders of the revolt

> having been roused by the aggressions of the shepherds, and unwilling, perhaps unable, after their expulsion, to restrain the natural impetuosity of the Egyptian troops, sought, in the conquest of neighbouring countries, that glory which was henceforward to ennoble them, and secure a lasting reputation (*Materia Hieroglyphica*, 1828: 73).

Effectively, a local campaign against a specific enemy was transformed into an attempt to dominate much of the then-known wider world. There is the possibility that Wilkinson was inspired in part by recent events: in the first decades of the 19th century, Muhammad Ali had risen to power in Egypt in part due to his army of Albanian troops. Faced with an unruly army, he had sent many of them abroad to conquer Arabia (Dodwell, 1931; Sayyid-Marsot, 1984; Sayyid-Marsot, 2007). After this met with success, he kept going and was only eventually contained in the late 1820s by western intervention which resulted in the creation of an independent Greek state on the southern tip of the Balkan peninsula.

[24] Now known as the Hyksos.

Modern-day politics might well have helped Wilkinson at a cognitive level to make the classical texts believable. When combined with the monumental record which featured peoples with physiological features of people from southwest Asia, clothed in exquisite dress that was apparently atypical of that worn in Egypt, an observer might easily have been tempted to place the location of the wars outside of, rather than within, the Nile valley.

If Wilkinson was certain that the Egyptians had waged a war in Asia that had perhaps reached as far as Bactria, he was less settled on precisely whom it had been fought against. Ironically, despite their prominence in the textual record and in Wilkinson's narrative, there were no known battle scenes which depicted the Shepherd kings themselves: there were only scenes showing the enemy that the Egyptians faced at a later date, after liberating themselves from the Shepherd kings. Josephus had equated the Shepherd kings with the Hebrews, and this was an association that scholars, keen to investigate a subject that had relevance to Biblical history but which had been covered all-too briefly in *Exodus*, were happy to accept. In *Materia Hieroglyphica*, Wilkinson also adhered to this. He reconciled the main point of conflict between the Biblical narrative and Josephus – that the Hebrews had been oppressed by the Egyptians rather than acting as the oppressors – by suggesting that the phase of oppression, better-known as the infamous Hebrew bondage, had occurred after some of the Shepherd kings/ Hebrews had been left behind in Egypt, following the defeat and flight of the Shepherd kings who had once reigned as overlords. (1828: 81-82).

However, by the time he published *Manners and Customs*, Wilkinson had changed his mind and concluded that the Shepherd kings were not the Hebrews. Instead, he suggested they were "viceroys" left by the Assyrians, an identification he had narrowly dismissed in *Materia Hieroglyphica*. Wilkinson's stated reason for changing his mind was that Assyria was the only "nation [that] had sufficient power to obtain possession of Egypt at so remote an era" (Wilkinson, 1837, vol. 1: 38). Like Egypt, Assyria's economy had been based on agriculture – the somewhat unruly Tigris in place of the Nile – which, as has briefly been alluded to, was believed to be the foundation for Egypt's great power. By contrast, the Bible recorded that the Hebrews had been relatively weak even when they had possessed their own state. In substituting this state, Wilkinson set up an image

of two great powers clashing in the depths of ancient history, which was in greater accord with his belief that ancient Egypt had been a mighty power. Wilkinson's choice of Assyria was by no means a random nod to keep his narrative relevant to scriptural history: there was a passage in Diodorus that seemed to indicate that Egypt had been conquered by an Assyrian queen, Semiramis, whose successors had been driven out after a revolt, which again recalled the Egyptian revolt against the Shepherd kings. It may be of more than passing relevance to note that Semiramis, like Alexander the Great's wife Roxanne, was originally a Bactrian princess, taken by King Ninus of Assyria during his conquest of the region. Since the clash between Necho's Egypt and Nebuchadnezzar's Babylon (which had succeeded the Neo-Assyrian empire as the paramount power in Mesopotamia) of a millennium later, recorded in the Bible, had resulted in Egypt surrendering all of its Asian territories to Assyria, the idea that the precise opposite series of events had occurred in an earlier age was not necessarily outlandish. If it had occurred, Bactria, already part of the Assyrian empire, might have fallen easily to the Egyptians.

The new identification was also in better accordance with the monumental record: images of tribute-bearers with exotic species, such as leopards, must have come from Asia – Wilkinson had earlier been forced to make an overly-complicated argument that the conquered Hebrews, rather than paying tribute in goods from their own country, had merely acted as middlemen, bringing in goods that they had sourced from elsewhere. Had Wilkinson been writing a decade later, he would have been able to cite the scholarly investigation of the Assyrian sites of Khorsabad, Kuyunjik and Nimrud by Paul Emile Botta and Austen Henry Layard: both men had uncovered fortified cities of the sort that Wilkinson saw at the Ramesseum and Medinet Habu (Lloyd, 1980; Stiebing, 1993). The only disadvantage with the new identification with the Assyrians was that the straightforward explanation that the title "Shepherd kings" or "Pastor kings" described a pastoral mode of life was unfortunately severed: evidently Wilkinson believed that the balance of evidence was such that this was a technicality of limited importance at this point.

If the Shepherd kings, now identified as Assyrians, were an unknown quantity at a visual level in Egyptian history, this was not true of the unnamed Asian people whom the Egyptians soon encountered as an enemy after their march into Asia. The earliest

inscribed monuments that he was aware of – and therefore those that he believed were closest in time to the Shepherd kings – were those associated with a king called Osirtasen I. Some of the most impressive of these were in the cave-tombs of Beni Hasan, located on the otherwise largely unremarkable stretch of the Nile midway between Cairo and Thebes/ Luxor. These featured paintings which appeared to show prisoners of war. Wilkinson was unsure where they originated: noting their appearance, he felt "rather inclined to consider them from some Asiatic country" (1835: 376). By the time of writing *Manners and Customs* (1837), perhaps having further reflected on his chronology in the meantime, Wilkinson decided to enlist them as among the first to have faced the Egyptian army's advance and subsequently brought back to Egypt as captives.

As well as the scenes of prisoners of war, Wilkinson noted other scenes at Beni Hasan. These showed potters, glass-makers, cabinet-makers, chess or draught-players and a vaulted arch structure, and were located behind "grand and chaste" architecture which Wilkinson took to have been the inspiration for the Greek Doric. All of this seemed to indicate that Egypt at this time had reached an advanced stage of development. To Wilkinson, this seemed to contradict Manetho's claim that, during the 17th Dynasty, when Osirtasen was supposed to have reigned, Egypt had been split between that king in the south, and a dynasty of Shepherd kings in the north. To him, the impressive state of the arts in Egypt and the monumental building programme could only have resulted from a lengthy phase of cultural development, which must have taken place after the departure of the Shepherd Kings – he did not consider the Shepherd kings as possessing an advanced culture which the Egyptians might then have borrowed from. However, this argument had a side-effect: Wilkinson was forced to argue that the Graeco-Egyptian historian Manetho had mistakenly placed later dynasties at an earlier date than they were in reality (1837, vol. 1: 36).

On one of his excursions into the Eastern Desert with his friend James Burton, Wilkinson had come across further evidence to suggest that there had been an Egyptian presence abroad at a very early date, in the form of a monumental tablet at Wadee Jasoos. This had been set up in the 28th year of the reign of a king Amun-m-gori II, who had reigned between Osirtasen I and Osirtasen II. It commemorated "the wars with the foreigners of Pount" (1837, vol. 1: 45), a group whom Wilkinson had noted on later monuments; from

this he concluded that Egypt's long war in Asia had already commenced by the reign of this king. As with the developed state of Egypt's arts and manufactures that he had noted, the tablet seemed to confirm Wilkinson's supposition that the Shepherd Kings had been expelled long before Osirtasen I's reign, as otherwise it was hard to explain why the Theban-based Egyptians would have been at war with a foreign enemy power – especially if it were based in the north, as the Shepherd kings would have been between them as a buffer state and barrier to supply the front lines.

Between Osirtasen II and Thutmose I – the next Pharaoh to have commissioned relief scenes celebrating martial progress – there was a gap of over 150 years. Rather than impeding the construction of a historical narrative, the gap actually added nuance: Wilkinson used it to explain the role of the Hebrews in ancient Egyptian history. Given that many scholars were interested in Egypt because of its Biblical associations, Wilkinson could not have left them out even had he wanted to – although in the event, the evidence suggests that they was more useful than inconvenient to him. Wilkinson argued that this group had entered Egypt during the reign of Amosis (Ahmose), during which time it increased the size of the Egyptian labour pool. Later on, he conjectured that the Egyptians became concerned that the Hebrews might join forces with the Asian enemy that the country was in the midst of waging a long war against. Consequently, the Egyptians began to treat the Hebrews as if they were prisoners of war. This was the infamous Hebrew bondage. Whilst the Bible emphasised the impact of this on the Hebrews, as it was written from their perspective, Wilkinson argued that enforcing these conditions on the Hebrews was detrimental to the Egyptian economy as well, and resulted in the military being confined within Egypt's borders. However, following the Exodus, and with (what Wilkinson envisaged as) the burden of oppression lifted, Egypt was free to resume its war in Asia, after over a century of inactivity in this region. The explanation was an inspired one, but also opportunistic: in *Materia Hieroglyphica*, Wilkinson had suggested that the renewal of the military campaign was due to the expulsion of the Shepherd kings. Having moved the expulsion of this group to several centuries beforehand, Wilkinson effectively seized the flight of the Hebrews to fill what was otherwise an inconvenient gap in his historical narrative.

The new war was conducted by the Pharaoh Thutmose III. His reign, "one of the most remarkable" (1837, vol. 1: 53) in Egyptian history, saw, according to his monumental reliefs, expeditions against the "Kufa", the "Rot-n-no", the "Pount", and the Ethiopians – at least the first two were Asian peoples whom Wilkinson was already aware of, from the Ramesseum and Medinet Habu. That Thutmose III's campaigns had been successful was not in doubt, as a scene in his tomb in the Valley of the Kings at Thebes (KV34) showed representatives from these states bringing tribute to him. Wilkinson included a fold-out reproduction of this scene at the end of *Manners and Customs*, the only one in the first volume, which perhaps gives an indication of how important he viewed this theme ((Fig. 4.1); see also 1835: 153).

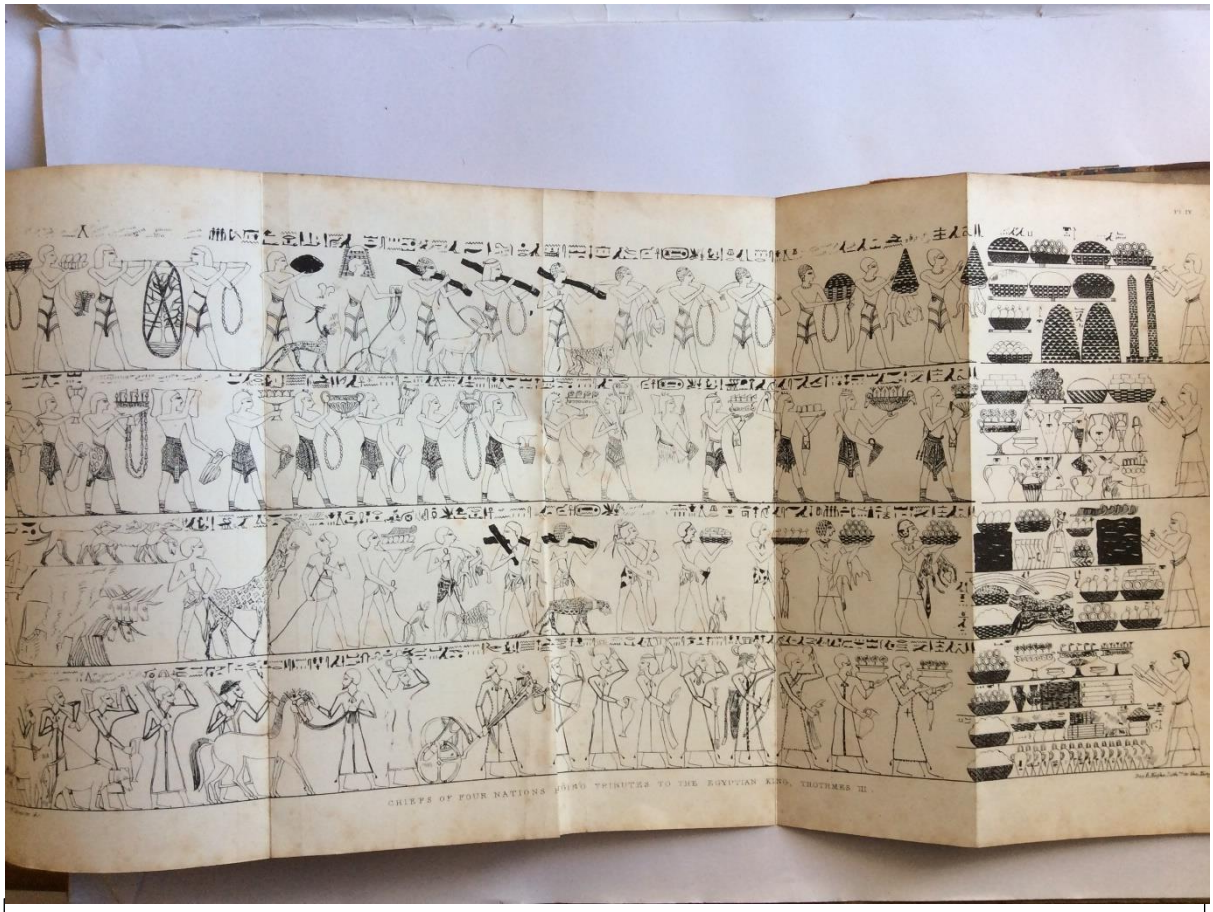


Fig. 4.1: "Chiefs of four nations bringing tribute to the Egyptian king, Thothmes III", fold-out at end of Wilkinson's *Manners and Customs* (1837), vol. 1.

A number of monarchs followed Thutmose III – some focused more on extending Egypt's southern borders – but the most militarily distinguished of these was Osirei (Sethos I). Using the reliefs at Karnak, a temple on the opposite side of the Nile to the Ramesseum and Medinet Habu, Wilkinson also managed to draw the conclusion that his immediate predecessors had been militarily undistinguished. On the grounds that the hieroglyphs accompanying these reliefs mentioned places such as Canana and Lemanon – most likely Canaan and Mount Lebanon – that were very close to Egypt, he concluded that Osirei had faced a rebellion from close allies, just over the Egyptian border. There was no way of knowing what had caused a rebellion, but Wilkinson suggested that a remission in tribute payments might have been the cause. Whatever the reason, Wilkinson noted the same series of events that he had recounted in *Topography of Thebes, and General View of Egypt* (where he had also given more extensive details about the Karnak reliefs of Osirei). The depiction of Egyptians marching through "mountainous and woody country in which the chariots could not pass" (1837: 62) strengthened the identification with Mount Lebanon. There were also grounds for believing that Osirei had campaigned further afield: another set of reliefs at the same temple depicted the "Rot-no-o", a people familiar to Wilkinson from being depicted bearing tribute in Thutmose III's tomb. Wilkinson directed his readers to a passage in *Topography of Thebes, and General View of Egypt* where he discussed that scene (he could also have directed them to the fold-out): the Rot-no-o were depicted with more clothing, instead of just kilts, and bringing tribute including a captive bear, which suggested they were from a colder part of the world, somewhat north of Egypt. Unfortunately, there was little more that could be said about them, as many of the relief scenes at Karnak were missing.

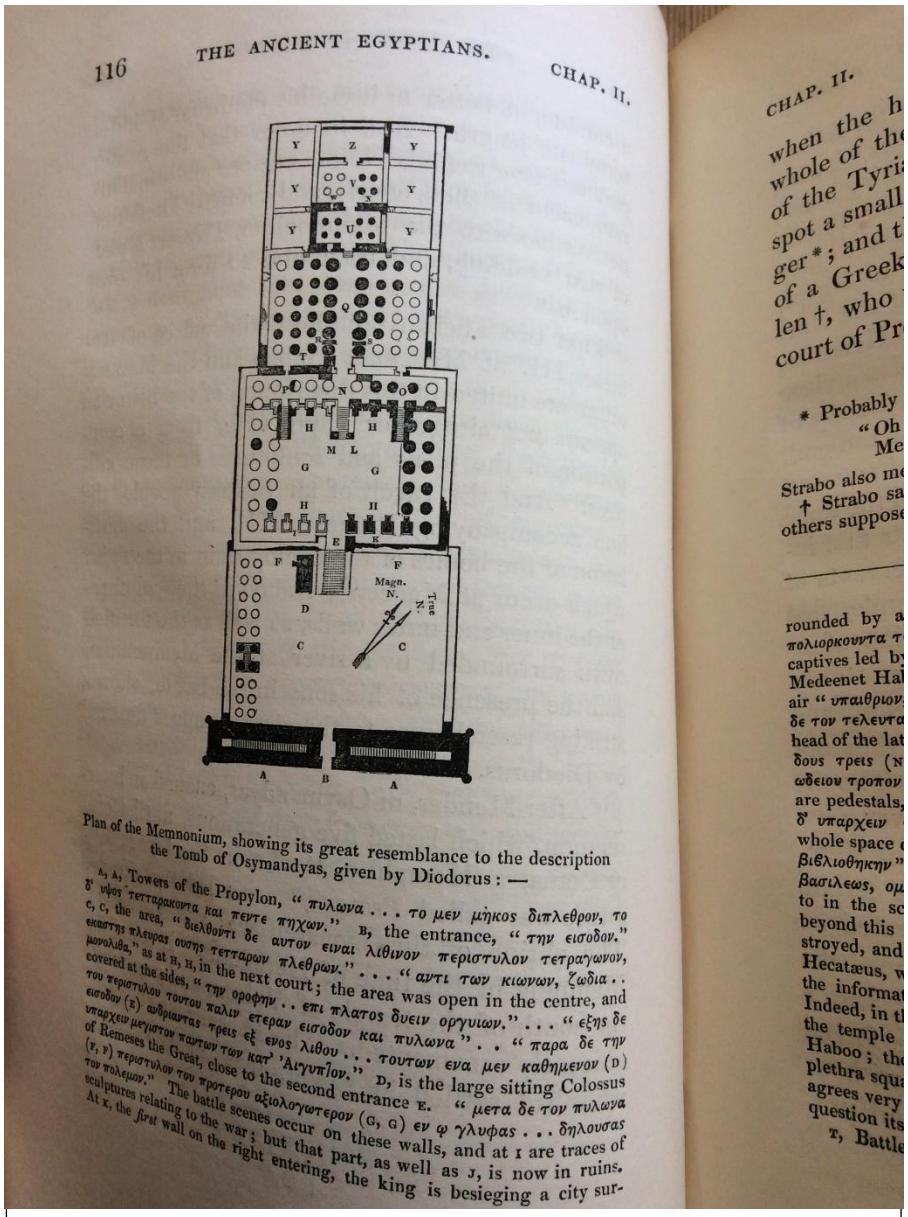
4.3.5 Ancient Egypt in the heart of Asia? The Ramesside Pharaohs

In several dozen pages of *Manners and Customs* (and *Topography of Thebes, and General View of Egypt*), Wilkinson established that the conquests depicted in the Ramesseum and Medinet Habu were far from being exceptional events: they were the culmination of a series of campaigns that had seen Egyptian armies active far from home. Having reconstructed these earlier conquests – previously entirely unknown to classical and western historians – Wilkinson turned to addressing those which had impressed

Herodotus and Diodorus, and attempted to explain precisely who the semi-legendary Sesostris had been.

That Ramesses II had mounted formidable military expeditions was not in doubt. Wilkinson had already given extensive details of the relief scenes depicting these at the Ramesseum, in which Egyptian troops were depicted assaulting fortified towns and engaging in hand-to-hand combat. Here, he added a note about the courage of the Egyptian bowmen, a group who had also been celebrated in the English historical tradition (1837: 66-67). His attempts to extract spatial information, from the physical features of the landscape and its people, had convinced him that the conflict took place far from Egypt's borders. Whilst there was the possibility that earlier monuments built by equally successful rulers might have vanished, the sheer weight of evidence, across multiple sites – Ramesses II's triumphs were celebrated at Karnak and several Nubian temples as well as at the Ramesseum – resulted in Wilkinson, and scholars including Champollion and Ippolito Rosellini (Salvoldi, 2018), concluding that the references of classical authors to Sesostris were in fact inspired by the exploits of Ramesses II.

Wilkinson acknowledged that while there had been a historical king named Sesostris, his conquests had been eclipsed by Ramesses II. Subsequent tradition (which was picked up by Herodotus on his visit to Egypt), which up until the reign of Ramesses II had eulogised Sesostris, subsequently conflated the two, adding Ramesses II's achievements to those of the earlier king (1837: 64). Once the Gordian knot had been cut and this identification made, supporting evidence multiplied: monuments inscribed with the name of Ramesses II were found in association with an ancient canal leading from the Nile to the Gulf of Suez, which seemed to support the other aspects of the record of Sesostris' achievements, such as his having maintained a huge fleet in the Red Sea (1837: 71). Later on, in a chronological consideration of the monumental evidence, Wilkinson noted the strong resemblance between the Memnonium (the Ramesseum) and Diodorus's description of the tomb of Ozymandias – the one which he had noted Egyptians fighting Bactrians – both had one of the largest statues from the Pharaonic age, and – with exaggeration of the dimensions acknowledged – the layout of both was similar. To cement the association for his readers, Wilkinson included a floor plan on one page of *Manners and Customs* (1837: 116 (Fig. 4.2)).



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Plan of the Memnonium, showing its great resemblance to the description the Tomb of Osymandyas, given by Diodorus: —

A, A, Towers of the Propylon, "πυλωνα . . . το μεν μηκος διπλεθρον, το δ' υψος τετταρακοντα και πεντε πηχων." B, the entrance, "την εισοδον." C, C, the area, "διελθοντι δε αυτον ειναι λιθινον περιστυλον τετραγωνον, εκαστης πλευρας ουσης τετταρων πλεθρων." . . . "αντι των κιονων, ζωδια . . μονολιθα," as at H, H, in the next court; the area was open in the centre, and covered at the sides, "την οροφην . . επι πλατος δυειν οργυιων." . . . "εξης δε του περιστυλου τουτου παλιν ετεραν εισοδον και πυλωνα" . . "παρα δε την εισοδον (E) ανδριαντας τρεις εξ ενος λιθου . . . τουτων ενα μεν καθημενον (D) υπαρχειν μεγιστον παντων των κατ' Αιγυπτον." D, is the large sitting Colossus of Remeses the Great, close to the second entrance E. "μετα δε τον πυλωνα (F, F) περιστυλον τον προτερου αξιολογωτερον (G, G) εν ω γλυφας . . . δηλουσας του πολεμου." The battle scenes occur on these walls, and at I are traces of sculptures relating to the war; but that part, as well as J, is now in ruins. At K, the *first* wall on the right entering, the king is besieging a city sur-

CHAP. II.

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Fig. 4.2: "Plan of the Memnonium [Ramesseum], showing its great resemblance to the description [of] the Tomb of Osymandyas, given by Diodorus", Wilkinson's *Manners and Customs* (1837), vol. 1: 116.

Evidence of a different sort to substantiate the contentions of Herodotus and Diodorus had also reached Wilkinson: in the Lebanon near Beirut, there was a carved figure, apparently carved in the Pharaonic style: "the singular circumstance of a Pharaonic monument without the confines of Egypt" (1837: 63). Since it was uninscribed, it was not certain that it could be attributed to Ramesses II, but it provided material evidence of an Egyptian presence outside Egypt, in precisely the region that Herodotus had noted it.

Although its geographical location was hardly resounding evidence for an Egyptian presence in Central Asia, there was a sense that if this case could be substantiated – alongside the other discoveries near the Red Sea and at the Ramesseum – then there were clearly grounds for being cautious about dismissing what had hitherto seemed the more outlandish parts of the narrative.

The other epic war that the Egyptians fought was the one that Wilkinson reconstructed from the relief scenes of Medinet Habu. This had taken place under Ramesses III. Using a similar logic to that which he had applied to Osirei and his predecessors, Wilkinson suggested that the kings following Ramesses II had neglected their foreign conquests, and that the new campaign was undertaken so as to force them to honour tributary payments (1837: 82). To some extent, Wilkinson was on firmer ground than he had been for Osirei, since Diodorus had attested to Sesostris's son and successor neglecting the foreign conquests: if Diodorus's Sesostris was effectively Ramesses II, as Wilkinson believed, then this time there was some textual evidence to back the theory (Diodorus, 1: 59). Without any evidence apart from the relief scenes, and nothing to indicate a contrary motive, it was an inspired idea at the very least.

Summarising the relief scenes – chariots, infantry and so on – of Medinet Habu, Wilkinson explained the presence of other peoples – evidently non-Egyptians due to their dress – fighting alongside the Egyptians by means of another novel idea: they were pressed into helping the Egyptians as a result of being recruited in Egyptian colonies, established by Sesostris. That the Egyptians had settled in other countries was both a new idea and an old one. Wilkinson had not been convinced of it a decade earlier, in *Materia Hieroglyphica*. However, both Herodotus and Diodorus had attested that Egypt, like Greece in later years, had established numerous colonies around the eastern Mediterranean. Diodorus went as far as claiming Athens as an Egyptian colony, while both maintained that Colchis had been one, on several grounds: the inhabitants of the city had a similar appearance to the Egyptians (or the Nubians), they followed similar cultural practices, and they had similar industries (see also 1837: 97). All of these pointed to some sort of Egyptian influence at an early date.

Given that there is debate over the nature of Greek colonialism which would have been the lens through which Herodotus and Diodorus saw Egyptian colonial practices – it is

now accepted that Greek colonies tended to maintain relatively loose ties to the cities that their founders came from (Boardman, 1999; Hurst and Owen, 2005), still less were they continually supplied with governors and levied locals into the military of the original city – it is possible that Wilkinson's ideas about Egyptian colonialism may have owed something to reading what were then modern-day political practices onto the ancient Greeks, and then impressing these on the Egyptians. As has been mentioned above, it is also likely that Wilkinson's thoughts would have been influenced by the Egypt of his own time: after Muhammad Ali sent troops into the Levant and Nubia, dragooning locals into his armies along the way, it was unclear where he would halt his advance (Dodwell, 1931; Sayyid-Marsot, 1984).

Returning to discuss the older war of Ramesses III, Wilkinson raised the possibility that this Pharaoh could have reached even further into Asia than Ramesses II, as new peoples, not depicted in the Ramesseum, were depicted at Medinet Habu. There was difficulty in drawing firm conclusions in this respect, as the ruined state of the Ramesseum – in contrast to the more complete reliefs of Ramesses III at Medinet Habu – made it impossible to know whether they had been encountered by Ramesses II: the crucial relief scenes might not have survived. As he had noted in *Topography of Thebes, and General View of Egypt*, Wilkinson identified two of the enemies as the Shairetana and the Tokhari. From a divergence with previous relief scenes, Wilkinson became convinced that at least some members of these groups had formerly been unwilling allies of the Egyptians, and that they had rebelled during part of the campaign. Whether this scene represented "the rebellion and defeat of the Bactrians" (1837: 84) that Diodorus had attested to, from the monument at Thebes he had described, was a question that Wilkinson chose to leave open. Wilkinson's reluctance to refer to the remote geographical location east of the Aral Sea that he had so firmly pinpointed in his earlier work is hard to explain. One possibility is that, following only a lukewarm reception of his claims in the *Quarterly Review*, he may have chosen to hold back the more controversial aspects of his work from publication (Anon., 1835). Alternatively, it is possible that Wilkinson, initially eager to match up the classical sources with relief scenes, shifted his own position on the matter.

Towards the end of his chronological account of the Egyptian Pharaohs and their exploits, Wilkinson turned to considering how best to match up the monumental record

with that provided by classical authors, notably Diodorus. Here, Wilkinson took a more critical approach to the one he had voiced in his earlier work. On the grounds that many of the motifs which Diodorus had mentioned in his description of the palace-tomb of Ozymandias – a king with a lion, captives yoked to a chariot and the mutilation of prisoners – were identifiable, Wilkinson declared that it was "not altogether improbable" that Diodorus had conflated features of the Ramesseum and Medinet Habu, in his description of the tomb of Ozymandias (1837: 67, 115). By this point, it is possible that Wilkinson was more concerned with definitively tying the achievements of Sesostris – which to use his words resulted in the "Augustan era" (1837: 20) of ancient Egyptian history – to the Ramesside kings than he was to proving that the Egyptians had reached Bactria: increasing numbers of visitors to Upper Egypt were able to attest to an Egyptian presence in that region, rendering the need to prove an Egyptian presence in Asia less of a priority. It is also possible that modern political developments influenced Wilkinson: by the time he wrote *Manners and Customs*, Egypt was far less of a military power in Asia than it had hitherto been: between Wilkinson's *Materia Hieroglyphica* (1828) and his draft of *Topography of Thebes, and General View of Egypt* in the late 1820s, and the publication of *Manners and Customs* in 1837, fears that Muhammad Ali's Egypt could upset the balance of power in the Eastern Mediterranean, by replacing Constantinople, were substantially reduced by a multi-powered reining-in of Egypt (Sayyid-Marsot, 2007).

Wilkinson's chronology continued after Ramesses III, but when it came to the question of the Egyptian empire, there was a long gap in both the monumental and textual record: there were no monumental relief scenes of large-scale military campaigns, and there was no mention of an Egyptian presence in Asia until the Bible picked up the narrative – and this only showed a much-reduced Egypt active in Mesopotamia rather than Central Asia. It was impossible to speculate whether Ramesses III's successors had abandoned the foreign conquests in favour of a more isolationist stance, or whether, after the ferocious campaign depicted at Medinet Habu, the empire was now secure, with the conquered territories paying the tribute that had presumably been imposed upon them (1837: 87).

An alternative explanation was that Egypt had declined, and this was a thesis followed by Wilkinson's friend and fellow traveller (and the dedicatee of *Manners and Customs*), Algernon Percy, Lord Prudhoe. Prudhoe's explanation involved dating the events of the

Exodus to the 19th dynasty rather than the early 18th dynasty as Wilkinson favoured, or after the end of Ramesses III's reign. He interpreted the Bible more literally than Wilkinson. Whereas Wilkinson – in keeping with his wider project of rehabilitation – presented the Egyptians as unwilling oppressors, Prudhoe emphasised that the state had gone to great lengths until the very last moment, to keep the Hebrews enslaved, as it was reluctant to let go of a plentiful supply of cheap labour. On them fleeing, Egypt would have been expected to decline rapidly. To Prudhoe, keen on directly equating aesthetics to civilization as Winckelmann had propounded, this had clearly happened: the Egyptian art following Ramesses III's reign was of a much worse quality. Winckelmann's thesis was one that was acceptable to Wilkinson too, and the latter agreed that Ramesses III's reign had seen the last great era of Pharaonic art, with new innovations in carving hieroglyphs. However, he was reluctant to draw wider conclusions about national decline from the reduced source base or art alone. As he had argued for Osirei's predecessors, including those who had rebelled against the Shepherd kings, the mere absence of monuments could not straightforwardly be interpreted as absence of militarism: they might well have been destroyed during the intervening three millennia, and destruction was still ongoing in the 1820s. Effectively, the events of *Exodus* were regarded as a historical event of great significance, but it was impossible to tie them down securely, and alternative chronologies were possible, of which Wilkinson – who had already used the flight of the Hebrews to fill a gap left by his moving the expulsion of the Shepherd kings back by several centuries – was aware. Prudhoe's explanation was better than Wilkinson's in another respect: if the Hebrews had fled Egypt before the foreign conquests began in earnest, the Bible should have recorded them. Wilkinson was forced to explain its silence on this matter on the grounds that the Hebrews became insular after their success in establishing an independent state, and the Egyptian conquests had not directly affected them.

During the late 1830s, an appreciation that Wilkinson was interested in finding the best workable solution to questions of chronology, as opposed to defending a pet idea for scholarly integrity, can be gleaned from his changing his mind on an issue which he had formerly regarded as settled, and which he had defended over two pages at the start of *Manners and Customs*: the identity of the Shepherd kings, who had apparently oppressed

Egypt for several centuries, before being expelled. Egypt's war of conquest in Asia, against them and subsequently other peoples, followed. In the first edition of *Manners and Customs* (1837), Wilkinson held that the Assyrians were the occupiers. Four years later in 1841, in the second expanded edition – at the very start of the last volume – Wilkinson admitted that he had changed his view, and had now come to favour Champollion's belief that the occupiers were the Scythians. Although it has been claimed on several occasions that Wilkinson and Champollion were rivals (e.g. Romer, 1981; Thompson, 1992a; James, 1997), it was not in fact the first time that Wilkinson had been convinced by Champollion to change his own opinion – he did so over both linguistic and artistic matters on several occasions.

In Wilkinson's time, relatively little was known about the Scythians; as with the Egyptians, the two main sources of knowledge were classical texts and material culture. Herodotus, in particular, gave extensive attention to the Scythians, their nomadic lifestyle, unusual customs and fierce resistance to empire-builders, such as the Persians. Greeks in general appear to have viewed the Scythians, with their nomadic lifestyle, as the "other" to their own culture, and this is thought to be a reason for their prominence in *The Histories* (Cunliffe, 2019). In the early 19th century, several explorations of tomb-mounds on the Russian steppes, believed to have been built by the Scythians, were excavated and revealed to contain artefacts including exquisite jewellery. Whilst it is unlikely that Wilkinson changed his ideas due to such discoveries, he possessed at least a sufficient passing familiarity with the Scythians to sketch one of these tomb mounds, alongside a European barrow and an Egyptian pyramid, in a notebook dating from 1821, shortly before he left for Egypt.[25]

Wilkinson's stated reason for changing his opinion over the identity of the Shepherd kings was that the Scythians, with their pastoral mode of life, were better candidates for the alternative name of the Shepherd kings, the "Pastor kings"; a dimension which Wilkinson had marginalised in his argument that they were Assyrians. Although this might have seemed to destroy Wilkinson's argument that only the Assyrians, a superpower based on agricultural wealth like Egypt, would have had the power to successfully

[25] "Extracts, notes and sketches on Egyptology, hieroglyphics and related subjects, *c*.1821", Bodleian Library Wilkinson Manuscripts, dep. e. 55, folio 79.

conquer the latter state, Wilkinson suggested that the issue was more complex in practice. Whilst states based on agriculture had the potential resource base to dominate their neighbours, "experience and the history of the world" had shown that nomad pastoralists were attracted toward and had conquered settled agriculturalists on numerous occasions (1841: 3). Wilkinson's conversion was so complete that he assured his readers that it was surprising that Egypt had retained its independence for so long following the expulsion of the Shepherd kings. He pointed readers toward the successful conquests of peoples such as the Persians, Macedonians, Arabs and "the Tartar hordes of Central Asia" (1841: 2), several of whom were known to have followed a mode of life based on pastoralism and pillage when they invaded Egypt. To some extent, the Scythians were a compromise solution: they were on record as having followed a pastoral lifestyle, so fitted in with the term "Shepherd kings" or "Pastor kings", while at the same time they were known, on the basis of Herodotus, to have some material culture – possessions including weapons and, in a few cases, burial mounds. To use language borrowed from Scottish and French conjectural historians such as Lord Kames, they were at a higher level of development than hunters and fishers, even if they were not as developed as agriculturalists (Meek, 1976). Wilkinson chose not to tackle the more difficult issue that if the enemy depicted on the relief scenes were indeed Scythians, their besieged towns did not accord well with Herodotus's assurance that they lacked towns.

Wilkinson's reason for preferring the Scythians over the Assyrians, despite the difficulties, may be a simple one: the Scythians were a more remote group: using information derived from Herodotus, scholars interested in ancient geography had identified the Scythian homeland as stretching from the lands north of the Black Sea for a considerable distance eastward into Central Asia. Had the Shepherd kings been Scythians, then the Egyptians, following them in pursuit – as Wilkinson's simplistic model, which bears some resemblance to Herodotus's own narrative, that the Scythians defeated the Cimmerians and subsequently pursued them – would have had a ready-made route to Central Asia. Diodorus – Wilkinson's principal guide in the whole matter of ascertaining how far Egypt's empire in Asia had extended – had also reported that Sesostris (whose conquests were attributed by Wilkinson to Ramesses II) had defeated the Scythians in battle. Retelling a possibly apocryphal story, Diodorus alleged that, on conquering Egypt,

the Egyptian priests had weighed the achievements of Darius of Persia against those of Sesostris, and found the former inferior, because he had failed to conquer the Scythians, as Sesostris had done. Effectively, Wilkinson's switchover to arguing that the Scythians composed Egypt's principal enemy functioned as an argument that the Egyptians, at the height of their power in the late second millennium BC, were militarily superior to the Persians of the 6th century BC.

There is also another intriguing reason why Wilkinson changed his identification of the Shepherd kings from Assyrians to Scythians: his attempt to make sense of the Tel el-Amarna relief scenes. Today the Amarna period, in which the Pharaoh Akhenaten replaced the polytheistic Egyptian religion with something that resembled monotheism, centred around worship of a solar deity, the Aten, is one of the best-known periods of ancient Egyptian history, not least because it is closely associated with Tutankhamun, the penultimate ruler of the episode and of the 18th dynasty (Reeves, 2001). By contrast, when Wilkinson was active in Egypt, the Amarna period was totally unknown and its distinctive art style hard to understand, as it differed greatly from most other known Pharaonic remains.

To Wilkinson, as we have seen, stylistics were of great importance in dating material culture that was not inscribed with a name of any king. However, this method proved inadequate for Wilkinson when he stumbled across the ruins of Akhenaten's city of Akhetaten, which he named Tel el-Amarna in 1824. It is possible that Wilkinson's reaction to the discovery – his decision to hide it from his colleagues is regarded by Thompson (1992a) and Selwyn Tillett (1984) as a major breach of an informal gentlemanly information-sharing culture that Wilkinson and his colleagues maintained while in Egypt – may well have ended up further hindering his understanding by making him unaware of similar material being uncovered elsewhere, which he learnt about by the time of writing his guidebook, *Modern Egypt and Thebes* (1843).

Although this work was essentially an expanded version of his earlier *Topography of Thebes, and General View of Egypt* (1835), the section on Tel el-Amarna is expanded beyond recognition. Whereas the earlier book simply referred to the Amarna relief scenes as reminiscent of a militaristic style, in his later book – the subject is dealt with in a very confusing manner in the intervening volumes of *Manners and Customs* (1837) –

Wilkinson now suggested that "the representation of the Sun under this form indicates the introduction of a new form of worship, by a foreign usurper" (1843, vol. 2: 73). As far as the strangely-depicted figures were concerned, he was convinced that "it is evident they are not Egyptians", instead they were "foreign princes" (1843, vol. 2: 74). This identification was strengthened still further by the finding that most of the names of the kings on the smashed statues had been deliberately erased, and those that could be identified were omitted from king-lists, such as that of Abydos. To Wilkinson, the only explanation was that, presumably after their expulsion, they had been "looked upon with hatred in the country" and deliberate acts taken to destroy their memory. There were several possible candidates, but, after dismissing the Phoenicians, he settled on one in particular: "Some of the Scythians, too, worshipped the 'Sun'; and these sculptures may possibly prove a second irruption of that people, whom I suppose to be the 'Shepherds'" (1843, vol. 2: 75).[26] To Wilkinson, the only question was whether the foreign kings had come to power through the result of a civil war (the recent political turmoil in Egypt which resulted in the rise of Muhammad Ali, an Albanian, might have been in Wilkinson's mind) or an enemy invasion. Whatever Wilkinson's precise reasoning, his suggestion that the Amarna period kings were simply another wave in the centuries-long war which the Egyptians had fought against an Asian enemy forms an interesting footnote to his contribution to the study of this period of ancient Egyptian history – especially given that existing books on the Amarna period simply mention Wilkinson's role as a discoverer and recorder of the relief scenes (Reeves, 2001), as opposed to his role as a scholar who struggled to interpret them with an approach that equated distinct material remains with distinct peoples, in some of the early years of the study of ancient Egyptian culture.

4.4 Conclusion

The exceptionally diverse range of methodologies that Wilkinson used to reconstruct the extent of the ancient Egyptian empire defies easy categorisation under narrow disciplinary terms. For investigating the ancient Egyptian presence in Upper Egypt,

[26] In a footnote, Wilkinson referenced the Massagetae as the sun-worshipping Scythian tribe. Herodotus Book 4.

literally anything could be important: bricks could indicate sites on the Antonine Itinerary, pottery and architectural fragments could distinguish towns, and the most discrete of stylistic fragments could indicate the presence of a much older Pharaonic-era temple on the site of a later one – such remains were important for demonstrating that the Egyptian empire described by classical authors had a basis in reality. All of this – which incidentally involved no excavation – belies the common suggestion that antiquarian scholars were uninterested in objects which lacked aesthetic appeal before Petrie arrived in Egypt (Petrie, 1931; Cottrell, 1956: 180; Wilson, 1964; Mertz, 1964**;** Thompson, 1992a: 87, 113). It also belies the other common casting of antiquaries as interested in such unappealing fragments for their own sake, when they were in fact frequently crucially important for approaching historical problems. To draw a line between aesthetic and unappealing sources would, however, be a serious mistake: in Upper Egypt and Nubia, stylistics, which depended on distinct architectural fragments, was a vitally important method. When Wilkinson was denied access to material in the field, he turned to artistic masterpieces – the relief scenes – to extract information for scholarly purposes.

However, this process of obtaining information from remains in the landscape, or the "discovery" of the extent of the ancient Egyptian empire in Nubia, was far from straightforward: the evidence required interpretation and Wilkinson brought his own western ways of seeing – an unavoidable issue – which affected his attempts to establish the original meaning of his sources. Inevitably, many of Wilkinson's conclusions are no longer accepted: his suggestion that the ancient Egyptians maintained a presence in the southern Red Sea would appear outlandish today, while his contention that the ancient Egyptian empire extended into Central Asia, as far as Afghanistan, now appears positively absurd, and no studies today maintain that a direct link existed between the two regions in antiquity.

Having pointed out such divergences, it is nevertheless important to reemphasise the results of Wilkinson's work and the intellectual challenges that he faced. Most obviously, Wilkinson helped – especially with his widely-read works which collated his research alongside that of others – to bring about a paradigm shift in how Egypt was viewed. By the 1830s, it would have been impossible to still accept Hamilton's contention that the Romans had extended Egypt's boundaries further into Africa than at any other period.

For a time, the idea that the Egyptians had conquered ancient Bactria, advanced tentatively by Champollion and boldly by Wilkinson, was taken seriously: the *Quarterly Review* discussed it, and, for a time, Austen Henry Layard (1849) took Wilkinson's suggestions seriously. Ironically, it was the continued study of hieroglyphs, which Wilkinson contributed toward, which led to his conclusions becoming invalidated in the second half of the 19th century. The way of seeing Egyptian images established by Champollion and Wilkinson lasted even longer: after being enthusiastically greeted by the *Quarterly Review*, its influence lasted into the 20th century: photographs of some of the Egyptian galleries in the British Museum show Egyptian tomb paintings as framed pictures, quite different from the full-wall paintings of their original context (Parkinson, 2008).

Wilkinson's working assumptions are also noteworthy. Whilst it is frequently noted that he relied heavily on classical sources – these are undoubtedly what led him to argue for an ancient Egyptian presence in Central Asia – and on the Bible, the above consideration reveals a far more complex picture. Wilkinson eagerly used numerous highly fragmentary indigenous sources, and attempted to reconstruct an independent historical narrative from these: an enterprise which is only gradually becoming widespread at the present time (Van De Mieroop, 2011: 4; Murray, 2013; Gamble, 2015). From this, Wilkinson concluded that the "Shepherd kings" were one of the key dynamics of ancient Egyptian history: it was a rebellion against them that resulted in wars of conquest abroad, and his continued thinking about this issue also conditioned his placing of Exodus: whatever else it was, his chronology was not one that was centred around Old Testament questions. That Wilkinson attempted to use relief scenes to construct a narrative about ancient Egyptian history is also important, given that many others who utilised the same scenes, before and after him, such as Blumenbach, Lyell and Petrie, put them toward their own ethnographical and anthropological projects (Blumenbach, 1794; Lyell, 1863; Challis, 2014).[27] Whether Wilkinson interpreted such sources "correctly" is a different matter –

[27] Wilkinson's approach to using ancient representations of people was quite different to that taken by Petrie, who was interested in ethnographical questions of a problematic nature, on behalf of Francis Galton (Challis, 2014). Wilkinson's approach is still used extensively in questions surrounding the identity of the Bronze Age "Sea Peoples" (e.g. Sandars, 1978; Cunliffe, 2008), although Shaw (2019) has suggested that such research may be misleading as the Egyptian images were highly stylised and unrepresentative of reality.

modern consensus, outlined by Shaw (1996; 2019), has moved toward seeing Egyptian artwork as laden with religious iconography as opposed to straightforward historical narrative – but not the major issue. Not least, there remains much debate around interpretation of ancient Egyptian art, including of the relief scenes at the Ramesseum and Medinet Habu, which are open to very different readings (Nelson, 1943; Schulman, 1982; Shaw, 1996; Shaw, 2019; D'Amato and Salimbeti, 2015), as is much of ancient Egyptian art. Such debates also exist at a textual level and, if Sesostris's activity in Asia has been discounted, passages in Herodotus and Diodorus remain heavily contested and open to very different readings today (Lloyd, 1988; Fehling, 1990; Pritchett, 1993). What matters far more is Wilkinson's devising an array of methods to approach these issues.

Chapter 5: The reconstruction of Egyptian landscapes: antiquarianism and geomorphology

5.1 Sir John Gardner Wilkinson's investigations into a dynamic Nile

5.1.1 Introduction

In addition to his attempts to investigate the extent to which the ancient Egyptian empire had extended beyond the Nile Delta and the northernmost reaches of the Nile Valley, into Asia in the north and Nubia to the south, Wilkinson also undertook research into ancient geography of a very different nature. He carried out geomorphological investigations into the ancient extent of the Nile Delta, in relation to the Mediterranean, and the ancient extent of the Nile valley, in relation to the desert on either side of it. As a result of two investigations, one in the harbour of Alexandria and the other in relation to the Colossi of Memnon at Thebes, Wilkinson came to believe that the environment had undergone substantial changes since the Pharaonic era. It had not been merely a static backdrop. Wilkinson's research had broader implications beyond reconstructing the landscape that the ancient Egyptians lived in.

Wilkinson's work in reconstructing ancient landscapes through attention to geomorphological processes was not an end in itself either: his ultimate aim was to use them to help answer historical questions and shine light on theories of human prehistory and the development of civilization in Egypt. Observing the slow rate of sediment accumulation in the Delta, Wilkinson concluded that the Delta was extremely old and need not necessarily have been peopled from Upper Egypt: the first Egyptians might have originated in the north rather than the south. Wilkinson also became convinced that since the fertile areas that were suitable for agriculture had been less extensive in the past, the inhabitants must have made extremely efficient use of them: they had been an agricultural and even industrial and commercial society, rather than one based on hunting and pasturage subsistence strategies. It was this factor that allowed the ancient Egyptian state to dominate its neighbours in Nubia and in southwest Asia, following the expulsion of the "Shepherd kings". Effectively, Wilkinson continued to ask historical and

antiquarian research questions, but drew on geological and geomorphological evidence to answer them. However, this remains unappreciated in current histories of Egyptology.[1]

In recent years, there has been a major growth in interest among practitioners of Egyptian archaeology as to how changes in the natural environment enabled and constrained human activity. Karl Butzer's (1976) innovative work on cultural ecology and the formation of the early Egyptian state has been followed by many studies seeking to reconstruct ancient landscapes, including the Egypt Exploration Society's two extended landscape archaeology programmes, the *Survey of Memphis* and the *Theban Harbours and Waterscapes Survey*.[2] Both projects have made use of scientific techniques including geophysics and mud logging, and both have the ultimate aim of ascertaining the impact of geomorphological changes, with the aim of solving historical problems. One of the impressive outputs of this work has been the discovery that the Tale of Sinuhe (a Middle Kingdom Egyptian story about loyalty and belonging, involving a man fleeing and returning to Egypt) describes a landscape that once existed rather than one hitherto thought to be metaphysical (Bunbury and Jeffreys, 2011). Another is the discovery of ancient canals in the vicinity of the Colossi of Memnon which may have been used to transport blocks of stone for the construction of temples on the west bank at Thebes (including the two colossi, which were each carved out of a single block of stone weighing nearly one thousand tonnes (Graham *et al.*, 2012)). Some of the most recent research has posited that geomorphological changes in the Nile Delta, which led to the environment reducing in productivity, may have led to people taking up agriculture, which in turn produced a surplus and furthered population growth and settlement (Pennington *et al*., 2021). In many ways, these research questions are very similar in their broad outlines – the precise details are more exact as would inevitably be expected from better-funded work with two centuries of innovation – to those that Wilkinson investigated in the 1820s and 1830s. Both phases feature the verification of an ancient myth or story, both seek to

[1] There is no mention of Wilkinson's geomorphological fieldwork in the *Dictionary of National Biography* (Thompson, 2004), *Who Was Who in Egyptology* (Bierbrier, 2019), Thompson's (1992) biography of Wilkinson, nor any history of Egyptology (Greener, 1966; Hobson, 1990; Thompson, 2015; Bierbrier, 2019; Naunton, 2020; Wilkinson, 2020) that I know of.

[2] For a good overview of such work since this publication, see K.W. Butzer, Landscapes and environmental history of the Nile valley: a critical review and prospectus, pp. 99-124 in I. Shaw and E. Bloxam (Eds) (2020). *The Oxford Handbook of Egyptology*.

ascertain how the Egyptian environment influenced the development of Egyptian civilization, and both involve the reconstruction of what ancient landscapes in particular parts of the Nile valley may have looked like in antiquity.

Despite these profound similarities, there is relatively little acknowledgement that modern landscape archaeology or research into environmental change had some sort of early 19th-century precedent. Mainstream histories of Egyptology in the 19th century have tended to focus on the race to decipher hieroglyphs, the scramble to acquire antiquities, and the discovery of tombs and monuments. For example, the first volume in Jason Thompson's multi-volume *Wonderful Things: A History of Egyptology* (2015) gives no attention to geomorphological research in the 19th century. In his biography of Wilkinson, Thompson (1992a) only records the title of an article in which he published the results of his research. Thompson is not alone: C.W. Ceram (1952), Leslie Greener (1966), Christine Hobson (1990), Jean Vercoutter (1992), Chris Naunton (2020) and Toby Wilkinson (2020) all take a similar approach. The introduction to one recent collection of studies into Egyptian landscape archaeology maintains that the earliest pioneers of relevance to the history of the sub-discipline were Jacques Boucher de Perthes and Charles Lyell, who applied geological principles to archaeology in the second half of the 19th century (Tristant and Ghilardi, 2018). Whilst there has been some acknowledgement that several scholars used geomorphological methods to help answer historical research questions elsewhere (Wagstaff, 1987; Wagstaff, 2001; Allen, 2002; Koelsch, 2013), this has not really been the case in Egypt, where geomorphological research has been presented as a relatively recent development, which makes modern archaeology truly scientific. David Jeffreys has asserted that scholarship in the 19th century Nile valley was held back by a belief in "eternal Egypt", which "essentially meant that things had always been the same – an extraordinarily lazy way of thinking" (2012: 8) and that, with few exceptions, interest into historic landscapes dates back only a few decades.

In his major study of ancient Memphis, Jeffreys (2010) points to one early pioneer into the study of ancient landscapes and environmental change, Joseph Hekekyan, who was active in the 1850s. Acting as an agent for Leonard Horner, a factories inspector who undertook geological work in his limited free time, Hekekyan conducted an excavation programme at two ancient sites in Lower Egypt, Heliopolis and Memphis, in an attempt

to investigate the length of time which humans had inhabited Egypt. For his pains, Jeffreys suggests that Hekekyan was regarded as an "interloper" by other Egyptologists, who were more used to using textual sources: "the idea that much could be learned from an approach which combined geology and excavation seems to have been dismissed, or at the very least under-appreciated" (2010: 196).

Jeffreys' admiration of Hekekyan's work is shared by his colleague, Judith Bunbury. In a historical sidenote to one research article, she notes that he might be regarded as "the first geo-archaeologist" (2013: 64), though in a subsequent book she implies that such an accolade should be granted to Gertrude Caton-Thompson, who "in the early twentieth century suggested that the environment had not always been what it is today" (2019: 5). Bunbury is surely correct in her assessment that Caton-Thompson's work in the Fayum laid the foundations for the research into what followed, and our current understanding of environmental change in Egypt (Romer, 2012; Hendrickx and Vermeersch, 2003). However, I argue that a lack of interest amongst Georgian and Victorian scholars in exact modern research questions or the utilisation of modern methods should not be conflated with a total lack of interest in investigating all forms of environmental change. It would be at least as valid to suggest that Caton-Thompson rediscovered an older methodological approach that had become marginalised as the remit of Egyptian archaeology narrowed during its consolidation as an academic discipline, as it would be to suggest that she took the subject in an entirely new direction.

As will also be seen in due course, the widespread elevation of Hekekyan's research (Quirke, 2001; Reid, 2002; Jeffreys, 2010; Naunton, 2020), which stems from the elevation of stratigraphy more broadly (Daniel, 2005; Renfrew and Bahn, 2004), and the viewing of the 1850s as a crucial time in which narrow-minded "amateur" antiquarianism transitioned to "scientific" archaeology, is not without its problems. Most importantly, Hekekyan's colleague, the geologist Leonard Horner, imposed Lyell's extreme brand of uniformitarian geological thought on evidence which suggested that the contrary was the case. The result was that the conclusions drawn from the first stratigraphic excavation in the history of Egyptology were deeply flawed. Whilst leading scientists, including Lyell, Darwin and Murchison, overlooked the problems, two antiquaries, Samuel Sharpe and then, at great length, Wilkinson, in a correspondence with Lyell, pointed out the

shortcomings. If Lyell was unsuccessful in obtaining the support he desired, he at least became aware of the more complex situation on the ground in Egypt. It is possible to draw these conclusions as Lyell's side of the correspondence has become newly available to view: in his biography of Wilkinson, Thompson only had access to Wilkinson's letters, without Lyell's requests for information.[3] From this limited source base, Thompson concluded that Wilkinson was primarily motivated by religious concerns to write to Lyell. In fact, as will be revealed, the correspondence deals almost exclusively with the Horner-Hekekyan excavations, and Wilkinson's religious objections were neither a significant part, nor hugely different from Lyell's own beliefs.

5.1.2 The "Catastrophist" Backdrop

Before going into any detail on Wilkinson's geological and geomorphological research, it is necessary to give a preliminary overview of debates in European geology and geomorphology immediately prior to his fieldwork. During the late 18th and early 19th centuries, there were considerable advances in the earth sciences (Davies, 1968; Rudwick, 2005, Rudwick, 2008).[4] In particular, there was a growing appreciation among geologists and other scientists that the Earth's surface was dynamic and that its landforms had been wrought over a very prolonged period of time. This was well underway by 1800, with pioneers including many continental earth scientists, such as Horace Bénédict de Saussure, André Deluc, Georges Cuvier and Georges-Louis Buffon. All were convinced that there had been a lengthy prehistory, lasting at least thousands, and possibly millions of years, before the arrival of humans, although some of their ideas were very speculative.

At the same time that a long timescale for Earth's history was becoming a scientific consensus among geologists (although not yet amongst the general public), there was a

[3] There exist two copies of Wilkinson's letters to Lyell: those in the Lyell collection owned by the University of Edinburgh, and those at Calke Abbey (National Trust). However, only Calke Abbey holds copies of Lyell's side of the correspondence, and these have only recently become available to researchers. With access to only half of the correspondence, and without knowledge of the existence of the other half, Thompson's contentions are understandable.

[4] Other key scholars in this area are S.J. Gould and G.L. Davies. Rudwick's prime interest is in geology, and more specifically geohistory, as opposed to geomorphology. By contrast, Davies is almost wholly concerned with geomorphology.

parallel debate taking place among geologists and geomorphologists, which has been described as predominating "over all others" (Davies, 1968: ix). This was as to whether the Earth's history had been overwhelmingly one of continuity, with the same naturally-occurring processes as those in the present day (such as weathering by the elements, erosion of land by rivers and wave action etc.) solely responsible for landforms, or whether the Earth's surface had been heavily-impacted by higher-magnitude events which happened infrequently and of which there was only ambiguous record in the then-known several thousand years of human history.

These two perspectives have been simplified in much of the subsequent historiography, until recently, into "uniformitarianism" and "catastrophism", with geologists assigned to either one or the other. Those following the former approach have been celebrated as methodical scientists; in contrast the latter have frequently been cast as fantasy-inclined speculators who distorted the evidence to fit the Biblical narrative. However, as Martin Rudwick and Stephen Jay Gould have shown, what has subsequently been termed "uniformitarianism" in fact encompassed (and encompasses) multiple dimensions, most of which were acceptable to all geologists – notably the principle (dating back to Newton) that natural laws had stayed constant (Gould, 1965; Gould, 1987; Rudwick, 2008).[5] It is largely due to an uncritical reading of the historiographical overview in the first volume of Charles Lyell's *Principles of Geology* (1830), written by the author to discredit his predecessors, that "the present is the key to the past" is now commonly assumed to be the monopoly of a uniformitarian branch of geology, and that "scientific" geology started only with Lyell.[6]

More seriously though, Lyell's deliberate misrepresentation has given rise to an assumption that "catastrophist" geologists invoked extraordinary forces of nature as a matter of course. This was far from the case: in practice, most acknowledged presently-occurring processes, and worked back from the present day wherever possible. The only

[5] In this regard, catastrophists were unlike Rupert Sheldrake, who has argued, against scientific consensus, that the laws of physics, such as the speed of light, have varied over time, as opposed to remaining constant. This is not accepted by most scientists.

[6] Examples include the following: C.C. Gillispie, *Genesis and Geology*, 1951; Holden, Approaching physical geography, 2012, 3-26; Lowe and Walker, *Reconstructing Quaternary Environments*, 2015, 5; Virgili, Charles Lyell and scientific thinking in geology (2007), 572-584. An exception is Summerfield, *Global Geomorphology*, 1991, 4.

substantive difference between the two sides was that those subscribing to catastrophism invoked events of a higher magnitude than any currently in action to explain landforms that they could not envisage the currently-occurring natural processes as ever having created, regardless of how long they were allowed (see Fig. 5.1).[7] They differed in this regard from others, notably James Hutton and, later, Charles Lyell, who, though sharing the belief that the Earth was dynamic, assumed that weathering and erosion had never exceeded their present-day power, but had acted over an incredibly long time. Despite Huttonian/ Lyellian uniformitarianism being widely assumed to represent a scientific gold standard in geology, several historians of geology have pointed out that it is ultimately an assumption which cannot be verified, and as such imposes simplicity upon evidence, regardless of whether or not it is appropriate (Gould, 1965; Baker, 1998; Anderson, 2007).[8] For this reason, it has been referred to as "overly restrictive" (Baker, 1998: 171) and even "stifling to hypothesis formation" (Gould, 1965: 223). In other words, geology took a wrong turn when it backed Lyell's extreme uniformitarianism. Although the term "catastrophist" itself has not made a comeback, it is worth noting that its tenets – that conditions in the past were not necessarily like those in the present due to the action of natural processes which vary markedly in degree – are present in modern scientific research into mass extinctions (Gould, 1987: 175-177), ice age theory (Rudwick, 2008: 517-522), proglacial environments (Soennichsen, 2008), and even Martian landscape formation (Anderson, 2007). It is for such reasons that Gould has commented 'we must admit that current views [in geology] represent a pretty evenly shuffled deck between attitudes held by Lyell and the catastrophists' (Gould, 1987: 177).

Another crucial feature which has also been inadequately appreciated in the existing, especially textbook, historiography is that uniformitarianism did not enjoy an inexorable rise from Hutton's time to Lyell's.[9] Contemporary ("catastrophist") geologists refuted

[7] Rudwick uses a French term, "actual causes" to describe these, as many of the geologists he discusses were French. As the word "actual" has a different meaning in English, it will be avoided here. Instead, I refer to "currently-occurring natural processes". These are sometimes referred to as "causes now in operation" or "causes now in action".

[8] For a representative popular example of support for Lyell's uniformitarianism above all else, see W. Nothdurft, J. Smith, M. Lamanna, K. Lacovara, J. Poole and J. Smith, *The Lost Dinosaurs of Egypt*, New York, 2002, 36-39.

[9] Examples include the following: Summerfield, *Global Geomorphology*, 4; Holden, Approaching physical geography, 6; Lowe and Walker, *Reconstructing Quaternary Environments*, 5; Virgili, Charles Lyell and scientific thinking in geology, 573.

Hutton's claims on multiple grounds, as the accruing evidence from fieldwork – geologists prior to Lyell did indeed carry out extensive fieldwork – suggested that currently-occurring natural processes alone were insufficient to explain Earth's landforms (Rupke, 1983; Rudwick, 2008).

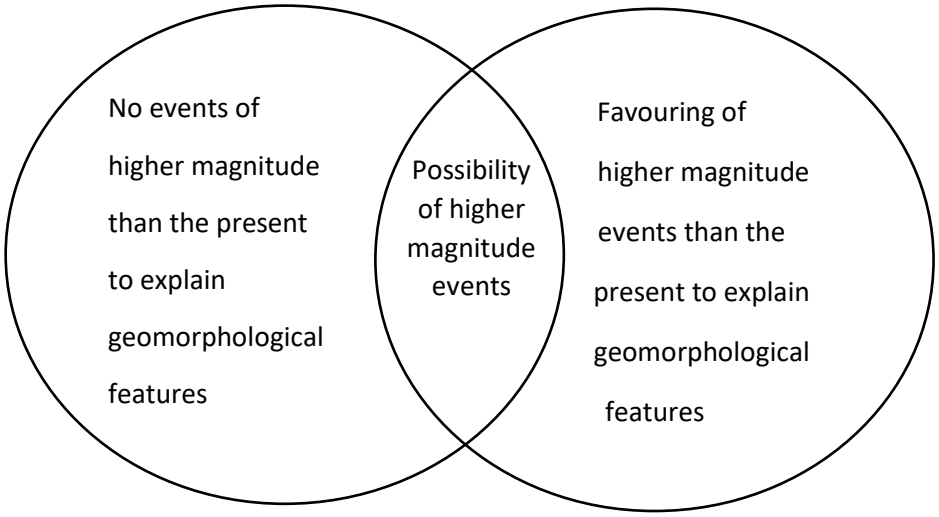


Fig. 5.1. Venn diagram demonstrating the relationship between Charles Lyell's brand of uniformitarianism (left), catastrophism as popularly conceived (right), and catastrophism as early 19th -century practitioners viewed it (centre).

Opponents of Hutton, notably André Deluc, pointed out numerous geomorphological features which seemed to defy Hutton's thinking (Davies, 1968: 238-241; Kennedy, 2006: 26-34, 41-42; Rudwick, 2008). Without reference to some powerful force no longer active, it was hard to believe that the great U-shaped valleys in Scotland and the Lake District had been eroded by the tiny rivers, or "misfit streams", which flowed through them. Even large rivers tended to erode a V-shaped valley, rather than a U-shaped one. It was even harder for Hutton and his followers to explain the formation of the dry chalk valleys leading to the south coast of England which lacked a river altogether. Other problems abounded. Deep lakes in the Alps could not be explained by extrapolating the erosive power of rivers. Elsewhere, many rivers appeared to be filling their valleys rather than eroding them: some other explanation was required for the formation of these valleys. Other valleys, including many on the western coasts of the British Isles, continued below sea level: it seemed absurd to ascribe the above-water features to fluvial action but to seek some other reason or leave unexplained the underwater extension, given the

inability of a river to erode below sea level. In many river valleys, geologists found countless pebbles of non-local origin; their existence could not be explained by reference to currently-occurring natural processes alone. Giant folds in the Earth's crust, clearly visible in the cliffs of the Isle of Wight, convinced many that Élie de Beaumont's explanation of "epochs of elevation" – the rapid folding and distorting of the Earth's crust – could be a possibility.

In the late 1810s and 1820s, further important pieces of evidence came to light which increased support for geologists who believed that confining explanatory powers only to natural processes still observable was reductive. In Edinburgh, the geologist James Hall found huge parallel grooves which were being erased by weathering, as opposed to being deepened; this led him to conclude that the site of Edinburgh had been drowned by "diluvial waves", several thousand years ago (Rudwick, 2005: 580). At the same time, Leopold Von Buch, a Prussian surveyor working in the Alps, suggested that the only way to account for the distribution of huge erratic boulders, many sited beyond the reach of rivers, even when in flood, was to invoke a huge flood event (Rudwick, 2008: 182). Such was the level of evidence that even John Playfair and K.E.A. Von Hoff, the leading proponents of explaining landforms by reference to currently-occurring natural processes in Britain and the German lands respectively, were forced to concede to catastrophist arguments on some issues.

In the succeeding decade, the 1820s (when Wilkinson was carrying out his fieldwork in Egypt), the evidence to favour the theory that natural processes had once acted more forcefully than they did in the present mounted still higher, and an older idea was resurrected: that these forces had wrought their changes at the same time, in a global cataclysm (Rudwick, 2005; Rudwick, 2008). The two leading geologists in France and Britain, Georges Cuvier and William Buckland, staunchly held this view. Both used fossil evidence to argue that a whole prehistoric mammal fauna had been wiped out by this event, which until then was mainly understood as having altered Earth's topography (Rudwick, 2008: 11-23, 73-87; Rupke, 1983: 31-41). Though Buckland followed Cuvier in this dimension, he also made entirely new arguments, in his book, *Reliquiae Diluvianae* (1823): he interpreted the distribution of pebbles in river valleys in southern England as showing the path that an ancient current of water that had flooded the country had

taken. He also interpreted large superficial deposits as “diluvium”, created during this flood event (Buckland, 1823). Such was the enthusiasm for observations of this sort that Buckland’s biographer has gone as far as terming the 1820s “the diluvial decade” (Rupke, 1983: 9). Buckland equated this conjectural flood event with the Biblical deluge, but even secularists found it increasingly difficult to argue with the theory that there had been some sort of revolutionary event in the relatively recent geological past.

5.1.3 “Catastrophism” in Egypt

Many European earth scientists in addition to Cuvier viewed currently-occurring natural processes as inadequate to explain the major landforms of Egypt as well. In 1793, a French savant, Déodat Dolomieu, published a paper which argued that two of Egypt’s major landforms, the Nile valley and the Nile Delta, had formed during the last revolutionary event, rather than having formed from gradual erosion and deposition respectively (Dolomieu, 1793: 41-61, 108-126, 194-215).

He argued that as the Nile was depositing alluvial silt in its valley, gradually causing the level of the land to rise, it was absurd to ascribe the formation of the valley to the river having eroded its bed over time: the precise opposite was happening. His solution was that the Earth’s crust had fractured during the theoretical revolutionary event, and that the river had only subsequently moved to this feature: it had not created it. He suggested that other landforms, in East Africa and Arabia, were probably formed at the same time.

Dolomieu also pointed out that, contrary to popular belief, the Delta was not a simple expanse of mud; cores had shown that, below the upper layers of alluvial silt, it had an exceedingly complex structure, with areas of limestone rock, and areas filled with coarse sand: neither were being deposited by the Nile any more. Dolomieu suggested that the limestone areas had always existed, while the coarse sand had been deposited in great quantities during the last revolutionary event. Like many of his colleagues, Dolomieu accepted that the Nile was capable of transporting vast amounts of material to its lower reaches each year, but he wrote this off as surface gloss: it was only adding to a pre-existing landform. One part of Egypt’s topography in which Dolomieu allowed deposition a role was in the region of Alexandria, specifically at the Isle of Pharos, a peninsula. Here,

a passage in Homer (*The Odyssey*, Book IV) suggested that the island had once been "a day's sailing" from the coast of Egypt.[10] Dolomieu suggested that Homer had not taken poetic license but had described an ancient landscape that had really existed. For him, subsequent deposition had joined up a series of formerly disconnected reefs to form a bar, on which Alexandria was situated. This in turn had enclosed an area which was formerly sea, into the Lake Mareotis, which was itself subsequently filled in by deposition. However, this episode was relatively unusual: Dolomieu held that Egypt's topography had mainly been wrought during the last revolutionary event. He also pointed to further evidence, like that which geologists had found in Europe, including stones, not otherwise known in Egypt, above the level of the inundation: he suggested that since currently-occurring processes were unable to explain their distribution, a higher-magnitude event must have taken place. Unsurprisingly, Dolomieu's conclusions were received well by Cuvier, who referred to them in his own work, *Essay on the Theory of the Earth*, which was published in England in 1827 (123-124).[11] Wilkinson was inspired by the work of both of these scholars: the first half of this chapter will outline how he ascribed the formation of the Delta to a high-magnitude event, while he saw presently-occurring deposition as responsible for the balance between alluvial land and desert in the Nile valley. In the second half, the validity of the first stratigraphic excavation in Egypt hinged upon whether the sediment at specific sites was formed as a result of annual deposition, or rarer (if not strictly "catastrophic") large-scale flood events.

5.1.4 How far into the Mediterranean? The Nile Delta Region

Having elaborated the geomorphological and geological context, it is now time to turn to how Wilkinson interpreted the Nile Delta, one of the most prominent physical features in Egypt, in terms of the extent to which he believed that still-occurring natural processes

[10] See *Homer The Odyssey*, translated by E.V. Rieu, London, 1946, 50. Dolomieu and Wilkinson used an older edition.
[11] G. Cuvier, *Essay on the Theory of the Earth*, Edinburgh, 1827, 123-124. As Rudwick (2005: 510-511, 601) points out, this was not an entirely literal translation: Jameson, the translator, adapted some elements of the work to make it more commercially appealing.

were sufficient to account for its formation, and what the broader consequences of this were for the study of ancient Egyptian civilization.

Ultimately, scholars who were interested in the formation of the Delta had been inspired by Herodotus. Speculating on the geomorphological history of Egypt in a manner which seems strikingly modern, Herodotus suggested that, in prehistoric times, the area of northeast Africa which was then (and now) occupied by the Nile valley had once been "a gulf of the sea", similar to the Red Sea (2:10).[12] However, it had been entirely filled up with alluvial material, deposited by the Nile. Herodotus speculated that 20,000, or even only 10,000 years, would have been adequate for such changes to occur, remarking "the Nile is a great river and does, in fact, work great changes" (2:11).

As the study of ancient Egyptian geography proceeded, a significant problem in Herodotus's scientific theorising emerged: ancient cities which had been described by classical authorities as coastal were still very close to – in fact generally within a few miles of – the shore of the Mediterranean. According to a theory which ascribed the formation of the Delta to the gradual accumulation of silt over many millennia, these should have been far inland. One partial solution was suggested by Colonel Rennell, a former East India Company soldier and surveyor who later wrote a pioneering work, *The Geographical System of Herodotus* (1800), which attempted to explain geographical peculiarities and problems in *The Histories*, with reference to recent scientific research. Rennell noted that there was exceptionally deep water immediately to the north of the Delta. He reasoned that this had slowed the growth of the Delta, but that it was probably still expanding, albeit underwater. It was an argument which owed more to proving Herodotus correct, than it did to the geological theories of Hutton and Lyell.

Wilkinson was unconvinced however, and advanced a radically different interpretation, at the start of his *Manners and Customs of the Ancient Egyptians* (1837) and again near the start of *On the Nile, and the present and former levels of Egypt* (1839).[13] Like Dolomieu, Wilkinson paid attention to the passage in the fourth book of *The Odyssey*

[12] Herodotus*, The Histories*, translated by A.D. Selincourt, London, 1954, 98-99.
[13] This may have been one of Wilkinson's first pieces of fieldwork in Egypt. A description of much of the following first appears in a letter that he wrote to William Gell in November 1821 (Wilkinson to Gell, "Exercise book, 1821", Bodleian Library Wilkinson manuscripts, dep. d. 51.)

which claimed that the Isle of Pharos had once been "a day's sailing" from Egypt. He suggested that when Homer used the word "Aegyptus", he had actually been referring to the Canopic mouth of the Nile, and not simply to the shore. This was an intuitive interpretation, and one which a traveller in the early 19th century could appreciate: before the construction of the Mahmoudiah canal between Alexandria and Cairo, travellers between the two cities were forced to sail a significant distance along the treacherous north coast of Egypt, as well as along the Nile (Thompson, 1992a: 34; Thompson, 2010: 30).[14]



Fig. 5.2. Alexandria in northwest Egypt, with red highlighting to show the historic environment, in classical times, as it was perceived by Wilkinson and others in the early 19th century. The Isle of Pharos (now extended so as to enclose the seaward side of the port of Alexandria) is in the centre and was connected with the mainland (on which the city of ancient Alexandria was sited) by a narrow causeway (the Heptastadion), later enlarged by land reclamation. Source: Google Earth.

As well as these literary-based arguments, Wilkinson also carried out practical fieldwork, from which he concluded that the Nile Delta in the vicinity of Alexandria had remained unchanged since antiquity: it had not expanded (Fig. 5.2). Wilkinson noted that

[14] The canal opened in 1819, so just in time for both men to use it.

Alexandria stood "on the rock of the Libyan desert" (1839: 433). Like the rocky Pharos, this area of ground had always existed – it had not been deposited by the Nile. His account in *Manners and Customs* was slightly more extensive. On the basis that the annual deposition in the harbour at Alexandria was "very trifling", Wilkinson concluded that the Isle of Pharos had become attached to the Egyptian mainland only due to artificial infilling along the Heptastadion – an artificial causeway which had been built to link the Pharos to the mainland in classical times (1837, vol. 1: 7). Wilkinson went on to include one last line of argument: whereas it was possible to observe sediment accumulating around vegetation at the mouth of many rivers, "this peculiarity is totally unknown at the mouth of the Egyptian Nile" (1837, vol. 1: 10). Overall, Wilkinson's conclusion, forwarded both in book and journal article format, agreed with Dolomieu's work in its broad outlines. It had also been reached by diligent study of the natural process of deposition. Wilkinson had not denied that it was occurring, but found it inadequate to explain large-scale landscape change. It was an observation which he later extrapolated to the rest of the Delta.

He departed from mainstream geology on a significant point however: whereas geologists invariably went on to discuss their work in relation to Earth history, Wilkinson always linked his discussions of physical geography back to historical research questions. His extensive discussion of the formation of the Nile Delta at the very start of *Manners and Customs of the Ancient Egyptians* (1837) is a case in point. This book was essentially an anthropological investigation into ancient Egyptian traditions and culture – both domestic and national. It started in an entirely intuitive way, with a section about the origin of the ancient Egyptians as a people – a research topic which nearly all subsequent historians of Egyptology have traced only as far back as Flinders Petrie's excavations at Naqada in the late 19th century (Midant-Reynes, 2003).[15]

In *The Histories*, Herodotus's speculation about the origins of the Egyptians – whom he regarded as a homogeneous group – was fairly simplistic. He claimed that, because Lower Egypt (the northern Delta region) had formerly been an impenetrable swamp, the Egyptians must have originated in the south, in Upper Egypt, and only later spread to the

[15] Wortham's *The Genesis of British Egyptology* is again an exception, though he only refers to excavations of prehistoric graves in the 1850s.

north of the country, including the Delta region, after deposition had rendered it habitable. Wilkinson however disputed this south-to-north model. He believed – in line with most other scholars in the early 19th century (Daniel, 1962; Haycock, 2002: 136-188; Rudwick, 2005: 600-601; Rudwick, 2008: 79) – that humans, had originated in Asia and then spread outward, including to Egypt.

It was here that his geomorphological research became relevant: if his (and Dolomieu's) theory – that the Delta was exceptionally old, having been present in roughly its present form for many millennia, as opposed to having grown gradually – was correct, then it followed that the earliest Egyptians could have originated in southwest Asia.[16] Wilkinson did not limit himself to such an environmentally deterministic argument alone, however, and informed his readers "nor would any one be permitted to assert the priority of a *nation* from the apparent antiquity of the soil which clothes the rocks of the country" (Italics in original text (1837, vol. 1: 11)). In a subsequent reissue of *Manners and Customs* (1841), Wilkinson was more committal and he explicitly sided with James Cowles Prichard, the leading proponent of monogenism (tracing all of humanity to a single place of origin), who had also published in the *Journal of the Royal Geographical Society* (Prichard, 1839). Impressed by apparent religious and linguistic similarities between the Egyptians and the Hindus, he suggested that the two groups had not influenced each other directly, but bore similarities due to having had "a common origin at a most remote period" (1841: v) – an observation that had first been noted by Sir William Jones in India (Stocking, 1987; Allen, 2002). Wilkinson's contribution was to offer evidence sourced from geomorphology: arguments had previously been based on linguistics and comparative anatomy. None of this was a major part of *Manners and Customs*, but discussing the origins of the ancient Egyptians was an intuitive way to start a book about their culture.

Interestingly, the nature of the Nile Delta in early times was not simply a question debated solely between classical scholars with competing interpretations of Herodotus, but was one which had become quite topical in the mid-1830s, with the publication of the Biblical scholar and explorer Charles Tilstone Beke's *Origines Biblicae, or, Researches*

[16] Wilkinson thus assumed that the earliest colonizers had travelled by land along modern communication corridors near the Mediterranean, rather than those nearer to the Red Sea.

in Primeval History (1834). Although not widely read today, such Biblical scholarship was extremely popular with the 19th century public (Rudwick, 2008; Gange, 2013). Beke's revisionist thesis, drawing on Lyell, suggested that the Nile Delta had until recently been a marshland which had not been fit for human habitation and that the events of the Old Testament had actually occurred in Sinai (1834: 167-176; 267-276). Wilkinson may not have been able to deal with this novel idea in *Topography of Thebes, and General View of Egypt* (1835), but it is quite possible that he was aware of it by the time of *Manners and Customs* (1837), so chose to include a swipe at it at the start of this. This controversy was a quite different one from that of the origin of the earliest Egyptians that Wilkinson was mainly interested in, but – given that the events of the Old Testament had occurred later than the first peopling of Egypt – it was one which could benefit from applying the same newly-established geological information.

5.1.5 A Mega-Egypt or a micro-Egypt? Wilkinson's geomorphological fieldwork at Thebes

Wilkinson's second concern, after the ancient extent of the Nile Delta, was with the ancient extent of the cultivated land in the centre of the Nile valley. In *On the Nile* (1839), he recounted having undertaken fieldwork from which he concluded that the annually inundated part of the Nile valley, on either side of the river itself, had advanced outward into the desert, over the millennia since the Pharaonic era. Far from believing that the environment in the Nile valley had always been static, Wilkinson appreciated that it had undergone dynamic changes. Whilst most of the discussion below will be drawn from his article, occasional reference will be made to Wilkinson's other published works in which he elaborated further on the same issues: the repeated appearance of this material is further testimony that Wilkinson regarded his geomorphological research as of great importance. Unless otherwise stated, however, the focus will be on Wilkinson's journal article, *On the Nile* (1839).

As with the Delta, Wilkinson was not in fact the first to have speculated on this aspect of environmental change. Herodotus had observed that the level of Egypt above the sea increased by a small amount each year, due to the deposition of a layer of silt, as a result

of the annual inundation; he had also expressed a concern for the future of Egyptian agriculture, noting that a greater rise in the Nile (measured in cubits) was required each year for the inundation to reach the increasingly elevated fields. If this did not happen, Egypt would be reduced to sterility as the Nile would be unable to irrigate the crops (2.13-14).[17]

In more recent times, as early as 1721, Thomas Shaw, a British traveller visiting Egypt, had been inspired by Herodotus to investigate the same issue. Nearly two decades later, he published an account in *Travels, or Observations Relating to Several Parts of Barbary and the Levant* (1738). His research design assumed that the Nile's discharge and sediment load had remained constant, and he concurred with Herodotus that the land on either side of the Nile had indeed risen over time, as a result of the deposition of silt, during each of the annual inundations.

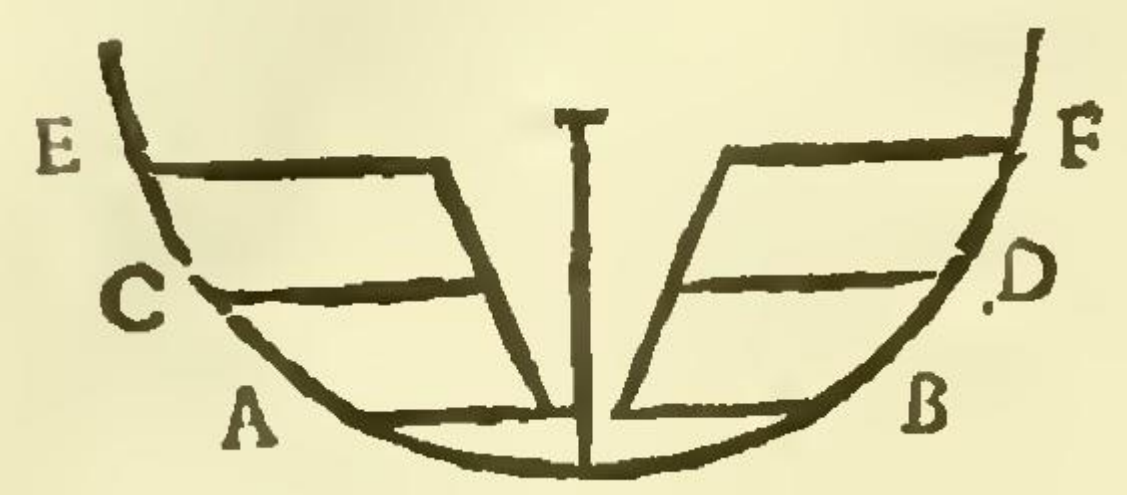


Fig. 5.3. Shaw's cross-section, with vertical exaggeration, of the Nile valley, showing the result of several millennia of deposition: initially Egypt was at the level AB, but had subsequentially reached CD and EF. Source: T. Shaw, *Travels, or Observations Relating to Several Parts of Barbary and the Levant*. Oxford, 1738, 438.

Shaw constructed a thought-experiment, with an accompanying diagram, consisting of a cross-section of the Nile valley (Fig. 5.3), to illustrate his theory. He suggested that, initially, the distance from A to B, or "the whole land of *Egypt*" (italics in original) had

[17] *Herodotus, The Histories*, translated by Selincourt, 99-100.

been covered by water, even during a modest inundation (Shaw, 1738: 438). After a thousand years or so, the level of Egypt had risen, due to the annual deposition of silt, to the level of C-D. By the time the land had risen to the level of E-F, Shaw suggested that "a still greater height of water would be required to refresh them [the fields]; which, in the time of *Herodotus*, was sixteen cubits" (Shaw, 1738: 438).[18] Shaw expressed some confusion as to whether the modern "cubit" was the same as the ancient Greek one, but eventually, after a lengthy set of measurements suggesting how much Egypt had risen since the time of Herodotus, he summarised the section by concluding that "at present, the river must rise to the height of 20 (*Stamboline*) cubits, (and it usually rises to 24) before the whole country is overflowed" (italics in original) (Shaw, 1738: 441). In other words, far more water was needed for an agriculturally-useful inundation in modern times, compared to Herodotus's time. The implications of this were startling: Shaw suggested that there would eventually come a time in which the level of Egypt would be raised to a height at which "the river will not be able to overflow its banks; and *Egypt* consequently, from being the most fertile, will, for want of the annual inundation, become one of the most barren parts of the universe" (Italics in original (1738: 441)). In other words, he agreed with Herodotus's apocalyptic prediction about the future that Egypt faced. Although Shaw's failure to distinguish between the plain of Egypt and the (raised) levees on either side of the Nile is a significant omission, it was nevertheless a serious attempt, which drew on some fieldwork, into investigating environmental change in the country.

Speculation about the slow desiccation of Egypt was not limited to Shaw. The idea was taken up by others, over the course of the 18th and early 19th centuries. In his first topographic work, Wilkinson referred to two of these, Claude-Étienne Savary and Dolomieu (1835: 339-340). Savary believed that Egypt was drying out on the basis that when the Nile was in flood, the villages on their raised mounds did not resemble islands in the sea as Herodotus had described. Dolomieu had the same end view in mind, but suggested that the issue was not so much that the land had risen too far above the Nile to be flooded, but that the discharge of the Nile had fallen since ancient times. He

[18] The cubit, which will be referred to several times in the below, was an ancient Egyptian measurement equivalent to 0.524m (20.625 inches) – see Wilkinson (1835) *Topography of Thebes*, 314.

ascribed this to the erosion of mountains in Ethiopia in the upper catchment of the river; the relationship between high relief areas and rainfall was understood, so this was theoretically plausible.

In stark contrast to his predecessors, Wilkinson made a strongly divergent claim: the fertile Nile valley was actually increasing in size (except, as noted above, in the Delta (1839)). He implied that the area of cultivated land in ancient Egypt had not extended as far across the Nile valley as in modern Egypt. Wilkinson's evidence base was much broader than that possessed by his predecessors: whilst they had only made fleeting visits, he drew on fieldwork which he had undertaken throughout Egypt, during his twelve-year sojourn in the country. As was the case for his research question about the geographical origin of the ancient Egyptians, it is notable that Wilkinson's research question here – about the extent of ancient Egypt – is one which draws on antiquities, in order to prove the action of geological processes. These are then used to answer a historical research question. After examining Wilkinson's geomorphological research, attention will therefore be given to the wider conclusions that he deduced from it.

The mainstay of Wilkinson's argument that the fertile part of the Nile valley was increasing in extent was that the bed of the River Nile was increasing at the same rate as the level of the rest of the floodplain that comprised Egypt, so the relative difference between the two stayed constant across the millennia. (He assumed that the discharge of the river likewise remained unchanged, as Shaw, but not Dolomieu, had done.) In his article, he included a diagram to convey the information in the text of the article (Fig. 5.4). Summarised briefly, Wilkinson suggested that the level of the ancient bed of the Nile was once at A-B, but it had risen (by an unspecified height) to the level of C-D. The

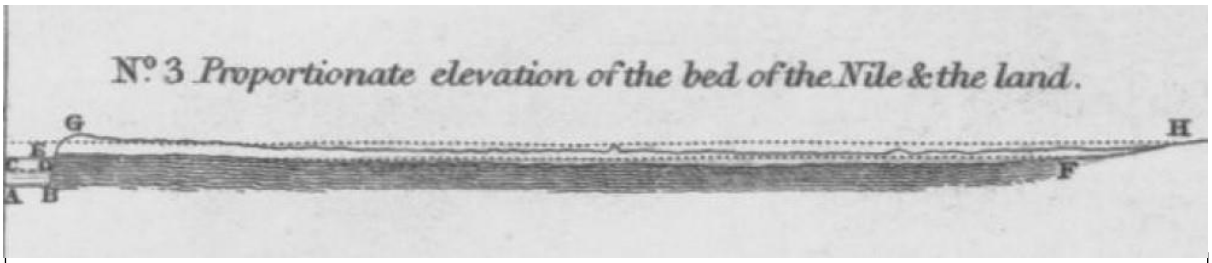


Fig. 5.4. Wilkinson's cross-section, without vertical exaggeration, of half of the Nile valley, showing the result of several millennia of deposition: when the Nile was at AB, it deposited material across EF, but when the river bed rose to CD, it deposited material across GH. Source: J.G. Wilkinson, On the Nile, and present and former levels of Egypt, *The Journal of the Royal Geographical Society*, 9 (1839) 431-441.

floodplain adjacent to the river had risen in proportion, from level E-F to G-H respectively (the inundation is signified by a fine dashed line, just above the newly-risen floodplain).

In a simple cross-section in which the width of the Nile valley is compressed (i.e. there is vertical exaggeration), such as that given by Shaw (Fig. 5.3), the impression would be that the area of land inundated by the Nile would remain fairly constant. However, the cross-section supplied by Wilkinson (Fig. 5.4) shows the Nile valley – or at any rate half of it – in its correct proportions: the Nile is barely visible on the extreme left-hand side, while the floodplain to one side of it is shown occupying a space approximately 25 times as wide. This has the effect of compressing all but two of the eight letters into a tiny space, with the consequence that they are very hard to read. However, it conveys the point that Wilkinson is making extremely well – far better than a cross-section with vertical exaggeration like that of Shaw's could ever do – by highlighting a perceptible horizontal (i.e. not just vertical) difference between F and H: this signifies newly-inundated – and hence fertile – land suitable for agriculture, making Wilkinson's point seem indisputable. Out of the nine cross-sections on the accompanying plate, only this one departs from using vertical exaggeration.

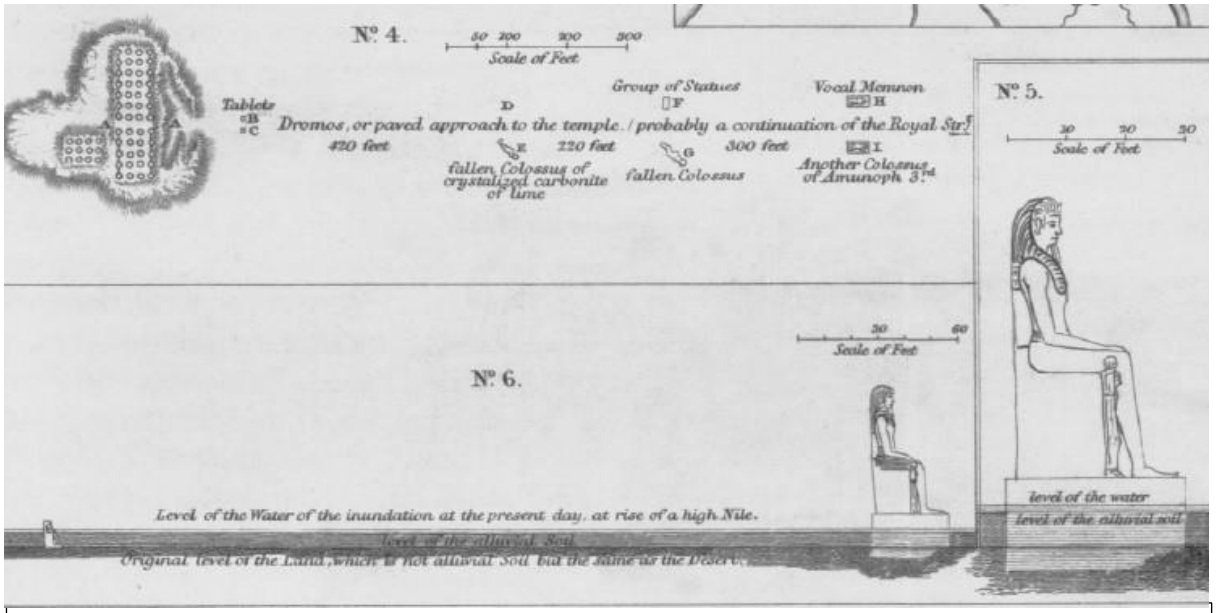


Fig. 5.5. Plan view and cross-section (with close-up of one of the two Colossi of Memnon) of Wilkinson's transect from the Colossi of Memnon to the Temple of Amenhotep III. The distance between the two is the equivalent of FH in Fig. 5.4, though the Nile is off the right hand-side of this image, rather than on the left as in Fig. 5.4. Wilkinson included similar images in his books. Source: J.G. Wilkinson, On the Nile, and present and former levels of Egypt, *The Journal of the Royal Geographical Society*, 9 (1839) 431-441.

That this exercise was not just a thought-experiment is made clear in the following part of the article, which revolved around a piece of fieldwork, carried out by Wilkinson in 1830, on the west bank of the Nile at Thebes. Following what he believed to be an avenue, or "dromos", leading from the Colossi of Memnon to the Temple of Amenhotep III, which stood in the desert further to the west, Wilkinson formulated the idea that he could map out a transect from one end of the avenue to the opposite end, and measure the depth of alluvial silt at intervals along it (Fig. 5.5). He confirmed his expectation that the silt would be deepest near the Colossi of Memnon and shallowest at a group of statues in front of the Temple of Amenhotep III (the figures for the depth of silt were 6 feet 10 inches to 7 feet, and 5 feet 3 inches respectively).

The finding, that the alluvial deposits were thickest in the region adjacent to the Nile and became shallower successively further away, was not new. A century before, Shaw had noted the same phenomena and assigned figures to it: the area nearest the Nile in feet; areas furthest away in inches. Wilkinson's contribution was to assign rough dates to

it. Finding sandstone paving slabs and a sandy soil in a pit which he excavated next to one of the colossi, at the end of his transect nearest the Nile, he concluded that the statue had stood at an unknown level above that of the inundation when it was built, around 1420BC. Given that the statue was about 15 metres high and weighed several hundred tonnes, it was unlikely that it had been moved in the intervening period. Any alluvial silt which had accumulated to the west of it, towards the temple, must intuitively have accumulated over the course of the 3,250 years since it was built. Wilkinson found that alluvial silt had accumulated as far as 1,900 feet west of the colossi, that is to say in the vicinity of the temple. This gave a concrete – and impressive – figure for his theoretical F-H distance in his model. Wilkinson noted it was also a conservative one: if the colossi had stood well above the level of the inundation when they were built as opposed to just above it as he had implied, then the figure for the 3,250-year period would have been even greater (Fig. 5.6).

Besides this transect at the Colossi of Memnon, Wilkinson had two other case studies – an obelisk covered in what he believed was alluvial mud at Heliopolis and a group of Roman-era statues covered by alluvial mud at Aswan – which he mentioned in *Topography of Thebes, and General View of Egypt* (1835: 313, 462) but referenced only as other "ancient monuments" in his article (1839: 435). Nevertheless, he did not hesitate in extrapolating from his fieldwork observations to the whole of Egypt: if it was not already clear, he confidently asserted that

> the perpendicular rise of the bed of the Nile extends the inundation and alluvial deposit much farther in a horizontal direction, E. and W., at the present day than at any previous period; that it has always continued to do so; and that, therefore, there is a wider extent of irrigated land now than in former times (1839: 437).

Wilkinson believed that this would continue in the future, in opposition to the doom-laden forecasts of Shaw and others.

Technically Wilkinson's model had not addressed, let alone refuted, the theory that the cultivated land would eventually be elevated to a level higher than that which the annual inundation could reach. To satisfy lingering doubts, at least in his mind – the issue is addressed in Wilkinson's *Topography of Thebes, and General View of Egypt* (1835) but

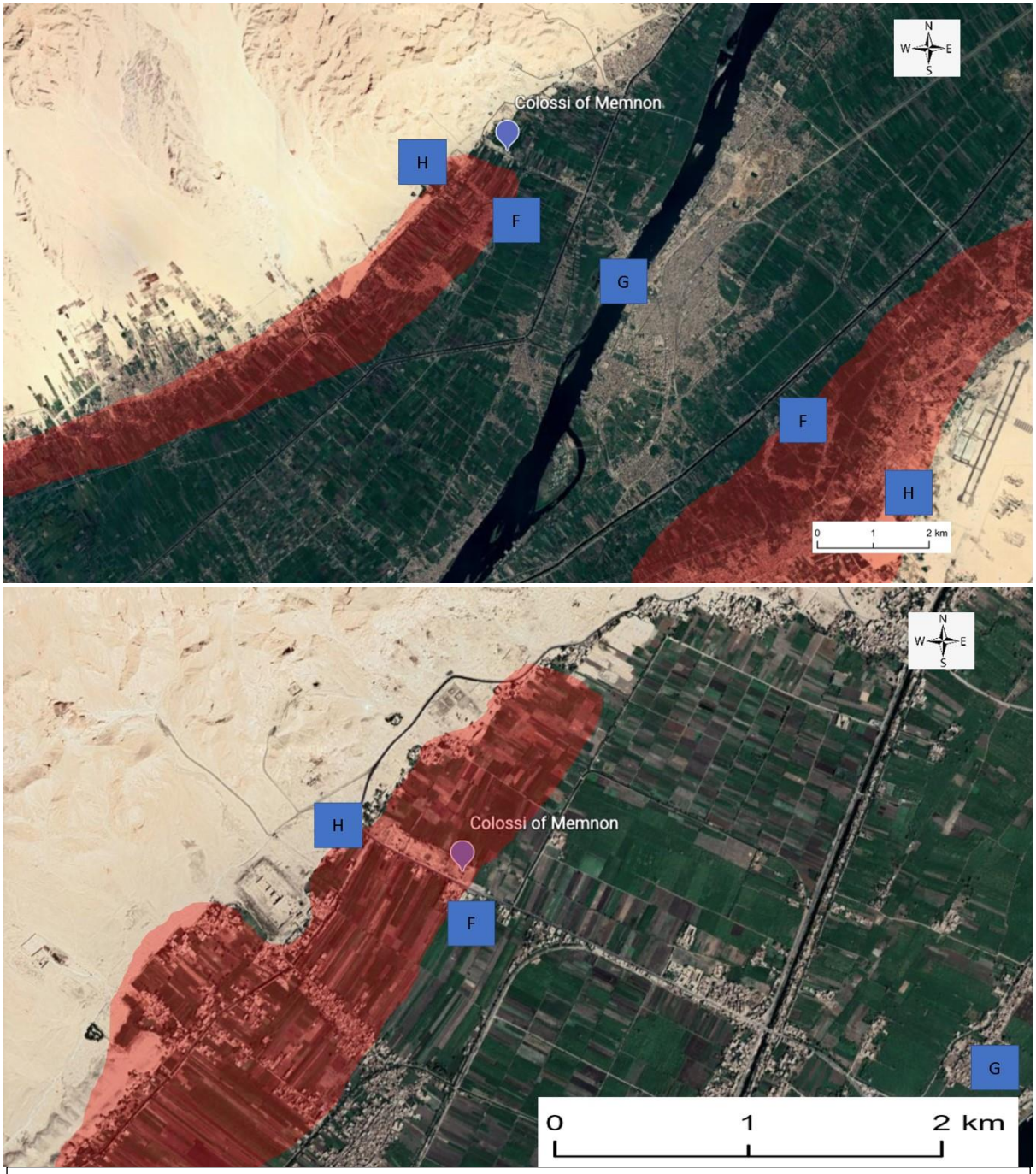


Fig. 5.6. Satellite view and close-up satellite view of Wilkinson's transect from the Colossi of Memnon westward to the temple of Amenhotep III, with red shading shows the approximate minimum extent of the new agricultural land which Wilkinson concluded had come into existence during the intervening 3,250 years, due to the Nile depositing an ever-increasing amount of sediment in its valley, between F and H – the Nile is at G (see Fig. 5.4). Wilkinson did not supply a map and appears to have only carried out fieldwork on the west bank; this image shows how he envisaged the ancient landscape and his extrapolation to the wider Nile valley. Source: Google Earth.

strangely not in his article published four years later – Wilkinson contended that the reason that a higher flood (measured in cubits) was needed in the 19th century than was the case in Herodotus's time was simply because the modern flood was measured from an artificially high baseline. He derived this conclusion from a study of the Elephantine Nilometer (a huge cistern-like chamber located adjacent to the Nile, into which the Nile waters flowed during the inundation, so it could be measured: these measuring stations were famously used in the Pharaonic era to decide on how much to tax the country). Observing that the base of the Nilometer had not been dredged, Wilkinson concluded that the same level of water would necessarily rise to a greater height in the chamber, giving an excessively high measurement. This rendered comparisons with Herodotus meaningless.

The question of whether the Nile was still able to flood regions that it had previously reached was also mediated to some extent by Wilkinson's noting that the land around the Colossi of Memnon was so waterlogged due to the inundation that they were inaccessible for months. Particularly geographically-aware viewers of the image that Wilkinson included at the start of the Second Series of *Manners and Customs* (1841) – just before his discussion of the wider historical implications of his project (see below) – might also have noticed that the unusual rear perspective from which Wilkinson chose to depict the Colossi of Memnon demonstrated that the inundation still extended a considerable distance beyond them to the west (Fig. 5.7): that is to say toward the temple of Amenhotep III at the desert edge of Wilkinson's transect. Thus the possibility that Egypt's irrigated land might be left high and dry in the future seemed unlikely.

Overall, this amounted to a sound demonstration of the power of one still-occurring natural process – deposition – to greatly alter the surface of Egypt, over a relatively short timespan during the historical period. On the face of it, Wilkinson's work at the Colossi of Memnon was perfectly in line with – or possibly even anticipatory of – Lyell's brand of extreme uniformitarianism. Such a reading is however simplistic (even though Lyell would have approved of Wilkinson's work). Wilkinson made no mention of Lyell, but referred to "catastrophist" geologists such as Dolomieu. Such geologists subscribed to an interpretation of uniformitarianism, or "the present is the key to the past" – including

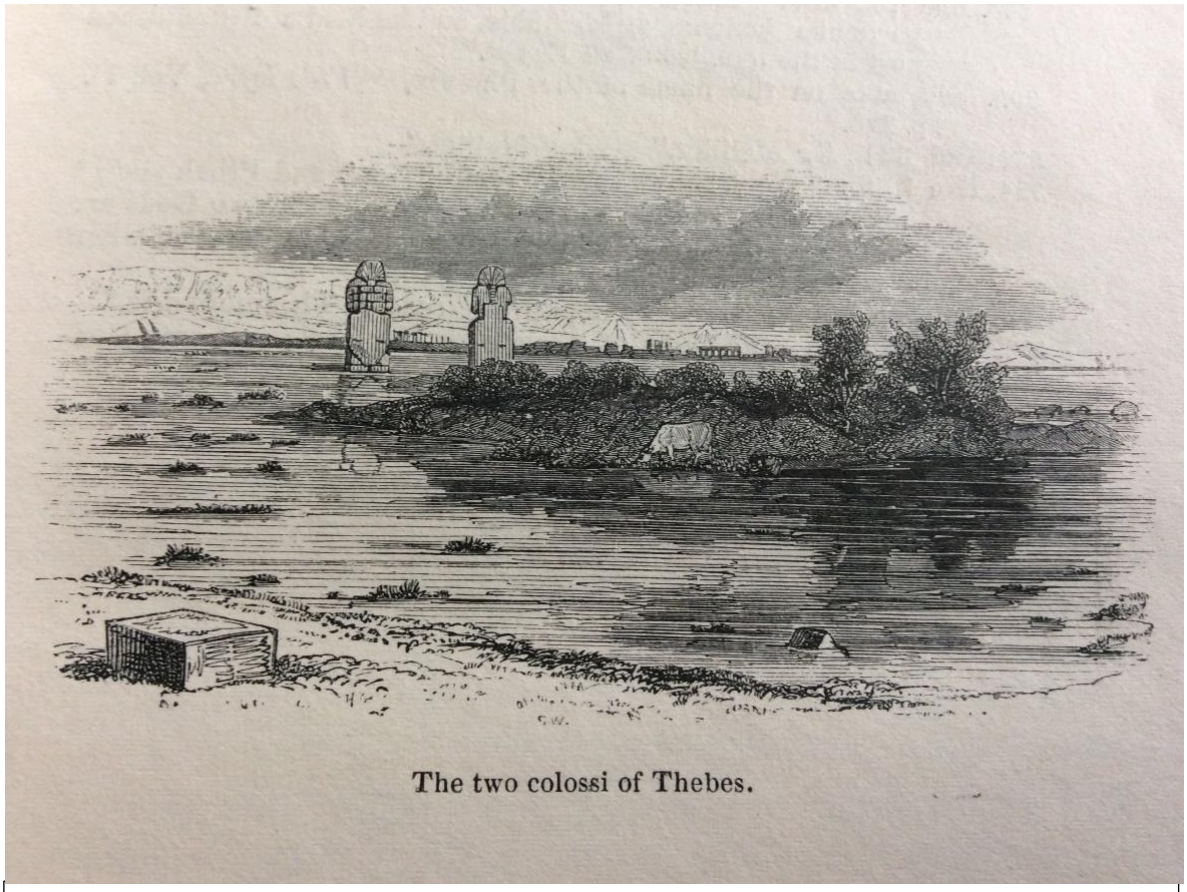


Fig. 5.7. Image of the Colossi of Memnon, from the temple of Amenhotep III, looking east, towards the Nile and east bank. Source: J.G. Wilkinson, *Manners and Customs of the Ancient Egyptians*, 2nd series, vol. 1, London, 1841, xxix.

ideas that natural laws had stayed constant, that it was valid to extrapolate back from present-day observations, and that these observed processes had stayed fairly steady – which was already consensus in the 1820s: it was not one which Lyell introduced in 1830. The point at which they differed from Lyell's thinking was that they refused to accept the possibility that only observable processes could be invoked, as there were features which could not be explained by these (Gould, 1987; Summerfield, 1991: 4; Rudwick, 2008). One of these was the Nile valley itself: according to arguments forwarded by uniformitarian geologists, rivers were supposed to erode their valleys, not fill them up. If the Nile was discounted as an erosive agent, then a catastrophic event, such as a major seismic upheaval larger than any earthquake, was the only factor which could be enlisted for the formation of the Nile valley. Even if Wilkinson did not address this particular issue in his experiment, it was a theory which he appears to have supported, and his research could only bolster the theory, thus aligning him with Dolomieu and Cuvier. To summarise the wider geological context, Wilkinson was intervening in a debate taking place between

catastrophist geologists, and not between catastrophists and uniformitarians (as they would later be called). That his research design would have been entirely acceptable to Lyell or any other proponent of extreme uniformitarianism is of incidental as opposed to central importance.

Wilkinson's geomorphological research into the ancient extent of the Nile valley in relation to the desert was not simply a revision of gloomy predictions for the future of Egyptian agriculture, regardless of its sensationalism: as with the Nile Delta, his fieldwork was ultimately brought to bear on research questions in ancient Egyptian history. In the Nile Delta, Wilkinson had been interested in Egyptian prehistory. In the Nile valley, however, the topic in question was the historic strength of the ancient Egyptian state, and its political economy. Technically this was not something that Wilkinson discussed in his *On the Nile* article. This may have been because on the publication of an earlier article, *Notes on the Eastern Desert* (1830), he had been dismayed to find that someone at the Royal Geographical Society had edited out nearly all of his antiquarian material to create a paper which was almost solely restricted to physical geography, a direction which the newly-founded RGS was moving toward. However, in two of his books, Wilkinson positioned a historical section, discussed below, immediately prior to a section on agriculture and the abovementioned exploration of environmental change, suggesting that he saw the two as intimately linked: to him, antiquarianism and the natural sciences were allied approaches that could mutually inform each other, rather than entirely separate disciplines (1835: ix-41; 1841, vol. 1: 1-112).[19]

At first glance, Wilkinson's argument that the Nile valley had been smaller in ancient times appears to conflict with his contention, repeated throughout his work, that ancient Egypt had been more powerful in the Pharaonic era than at any time in its subsequent history. In fact, Wilkinson's geomorphological fieldwork was entirely in accordance with his view that ancient Egypt had been a mighty state, as he avoided a crude deterministic argument of linking a year-on-year increase in available agricultural land with a year-on-year increase in power altogether (which would have implied that Egypt in antiquity, with a smaller agricultural base, would have been comparatively weak). Instead, he argued

[19] The interlinkages between antiquarianism and natural history are discussed in Heringman (2013).

that, if classical authorities who reported a population in excess of seven million people in Graeco-Roman Egypt were correct, then the ancient Egyptians must have pioneered agriculture at an early date, and it was a well-managed intensive farming base that formed the foundations of the Egyptian state's power. As Wilkinson explained, "hunters" and "shepherds" were only able to produce enough food for their own family, but farmers were able to produce a surplus. This in turn generated a surplus labour force, which could be drafted into an army or a bureaucracy. Wilkinson even took his argument as far as suggesting that Egypt became a manufacturing power, producing "fine linen … cotton … woollen stuffs, cabinet work, porcelain, glass, and numerous branches of industry".[20] The result was that Egypt was able to dominate its neighbours as a result of this head-start in agriculture and industry – factors which were more important than land area alone – although in time, the country's comparative advantage was eroded as others caught up with, surpassed, and conquered it.

Wilkinson's explanation owed much to the numerous multi-stage models of human development that had been devised by philosophers and economists in mid-18th-century France and Scotland, which posited societies as transitioning from hunting and fishing, to pastoralism and, finally, agriculture and commerce, as a result of population pressure (Meek, 1976; Stocking, 1987). As the mode of subsistence changed, it was assumed that the way in which society was organized changed too, invariably becoming more complex, with the development of manners, laws, punishments and institutions over time. Strictly speaking, Wilkinson did not note precisely which system he was following, although given that these conjectural systems had permeated popular culture by this time, this is perhaps a less important issue than his evidently following the general idea, which fed into his wider project, as was described in the previous chapter.

If Wilkinson's geomorphological research in the Delta had lent legitimacy to the thesis that the ancestors of the Egyptians had migrated from southwest Asia to northeast Africa, then his work at Thebes posited that the development of Pharaonic civilization itself was autochthonous: it had gradually grown up in the Nile valley. To some extent, these conclusions about ancient Egyptian political economy, and its implications for

[20] Wilkinson, *A Second Series of the Manners and Customs* (1841: 5).

Egyptian foreign policy, corroborated what was already known about Egyptian agriculture, to Wilkinson and others, from classical authors, the Bible (notably the story of Joseph), and rural scenes in Egyptian tomb paintings. Wilkinson's contribution was to add an additional form of empirical evidence to what had formerly been mostly conjectural history. In this regard, his approach was similar to that of Thomas Pownall and other antiquaries of the 18th century (Sweet, 2004: 24).

Wilkinson's conclusions were important on another level too. Namely, Wilkinson's model of Egyptian development would have been easily recognisable to the average educated Briton, who was aware of industrial changes taking place in Britain itself at the time, which were revolutionising manufacturing of nearly all of the articles in which Wilkinson had noted ancient Egyptian excellence: textile manufacturing was beginning its move to become concentrated in Derbyshire, Lancashire and the West Riding of Yorkshire, while the Potteries in Staffordshire had emerged as a centre for ceramic production in the mid-18th century (Trindler, 1982). Although Wilkinson left this implicit in his earlier work, he drew out such parallels in his guide to the Egyptian galleries of the industry-themed Crystal Palace, in *The Egyptians in the Time of the Pharaohs* (1857b).

Presenting the ancient Egyptians as similar to Georgian and Victorian Britons was in fact a running theme throughout Wilkinson's *Manners and Customs*. In part, his project was a success because it challenged the cruel Biblical image of the ancient Egyptians, and replaced it with a domesticated one: the numerous woodcut images in the book showed the ancient Egyptians enjoying themselves by playing games and musical instruments, and even using recognisably modern furniture and utensils (Thompson, 1992a; Gange, 2013). The overall impact was similar to that which emerged from Pompeii and Herculaneum (Sweet, 2012: 164-198; Ouditt, 2013: 41-59). Essentially, Wilkinson's geomorphological conclusions allowed him to show that it was not just the material culture of ancient Egypt that was curiously modern, but that the economic conditions in which it was produced and used, were also similar. To use modern parlance, Egypt had undergone something of an agricultural and industrial revolution just like Britain: a conclusion which could well have helped Wilkinson's attempt to reduce the sense of otherness of the ancient Egyptians still further.

5.2 A Geologist and an Egyptologist in Conversation: Lyell and Wilkinson

5.2.1 Background: Hekekyan's stratigraphy in history

Three decades after carrying out his own fieldwork in Egypt, Wilkinson remained interested in how geological and geomorphological research could, if carried out with sufficient care, bring insights into historical research questions: he engaged in a lengthy correspondence with the eminent geologist Sir Charles Lyell over a recently-conducted stratigraphic excavation programme, conducted by Joseph Hekekyan, an English-educated, Armenian engineer-turned-archaeologist, based in Egypt, on behalf of Leonard Horner, a factories inspector who conducted geological research in his limited free time. In fact, it was Lyell who had initially contacted Wilkinson. On 25 November 1861, he penned a letter to Wilkinson, remarking "I have been studying your handbook of Egypt with great pleasure & instruction but I have been seeking for information which I could hardly have hoped to find & what perhaps no Egyptologist can furnish me".[21] His reason for making contact was that Horner's and Hekekyan's study, into the length of time that the Nile valley had been occupied by prehistoric humans, had been heavily criticised and largely discredited by mainstream geologists and other interested parties. Lyell did not simply become involved to save the reputation of his relative; the uniformitarian methodological assumptions were ones in which he was heavily intellectually invested. Effectively, Lyell viewed Wilkinson as possessing specialist regional knowledge, which might help to settle the dispute, one way or the other. Over the course of their correspondence, which extended across November and December 1861, Wilkinson was able to supply crucial information about a local situation in which generalist theories like uniformitarianism had profound limits, to the extent that Lyell himself was forced to admit that the assumptions central to the Horner-Hekekyan investigation were flawed, and hard to rehabilitate.[22] Horner also backtracked.

This issue – that the Horner-Hekekyan investigation was dismissed due to its profound scholarly shortcomings – is worth emphasising, because stratigraphy has frequently been

[21] Sir Charles Lyell to John Gardner Wilkinson, 25 November 1861, p. 1 (National Trust: Calke Abbey). Both men had entered Exeter College at Oxford University at the same time, in 1816, and were members of the *Athenaeum* club in London, though it appears that they had not previously corresponded.
[22] Wilkinson also wrote three further letters: one in August 1862 and two more in February 1863 – the latter two after Lyell sent him a copy of his *The Geological Evidences of the Antiquity of Man* (1863).

held up as the gold standard of good excavation practice (Daniel, 1975; Renfrew and Bahn, 2004).[23] Whether an excavator paid attention to stratigraphy has automatically been viewed as an indicator of whether their contribution was a serious one. Frequently, it has been presented as the central technique that separates archaeologists off from their antiquarian predecessors: two prominent historians of archaeology, Alain Schnapp (1996) and Paul Bahn (1996), take this perspective. In histories of human antiquity, a similar narrative exists: antiquaries have been presented as apathetic at best and obstructionist at worst in responding to the growing evidence base for stratigraphy and investigations into a lengthy human prehistory (Van Riper, 1993; Briggs, 2007).

The elevation of Hekekyan due to his stratigraphic excavation programme features in many histories of Egyptology, which largely present the discipline as having formed as a result of antiquarian amateurs giving way to professional and scientific archaeologists or Egyptologists – with the latter requiring no direction from the former (Greener, 1966). Jason Thompson, in his biography of Wilkinson, regards stratigraphy as a key factor which separated Wilkinson and his other antiquarian colleagues from later archaeologists: in association with a remark on stratigraphy, he notes that "[Joseph] Hekekyan Bey conducted the first recorded excavation" (1992: 87, 249). By contrast, Wilkinson and his colleagues simply did digging. David Jeffreys' research undertaken in the 1990s, published as *The Survey of Memphis VII: The Hekekyan Papers* (2010) and elsewhere, has brought about a widespread appreciation of Hekekyan's archaeological fieldwork, with particular focus on his meticulous record-keeping of the context of artefacts (Jeffreys, 1999; 2010; 2012).[24] Following on from Jeffreys, other Egyptologists and historians of the subject have given Hekekyan ample praise: Donald Reid has noted that his work at Heliopolis was "the first recorded stratigraphical excavations in Egypt… far in advance of [Auguste] Mariette, who merely kept a list of finds" (2002: 62). Stephen Quirke's accolade, that Hekekyan's method was up "to a standard unparalleled in world archaeology prior to the Second World War", is even more positive (2001: 74). The sum total of this scholarship has been to give the impression that Hekekyan was a forgotten pioneer, whose contribution to

[23] This study does not seek to critique the concept of stratigraphy, the premise is that stratigraphy could be done badly, or misinterpreted.

[24] Jeffreys also suggests several reasons for Hekekyan receiving less attention in some of the older historiography than he deserved, including how Hekekyan's inability to attend academic forums in Britain in person would have put him at an intellectual disadvantage. Gold (2019) develops this theme further.

early Egyptology requires urgent acknowledgement. Although these scholars are correct to give such praise to Hekekyan, their rehabilitation reinforces the narrative that Egyptian archaeology progressed linearly, and that the first stratigraphic excavations were the key turning point. However, as argued by Bruce Trigger (1989) more broadly, there is a need to distinguish between such observation on the one hand, and interpretation on the other, and Hekeyan's fieldwork was interpreted and published by Leonard Horner. During this process, Horner enlisted Hekekyan's stratigraphy for quite different ends.

Most recently, Meira Gold has highlighted this distinction, in research which repositions the Horner-Hekekyan excavation programme as a geological enterprise that took place in Egypt, but which was independent of the formation of Egyptology as an academic discipline (Gold, 2019). Gold has sought to build on current explanations of why the Horner-Hekekyan excavation programme met with disapproval among contemporaries, by suggesting that there were doubts as to whether long-distance fieldwork was a viable approach; Gold also suggests that there was distrust in Hekekyan; and that the results of the investigation were seen as threatening to Biblical chronology and that this was the major factor in the discrediting of the excavation. Such concerns are certainly evident for at least one critic, sufficiently so as to suggest that one earlier commentator, G.L. Davies, was overly hasty when he dismissed Horner as 'suffering from a strange delusion' (1968: 10) in regard to his belief that he had to contend with the Mosaic chronology: evidently such concerns lingered in some circles.

Whilst noting this important dimension, a balance needs to be struck between academic debates on the one hand, and cultural context on the other, even whilst acknowledging overlap between the two. Whilst the programme undoubtedly attracted more attention than it otherwise would have done due to its implications for the traditional religious chronology, Lyell and Horner repeatedly displayed ignorance and wishful thinking about the nature of the Egyptian environment and material culture, and this was not lost upon commentators. It will be contended in the below that debate amongst academics and intellectuals focused overwhelmingly on Horner's problematic interpretation of Hekekyan's fieldwork. If Gold's contention that 'there were relatively few British travellers and residents in the Nile valley and this type of work was unfamiliar to British audiences' (2019: 228) is correct in strictly numerical terms, it is still worth

giving attention to such voices, some of whom were, like Hekekyan himself, very knowledgeable about Egypt and geological processes. The objections raised by Samuel Sharpe (Anon., 1859), James Forbes (Anon. 1863a), John Phillips (Anon. 1863b), and an anonymous author of an article in the *Quarterly Review* (Anon. 1859), were not merely due to personal bias or religious scruples. Much the same can be said for Wilkinson.

From this perspective, the Horner-Hekekyan excavation emerges as an innovative experiment, but one that was revealed, by what amounted to an interdisciplinary discussion, to be badly flawed, due to Horner's continual imposition of uniformitarianism, despite contrary evidence, and ignoring people who were familiar with it (including Hekekyan). Few of Horner's critics treated a single type of evidence – geological, textual or material culture – as authoritative; they usually combined several to make their case. In particular, Wilkinson, who belonged to the previous generation of British antiquaries who had taken an interest in ancient Egypt, took a holistic view: he considered all three types. The depth at which he did so enabled him to grasp the complexities and issues at stake to a greater extent than nearly all of the professional Egyptologists and geologists of the time. His contribution was not obstructionist, but was born out of genuine questions, shared by others and taken up by them, about the validity of an excavation that was couched as having been undertaken with innovative new archaeological techniques. It should be seen as an episode of an antiquary contributing to a contemporary debate, along the lines argued by Matthew Goodrum (2008; 2011), Nathan Schlanger (2011), and Clive Gamble and Theodora Moutsiou (2011). In terms of the basic assumptions made – albeit not the actual excavation techniques – at least some of the investigative work carried out at a time when the respective disciplines were emerging was retrograde compared to what had preceded it. As such, this episode should best be positioned as a demonstration that neither geology, archaeology nor Egyptology took a linear path to their modern forms, an argument previously convincingly argued for Egyptology by David Gange (2013) and William Carruthers (2015), although neither discuss antiquarianism. Whilst there has been extensive work which has shown how professional Egyptologists and archaeologists presented themselves in a self-congratulatory manner – and in the process marginalised Egyptians by restricting their access to Pharaonic remains and criminalising their behaviour – there has perhaps

understandably been less exploration of how such attitudes impacted earlier antiquarian scholars. None of this is to suggest that the Horner-Hekekyan excavation programme should be omitted altogether from future histories of the study of ancient Egypt. Rather it should be repositioned as an episode of scholars from different backgrounds cooperating to approach intellectual questions, and above all as a demonstration of the considerable limitations of new archaeological techniques, rather than as an episode in a straightforwardly progressive narrative, or of nascent disciplines invoking different forms of evidence in direct opposition to one another.

In what follows below, the Lyell-Wilkinson correspondence will be discussed, situated within its historical context: a dialogue about the Horner-Hekekyan excavation programme and associated questions, rather than theological issues, as Thompson assumes, due to his not having been able to read Lyell's side, which was formerly inaccessible to researchers. First, there will be a preliminary overview of the Hekekyan-Horner excavation programme. Second, it will be shown that Lyell attempted to rescue the programme on several occasions, but was thwarted on nearly every occasion by Wilkinson. Third, attention will be given to a controversy over ancient "burnt brick" and pottery, which was also, and more directly, resolved in Wilkinson's favour. In addition to these matters directly related to the Horner-Hekekyan excavation, Wilkinson's (and Lyell's) relationship with human prehistory will be explored. Before concluding, an overview will be given of how Lyell incorporated some of Wilkinson's ideas into his own work.

5.2.2 The Horner-Hekekyan excavation: new methods and new problems

In 1851, Leonard Horner enlisted Joseph Hekekyan Bey, an Armenian engineer and scholar, to conduct excavations in the Pharaonic-era cities of ancient Heliopolis and Memphis (near modern-day Cairo) (Horner, 1854-55; 1855; 1858; 1858).[25] The initial aim was to investigate the rate at which the alluvial silt of the Nile valley was elevating the floodplain, but it quickly turned into an investigation to establish how long Egypt had

[25] For excellent, although partially incomplete overviews (and additional commentary), see Gold (2019: 202-212) and Jeffreys (2010).

been inhabited by humans, on the basis of material culture – fragments of pottery and mudbrick – found at depth, a question that Horner had previously urged geologists to take up (Horner, 1855: 108, 120). Their study sites were located amidst two Pharaonic-era monuments, an obelisk at Heliopolis, and a colossal statue of the Pharaoh Ramesses II at Memphis, which the Prussian Egyptologist Karl Lepsius had dated to the 24th and 14th centuries BC respectively. Horner assumed that the debris that had accumulated around the base of each monument was mainly formed of alluvial mud, deposited at a uniform rate each year, as a result of the annual inundation of the Nile.

Hekekyan – on Horner's instructions – proceeded to measure the depth of the deposit down to the base of both monuments, which Horner then divided by the number of years which had elapsed since they had been erected. Having found that 10 feet of mud had accumulated below the statue at Memphis, he came up with a mean annual deposition rate of 3.5 inches/ century (Horner, 1859: 133). In a second stage, Hekekyan excavated a series of pits below the level of the base of the monuments, as far as was practical, up to 30 feet below the surface. Some of his cores reached depths of 60 feet (Horner, 1859: 130, 133) (Fig. 5.8). In his excavations – and as far as was practical for his cores – Hekekyan kept meticulous records of every artefact that his huge Egyptian labour force recovered, as well as the depth at which it was found. However, the more immediate result – reached by Horner in England – was to arrive at a date for the presence of people in Egypt at a very remote period. Horner rearranged the same basic equation, dividing the depth by the recently-established rate of accumulation. This time, he applied it to the depth of alluvial silt below the base of the monuments. He concluded that the material remains – "burnt brick" and pottery – found in the lowest pits dated to 11,646BC, pointing to an extremely early origin for ancient Egyptian civilization and human occupation of Egypt (Horner, 1859: 133). This was a date which was then several millennia earlier than any other scholar believed might be possible, but which, by 1859, as a result of the widespread acceptance of Jacques Boucher De Perthes' tools and the

Somme valley excavations, was increasingly seen as a possibility (Grayson, 1983; Van Riper, 1993).

In all, Horner published four articles in the Royal Society's journals, two in *Philosophical Transactions* and two in the *Proceedings of the Royal Society of London*. His essays were sent to some of the leading scientists of the day, among them Roderick Murchison, the discoverer of the Silurian system and a former president of the Geological Society, and Charles Darwin, for review. Darwin had started out as a geologist, inspired by Charles Lyell's *Principles of Geology* while on the voyage of the *Beagle* in the 1830s (Rudwick, 1992; 2008). Though Murchison and Darwin disagreed as to the extent to which currently-occurring processes were responsible for Earth's landforms, both received the article enthusiastically, and recommended it for immediate publication. In 1855, Darwin predicted that Horner's "observations probably will become in future times of still higher

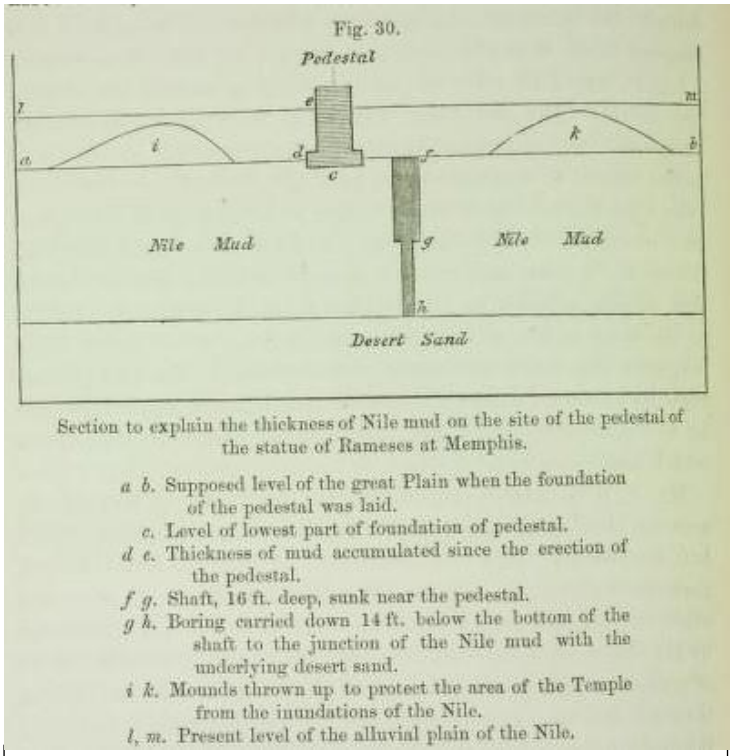


Fig. 5.8: Cross-section of Hekekyan's excavation at Memphis, Lower Egypt. Horner divided the depth of alluvial mud between e and d to establish an average rate of deposition. Following Sharpe's embankments critique (shown as i and k), Lyell sought Wilkinson's advice in establishing an average rate of deposition from between f and h. Source: Charles Lyell, *Principles of Geology*, vol. 1, 10th edn., London: John Murray, 1867.

value".[26] Three years later, commenting on the second part of Horner's paper, he recommended that the paper "ought, in my opinion, to be published in full" as a result of the incredible – not to mention sensational – conclusion that technologically-advanced humans had occupied the Nile valley for 13,000-14,000 years.[27] Darwin was not entirely neutral: aside from being a personal friend of Horner, he was a keen follower of Lyell when it came to ascribing features of Earth's surface to presently-occurring natural processes, despite this theory having led him toward what many regarded as a major misinterpretation at Glen Roy in Scotland (Rudwick, 2008). He went as far as including a short note that Horner's conclusions were "in some degree probable", in his landmark *On the Origin of Species* (1859: 18). Murchison's main concern was with the images: he doubted that Horner's extravagant use of colour was necessary, likely out of concern for printing costs.[28]

Once in print though, Horner's conclusions became fiercely contested. At a meeting of the Syro-Egyptian society in 1859, a summary of which was afterwards published in the *Athenaeum*, Samuel Sharpe (1859), the author of a prominent mid-19th-century history of ancient Egypt, criticised Horner's methodology. He argued that Horner was mistaken in his assumption that deposition had occurred since the first building of the monuments; simple intuition suggested that the Egyptians would never have allowed their cities and temples to flood, and that they would have protected them with walls and embankments once floodwaters became a threat – Herodotus had noted the presence of these. Sharpe suggested that deposition of alluvial sediment had only occurred once the embankments became neglected – this would only have occurred after the cities of Memphis and Heliopolis had been abandoned. The implication was that, instead of accumulating gradually at a uniform rate as Horner had thought, no sediment at all would have collected for several millennia, and that which was present would have accumulated in only a few centuries, probably well into or after the Roman period. In other words, Sharpe largely discredited Horner's carefully-established rate of accumulation. This in turn meant that Hekekyan's careful stratigraphic work and recording of the depth at

[26] Referee's report by Charles Darwin to the Royal Society, 19 March 1855. Royal Society archive, RR/2/111.
[27] Referee's report by Charles Darwin to the Royal Society, 12 [actually 22] March 1858. Royal Society archive, RR/3/147.
[28] Second referee's report by Roderick Impey Murchison to the Royal Society, 11 March 1855, Royal Society archive, RR/2/110.

which each object was found, in the subsequent stage of the investigation, was rendered largely pointless, at least for the present research question under investigation. Not only did it fail to provide an indication of absolute age, but even some of the relative dates were in doubt, and of those that were not, only a few centuries, rather than several millennia, were represented.

A month later, an anonymous writer in the *Quarterly Review* (1859) also attacked Horner's conclusions.[29] Besides echoing Sharpe's critique about embankments and Horner's counterintuitive assumption that the ancient Egyptians allowed their cities and temples to flood, the author made a number of further objections, ranging from concerns about the reliability of the workmen, to entirely practical explanations (1859: 419-421). One alternative was that if the fragments of pottery and brick had fallen into the Nile, and if the river subsequently shifted its course, then these would be buried tens of feet below ground in a comparatively short time. Another was that the artefacts could have fallen into fissures in the ground during the summer months. The reviewer pointed out that fragments might also have fallen into field wells. Therefore, even without accusations that someone had been dishonest or incompetent, there were plenty of explanations to account for how pottery and brick could be found at depth, without any need to assume that they had been slowly buried by successively deposited layers of mud.

Overall, the objections of the *Quarterly Review* article – which ultimately stemmed from those of Samuel Sharpe – were sufficient to convince Edward Stanley Poole, the nephew of Edward William Lane, who wrote *Manners and Customs of the Modern Egyptians***.** When the book was re-issued in a fifth edition the following year, in 1860, Poole included an extended note on the Horner-Hekekyan excavation programme, referring to it as "so-called scientific", but in any case, "crushingly refuted" by the *Quarterly Review* article (Lane, 1860: 594). Poole quoted several of the most scathing paragraphs from the *Quarterly Review* article. The end result was that, having been reported in two of the widely-read society magazines of the day, Sharpe's critique now gained a degree of

[29] Anon. (1859). Bunsen's Egypt and the chronology of the Bible, *The Quarterly Review*, 105, 382-421. Gold (2019) identifies the reviewer as William Smith.

permanency from being written into one of the most widely-read books about the Near East of the mid-19th century.

The sum total of the critiques amounted to an engaged rebuttal of the results of the Horner-Hekekyan investigation. Sharpe, Poole and the author of the *Quarterly Review* piece had not simply dismissed the Horner-Hekekyan investigation out of hand. The situation was quite different to that which several previous British and French investigators of cave deposits, such as John MacEnery and Paul Tournal, had faced in previous decades (Grayson, 1983). Rather, the critique had focused on realistic problems and cited an objection which was entirely intuitive: if British towns and cities were at risk of flooding, they would typically be protected in one way or another, such as with embankments – they were not allowed to be annually flooded, still less was any mud and flood debris allowed to accumulate in streets year after year. As such, Sharpe's objection that Horner had failed to take the embankments mentioned by Herodotus into account might have gained currency due to the cultural authority of the classics, but it was also an objection that seemed entirely reasonable. It was also reasonable to Horner: he privately admitted that Sharpe's critique was valid and seriously damaging to his project (Gold, 2019: 212).

The criticism that deposition had occurred all at once rather than gradually – as Horner concluded as a result of his subscription to Lyell's uniformitarianism – was in fact an obvious one to make, as there was stark evidence for it in Horner's own papers. In 1855, Horner, writing about Hekekyan's work at Heliopolis, noted that the excavated cores showed no trace of lamination (sediment which has distinguishable annual layers, usually due to the alternating deposition of coarse and fine material) (Horner, 1854-55: 239; Horner, 1855: 137). Horner also received observations from Hekekyan that were "still more inconceivable… in pits within a very short distance of each other different kinds of soil… at the same levels" was found (Horner, 1854-55: 239). Horner's preliminary conclusion that "other causes than the tranquil deposit from inundation water must have been at work in the formation of this portion of the alluvial land" was an understatement: Hekekyan had uncovered damning evidence against the idea that material had been deposited over a vast area of the plain of Egypt at the same time (Horner, 1854-55: 239).

After Horner concluded that the deposits had likely been disturbed, and thus would not give an accurate record of the natural rate of deposition, the centre of investigations switched to Memphis, the ancient capital of Old Kingdom Egypt, on the opposite side of the Nile. Horner found the results equally bad: not a single one of the excavated pits or boreholes contained any trace of lamination. Horner recorded experiencing "no small surprise", but rather than admit defeat, he came up with an explanation to save the assumption of uniformitarianism. Convinced that annual deposition had occurred, he suggested that Egyptian agriculture was to blame: farmers who tilled their fields after each inundation would have mixed up the soil, destroying any trace of lamination (Horner, 1858: 66). It was a workable solution, but a reductive one, and gave Horner little defence against Sharpe's critique.

To make matters even worse for Horner, Hekekyan himself sided with the critics: he informed Horner that the sediments at Memphis were associated with settlement debris, and not the annual inundation alone. For what must have been a friendly arrangement to start with, Hekekyan's letter is surprisingly scathing. Despite Horner's meticulous instructions, there had clearly been confusion at some point as to what Horner desired and what Hekekyan had supplied.[30]

Poole and the author of the *Quarterly Review* article also both took issue with the early date that Horner assigned to a collection of pottery and burnt bricks. Pottery required a kiln of some sort, an advanced technology which Horner regarded as indicative of the existence of civilization (Horner, 1858: 58-59).[31] The burnt bricks were produced with a similar technology, and were different from "crude bricks", which were manufactured by being moulded and then left to dry in the sun (Horner, 1858). Horner's estimate that the pottery and bricks were 13,500 years old – a conclusion based on the depth at which Hekekyan had found them thanks to his stratigraphic record-keeping – was astounding, as previously scholars of Egypt's material culture had assumed that these had first appeared in the Graeco-Roman period (Horner, 1858). Horner's suggestion pushed the date for their manufacture back by over ten millennia.

[30] Hekekyan to Horner, July 1859 (British Library Add Ms 37,459, see especially pp. 672-673).
[31] For a more detailed explanation of the difference, see Paul T. Nicholson, Kilns and Firing Structures, in W. Wendrich, J. Dieleman, E. Frood and J. Baines (Eds) *UCLA Encyclopedia of Egyptology*: Los Angeles.

There was more room for debate as to the age of the burnt brick than there was for the other objections, since one of the most eminent British Egyptologists, Samuel Birch, took Horner's side, and backed him over the early date of the burnt bricks. In his *History of Ancient Pottery* (1858), Birch concluded that most of the burnt bricks from Egypt appeared to be Roman, though he was aware of one, inscribed with the name "Tetmes or Thotmes" which he concluded was "probably contemporary with the kings of the Eighteenth dynasty" (Birch, 1858: 23).[32] As Birch worked at the British Museum, with access to its large collection of Egyptian material – many of the references in his book simply cite a catalogue number from the "Egypt Room" – he was in a position to know, even if his knowledge was hampered by questions about the context and provenance of objects: in this case his dating seems to have been made entirely on the grounds that there were several 18th Dynasty kings called Thutmose. Arthur Charles Harris, a British resident of Cairo, likewise backed Horner and Hekekyan; he too claimed to have pre-Roman burnt bricks in his collection (Gold, 2019: 212-213).

5.2.3 Charles Lyell and Gardner Wilkinson

Horner was, however, lucky that Charles Lyell was his son-in-law, and that Lyell was prepared to further investigate the case. Lyell's willingness to help can partly be seen as an example of a family member being dragooned into an intellectual project, in a manner that was not uncommon in the 18th and 19th centuries (Kölbl-Ebert, 2002; Sweet, 2004). However, there is an additional factor of potentially greater importance: both men were keen proponents of attributing the formation of landforms solely to currently-occurring natural processes (uniformitarianism). At much the same time, Lyell suggested that implements found by Boucher de Perthes in the Somme valley were 100,000 years old, based on an extreme uniformitarian interpretation of the deposits that lay above them. By 1863, he had been criticised by Joseph Prestwich, John Phillips and James David Forbes, who were convinced, on the presence of boulders which could only have been

[32] According to the preface, Birch finished his book in October 1857, between the publication of Horner's two reports. Birch was an unlikely ally, since he opposed the higher criticism which Gold suggests was a prominent factor in the discrediting of the excavation programme. He was clearly more open-minded than several of the geologists who participated in the debate.

moved by floodwaters, that the deposits had been laid down rapidly, due to repeated episodes of heavy flooding, in only 20,000 years (Van Riper, 1993: 161-162). Given that Lyell had spent the last three decades defending his extreme brand of uniformitarianism that he had advocated in *Principles of Geology*, it is possible that he anticipated such arguments and saw an opportunity to gain support for his own theorising, at the same time as defending Horner. At the very least, Lyell presumably saw the value of defending someone who subscribed to a theory which he had spent his intellectual life developing and defending.

In 1860, Lyell had visited Birch, in an attempt to get his support, for the burnt bricks controversy, which continued to be debated (Gold, 2019: 215). As noted above, Lyell contacted Wilkinson a year later, after having recognised that he was an authority on Egyptian geomorphology, as a result of reading one of his books. Based on Wilkinson's knowledge alongside his first-hand experience of Egypt, it is likely that Lyell saw him as offering similar expertise to Hekekyan.

One possibility for saving Horner's work from Sharpe's critique was to draw a conclusion based on the firmer but more limited evidence which he not disputed: namely, if the date of the abandonment of the embankments was known, then, at a minimum, the deposition rate for more recent years could be ascertained. After complimenting Wilkinson's intelligence and referring very briefly to Horner's two papers, this was precisely what Lyell requested of Wilkinson in his first letter, when he asked whether the abandonment of Heliopolis and Memphis was "1000-1500, 3000 or more years [ago]? Might they have gone on inhabiting Memphis & yet allow the colossal statue to be overflowed?"[33] Lyell's thinking was that, if Horner's assumption (that the depth of the alluvial deposit could be divided by the number of years that had elapsed since the monument below it was erected so as to establish an annual deposition rate, which could then be extrapolated to alluvial deposits below the statue) was incorrect due to flooding only occurring after the abandonment of Memphis (the critique made by Sharpe), then a different – but just as workable – solution would be to work out the average annual deposition rate from the number of years that had elapsed since the abandonment of

[33] Lyell to Wilkinson, 25 November 1861, p. 3 (National Trust: Calke Abbey).

Memphis alone. Dividing the depth of this deposit (the assumption still being that it had accumulated gradually, due to annual deposition) by the number of elapsed years would then give a more robust rate of accumulation. In turn, it would be possible to apply this to the material below it: this had the potential to sidestep Sharpe's critique altogether – assuming the deposition rate had stayed constant. In theory, it should have been fairly easy to work this out, given Hekekyan's meticulous records. Lyell appears to have been sufficiently confident to believe the results would be the same.

Of the two date ranges that Lyell supplied, his – and Horner's – preference was for the higher number – 3,000 years – for two reasons. First, Horner's entire rationale for selecting a site in Egypt was because of the scope for correlating sedimentary deposits with monuments of a great – and precisely known – age. This average rate of accumulation would in turn be more reliable for dating the deposit below the monuments which had accumulated over an unknown period of time, compared with an average worked out from 1,000-1,500 years. Second, if the sediment had taken 3,000 years to accumulate, then it followed that the rate of accumulation per year was less. This in turn lent credence to there having been ten millennia of deposition. That said, even if the 1,500-year figure, with its higher annual rate of accumulation, was correct, there was still potential for a remote date to be reached – either way, Lyell saw an opportunity to tackle Horner's critics.

In response, however, Wilkinson admitted that he was unable to give Lyell a "satisfactory reply": he noted that it was wrong to assume that there had been a fundamental break in Egypt's settlement pattern at the time of the Islamic conquest, instead there had been continuity despite the founding of Cairo as a new capital. He also suggested that the obelisk and statue had probably been abandoned "long before" this time.[34] In other words, there was no easy answer to the question of when the embankments had been breached.

In addition to being unable to answer Lyell's original question, Wilkinson outlined another difficulty at great length. He pointed out that it could not be assumed that the monuments – both the obelisk at Heliopolis and the statue of Ramesses II at Memphis –

[34] Wilkinson to Lyell, 27 November 1861, p. 1 (N.T.: Calke).

would have been flooded as soon as the respective cities had been abandoned, as it was unclear whether they had been built on the alluvial soil, which flooded each year, or whether they had been sited on a raised area of the desert edge. If they had been sited on a raised area, which seemed likely, then, even after the embankments were abandoned, several centuries could have intervened before the abandoned sites were flooded. A lower base level in association with the same age would have resulted in more rapid accumulation of sediment, which would in turn have further reduced the remote age calculation. Wilkinson included several sketches to illustrate the issue, showing Pharaonic-era monuments at different places on slopes of different gradients (Fig. 5.9).

Image omitted due to copyright restrictions.

Fig. 5.9: Four of Wilkinson's sketches, showing that the Heliopolis obelisk and statue at Memphis would not necessarily be submerged immediately after the overwhelming of a protective embankment, as it was unclear how far above the original ground surface they had stood. Source: Letter from Wilkinson to Lyell, 27 November 1861. University of Edinburgh Archive (identical to Calke Abbey copies).

As an additional demonstration, he referred to his own research at Thebes, thirty years earlier – this was quite possibly the passage that Lyell had read that had resulted in him

contacting Wilkinson in the first place. On the west bank of the Nile, two huge statues, the Colossi of Memnon, stood in front of the ruined temple of Amenhotep III. The latter was sited on the edge of the desert, furthest from the Nile. Here, Wilkinson had conducted a transect survey (Wilkinson, 1847: 351-352; 1841: 108-112; 1839: 431-444). Assuming that the ancient Egyptians had sited all of these monuments above the level of the inundation, but finding that the inundation was now rendering them inaccessible for several months of the year, with silt covering the bases of the statues, Wilkinson concluded that the fertile alluvial deposit in the Nile valley had expanded outwards, by 1,900 feet into the desert (Wilkinson, 1837: 437).

Unlike Horner, however, Wilkinson had made no attempt to establish an annual deposition rate and had recognised at the time that his 1,900 feet estimate was conservative because it was impossible to establish a baseline – the Colossi of Memnon might have stood at the level of the inundation in ancient times, or they might have stood considerably above it – in which case the Nile could have taken centuries to reach them, depositing more alluvium (fertile silt which could be used for agriculture) both in front and behind the colossal statues.

Having elaborated at length on an issue that Lyell did not want to hear, Wilkinson returned to consider Lyell's original question again – and Sharpe's critique – and suggested that Horner's (and Lyell's) premise was flawed: he pointed out that even if the date that the embankments were abandoned (and therefore liable to being breached) could be ascertained, "we should still be in a state of uncertainty as the quantity of deposit would at first be much greater than in subsequent years".[35] In other words, Wilkinson aligned himself with Sharpe. Wilkinson sent an additional letter to clarify minor points and – perhaps concerned that his original drawings had not been sufficiently clear – included several more to demonstrate what the situation at Heliopolis might once have looked like (Fig. 5.10).[36]

[35] Wilkinson to Lyell, 27 November 1861, p. 5 (N.T.: Calke).
[36] Wilkinson to Lyell, 28 November 1861, p. 4 (N.T.: Calke).

Image omitted due to copyright restrictions.

Fig. 5.10: Three more of Wilkinson's sketches, showing how it was unclear whether the obelisk at Heliopolis had been inserted into the alluvial silt (A), or if it had been positioned above it (B), or upon it (C). Source: Letter from Wilkinson to Lyell, 28 November 1861. University of Edinburgh Archive (identical to Calke Abbey copies).

Although it was not the answer that Lyell had been looking for, Wilkinson was able to correct his simplistic assumptions and make him aware of a more complex reality: there were simply too many known unknowns for Horner's assumptions – even though they were reached through meticulous excavations carried out by Hekekyan – to stand up to scrutiny, especially from the perspective of someone who was equally intimately familiar with the wider landscape. There is an irony that Wilkinson, an antiquary and amateur geologist, who had avoided making such assumptions in his paper, and his books in the 1830s and 1840s, had immediately recognised the limitations in a new study published two decades later, whilst two leading geologists – Horner and Lyell had both been president of the Geological Society in London – had not. Instead, Wilkinson backed Sharpe, a fellow antiquary, although he also went further than him too: Sharpe's criticism of Horner had mainly been made with reference to Herodotus's claim that the ancient

Egyptians had protected their cities with embankments; Wilkinson added field observations to his critique. There was, however, one point which would have provided some consolation to Lyell: Wilkinson indicated that he did not believe the course of the Nile had shifted appreciably.[37]

Despite not receiving a simple answer, Lyell must have immediately recognised that Wilkinson possessed expert knowledge of the sort possessed by literally nobody else except Hekekyan. He remained persistent and wrote a second letter, this time offering to send Horner's two papers from *Philosophical Transactions* to Wilkinson, politely suggesting "that [with these] you may refresh your memory, as you will I am persuaded be able to make very valuable suggestions".[38] At this stage, Lyell had several reasons for optimism from Wilkinson's somewhat rambling reply. First, he had at least not backed the *Quarterly Review* article's contention that the Nile frequently shifted its position in the Nile valley. Another – more important – point was that in one of Wilkinson's somewhat throwaway qualifying remarks (in relation to the slowness of deposition in lakes), Lyell was strongly reminded of Horner's assumption; that the rate of deposition by the Nile was very low, and required many millennia to accumulate. In response, Lyell transformed Wilkinson's remarks into a small cross-section sketch. Although he did not work out the calculation in full, Lyell noted that Wilkinson's observation closely matched what might be expected if the more gradual rate of accumulation – in association with the 3,000 as opposed to 1,000-1,500-year figure – was preferred.[39]

Spurred on by apparent agreement on some of the underlying assumptions, and with the hope that Wilkinson might be able to give him exactly what he was asking by looking at the papers he sent, Lyell again asked Wilkinson to comment on Sharpe's objection involving embankments. Though it was not connected to the questions of topography that they had discussed, Lyell also took the opportunity to raise the issue of the fragments of burnt bricks, found by Hekekyan 60 feet below the surface at Memphis.[40] In the event of Wilkinson's inability to give precise information as to when Heliopolis and Memphis were abandoned, Lyell probably saw these as a different possible route for

[37] Wilkinson to Lyell, 27 November 1861, pp. 7-8 (N.T.: Calke).
[38] Lyell to Wilkinson, 30 November 1861, p. 1 (N.T.: Calke).
[39] Lyell to Wilkinson, 30 November 1861, p. 2 (N.T.: Calke).
[40] Lyell to Wilkinson, 30 November 1861, p. 4 (N.T.: Calke).

saving Horner's study. After noting Birch's opinion that the author of the article in the *Quarterly Review* was mistaken, and that these bricks were indeed pre-Roman, he asked Wilkinson a leading question, "can you confirm this?"[41] Essentially, having failed in gaining an immediate alternative opinion from Wilkinson for the major issue, of deposition, discounted by Sharpe, and ambivalent as to whether one was likely to come, Lyell now sought Wilkinson's stamp of authority for another issue, the question of the age of the burnt bricks, which still remained controversial: if the objection that there were no pre-Roman burnt bricks was overcome, Lyell would technically be free to press for a date ten millennia earlier.

5.2.4 Mr. Birch's burnt bricks and Mr. Hekekyan's deep pits

Wilkinson wrote back to Lyell on two occasions, the following day (1 December), and then again, four days later (5 December), having read Horner's papers in the intervening time. The first of these replies would have been a disappointment for Lyell: Wilkinson noted that he knew that burnt bricks and pottery had been found at depth, but disagreed that this was an indicator of great age: he remarked that he was "surprised" to hear about Birch's conclusions on pre-Roman burnt brick.[42] Although he conceded that there were some older burnt bricks, he maintained that these had been due to destruction associated with unrest or foreign invasion: the ancient Egyptians only used mudbrick. He then confirmed that "I must keep to the opinion we have always entertained in Egypt – that burnt bricks were unknown there before the Roman conquest – & that they indicate a Roman period" – although he agreed that there were earlier kiln-produced pottery examples, albeit that these dated to only two millennia earlier (he later suggested that these could be mistaken for broken bricks).[43] Wilkinson also explicitly confirmed Lyell's fear that Sharpe was correct: he was of the opinion that "the land on which the obelisk & statues stand could not have been annually flooded by the Nile in the time of their first erection as the Eg[y]p[tia]ns carefully excluded the water from their towns".[44]

[41] Lyell to Wilkinson, 30 November 1861, p. 4 (N.T.: Calke).
[42] Wilkinson to Lyell, 1 December 1861, p. 1 (N.T.: Calke).
[43] Wilkinson to Lyell, 1 December 1861, p. 2 (N.T.: Calke).
[44] Wilkinson, to Lyell, 1 December 1861, p. 2 (N.T.: Calke).

In his next and more extensive letter, Wilkinson addressed Sharpe's objection directly. While commenting that Horner's research project was "most valuable & interesting" – words which contemporaries frequently used to describe the project – Wilkinson carefully dismissed Horner's uniformitarian assumptions – which Lyell had hoped he would back – by remarking that 'whether this thickness of the deposit can be taken as a measure from which to calculate the exact length of time it took to make that deposit is a question which I will not pretend to discuss' – the same conclusion that he had reached in his initial reply to Lyell.[45] The rest of this letter at first sight appears rambling and at times stubborn, accusatory and even evasive, but read in conjunction with Horner's published essays, a quite different picture emerges, showing that Wilkinson closely engaged with Horner's arguments, and did not automatically dismiss them.

Across his subsequent letters, though mainly in his letter dated 5 December, Wilkinson made an array of points, mainly objections to Horner's approach. Wilkinson's chief objection to Hekekyan's excavations was directed at the assumption that the alluvial deposits were the result of a slow year-on-year annual accumulation that had built up over the millennia, and that the material found at depth, as much as 60 feet below the surface, was thirteen millennia old.[46] He urged Lyell to consider what his conclusions would have been had he encountered the same phenomenon in a British context: "if we found brick or pottery at that depth in this country we should at once conclude that it had come there in consequence of a pit, well, or other excavation".[47] Wilkinson's favoured solution for Egypt was that "it is evident that a well might have been sunk in Roman times at any of those places where they were found": he suggested that Hekekyan had inadvertently excavated old field wells from the Roman period.[48] He added that digging further pits dozens of feet deep was "unfortunately difficult" and would necessitate "great trouble & expense", before cautioning Lyell to "hesitate" before

[45] Wilkinson to Lyell, 5 December 1861, p. 2 (N.T.: Calke).
[46] Wilkinson always referred to Hekekyan in a highly respectable manner. Here he noted that the excavations had been conducted with "great care & from what I know of Hekekyan Bey I feel quite sure that his part was performed with the greatest diligence & accuracy".
[47] Wilkinson to Lyell, 5 December 1861, p. 4 (N.T.: Calke).
[48] Wilkinson to Lyell, 5 December 1861, p. 3 (N.T.: Calke).

concluding that humans possessing kiln-fired pottery had occupied Egypt ten millennia ago.[49]

Given that in his first letter Lyell had pointed out that Hekekyan had excavated 95 pits, finding pottery in many, Wilkinson's argument at first glance borders on absurdity.[50] For Hekekyan's excavations across such a wide area to have struck 95 pre-existing wells would have been extremely improbable; Wilkinson's pessimism about the practicality of future endeavours resembles an act of special pleading to avoid uncovering further awkward facts – in this case, the discovery of a lengthy human prehistory.

Such a reading would, however, be a mistake, for a number of reasons. Wilkinson's remarks about difficulties and expense were not an attempt to shut down debate, but simply reflect an attentive reading of Horner's own papers. Horner had instructed Hekekyan to excavate to bedrock, which he assumed existed below the alluvial silt (the presence of limestone rocks on either side of Cairo suggested a geological basin structure). However, Hekekyan had failed to do this: groundwater filled his pits with water as fast as he dug them, forcing him to abandon his careful excavations and use a then-less-proven technique of boring. Eventually he was forced to give up on this too, and Horner concluded that "filtration water… must ever be found a great obstacle to any satisfactory ascertaining of the nature and depth of the entire amount of Nile sediment in any section of the valley" (Horner, 1858: 71). Both Horner's and Wilkinson's pessimism reflected the very real difficulties involved.[51]

Wilkinson's suggestion that Hekekyan had excavated wells as opposed to straightforward alluvial deposits also requires siting in its wider context: namely, Horner's contention that there existed brick ten millennia older than commonly accepted conflicted directly with what scholars thought they knew about ancient Egypt's material culture. Wilkinson himself had lived in Egypt for over a decade, travelling the length and breadth of the country, yet the only "burnt bricks" he had encountered were Roman-era ones: he had written about them in his books, such as *Modern Egypt and Thebes* (1843).

[49] Wilkinson to Lyell, 5 December 1861, pp. 4-5 (N.T.: Calke).
[50] Lyell (1863), p. 36 dismissed Wilkinson's suggestion.
[51] Issues with groundwater and the level of the water table still hinders excavation of archaeological sites in the Nile Delta, such as Sais and Naukratis. See Judith Bunbury (2019), 96-97.

He had dismissed claims that there were older ones on the basis that old mudbricks were being burnt for modern construction.

Almost inevitably, Wilkinson's belief that the ancient Egyptians had only started using kiln-fired brick in the Roman period would have made him greet a new discovery, from a single excavated site, with some caution. Yet again though, Wilkinson was not acting disingenuously. To Lyell, an otherwise undatable ancient brick found at depth was a convenient artefact, because it could be assigned a date based on geological reasoning alone – the date assigned to it by Horner. However, in order for this conclusion to stand, Lyell was forced to ignore all of the other artefacts that Hekekyan had excavated, and suspend his judgement for the dates that Horner would have reached for these had he applied the same calculation to them.

In a subsequent letter, written six months after the main correspondence, Wilkinson pointed out the problem, noting that a fragment of vase with a Greek motif, likely of about 200BC, had been found at a depth of 11 feet.[52] Wilkinson did not supply a calculation, but his reasoning is sufficiently clear: using Horner's own sediment accumulation rate of 3.5 inches each century, a date of more than a millennium earlier would be reached. Numerous other objects, including fragments of vases, glasses and sculpture were typical of material culture from the Greek period, yet found at even greater depths. In the most extreme case, "a head from a depth of 40 feet of greenish or greyish stone" was "probably Ptolemaic". Wilkinson, who had put ancient Egyptian material culture at the centre of his *Manners and Customs*, was well-positioned to make such statements. The challenge was therefore not simply limited to redating the bricks, but to redate a wide suite of ancient Egyptian material culture, in many cases by a millennium and in some cases by many. Perhaps unsurprisingly, Lyell's interest in ancient Egyptian material culture waned after its potential geological significance evaporated: he opted against taking the issue further and did not respond to Wilkinson's letter. He simply summarised the contents on the envelope of the letter, finishing with a matter-of-fact note: "Bricks age of".

[52] Wilkinson to Lyell, 29 August 1862 (Edinburgh University Archive, E 60/3). Wilkinson appears not to have kept a copy of this letter.

5.2.5 Wilkinson and the "Antiquity of Man" debate

At a time when many believed in a relatively recent origin of humanity (albeit on a much older Earth), Wilkinson's caution to Lyell to avoid concluding that the burnt bricks – and therefore presumably advanced civilization – had existed over ten millennia before the Roman period linked directly into a major debate at the time, on the antiquity of humans. Although Wilkinson made only a brief direct reference to this debate during his correspondence with Lyell, his remark warrants attention because it suggests that existing assumptions about Wilkinson's engagement with the debate are mistaken, and that he was flexible over the newly-developing consensus.

Before his correspondence with Lyell, Wilkinson had taken an interest in the issue and had written a letter, "Remains of Man in Caves", which was published in the *Athenaeum* (1860). From reading this, Thompson implied that Wilkinson took the "conservative" side in the debate on human antiquity (Thompson, 1992a: 196). Rather than accepting the growing scientific consensus of a lengthy human prehistory, inferred from Boucher de Perthes' Somme valley excavations, the Brixham Cave and other deposits in which human and extinct mammal bones were found in close association, Wilkinson (1860) maintained that extinct mammal and human bones found together were not contemporaneous: they only later became mixed together.[53] The objection itself had some validity: it had been made on numerous occasions in relation to other cave deposits, but greater diligence in excavating cave deposits and changing cognitive frameworks meant that there was ever-increasing certainty about relative age (Grayson, 1983; Rudwick, 2008). From the same letter, Bowdoin Van Riper (1993), accepts a similar narrative, citing and tabulating Wilkinson as a member of a group mostly from "outside the geological community" who refused to accept the emerging consensus post-1859. Other scholars have similarly assumed that Wilkinson was influenced by religious concerns to manipulate his chronology of Egyptian kings (Klaver, 2010; Gange, 2013).

[53] Wilkinson derived his argument from William Buckland, who voiced it on numerous occasions. Wilkinson's copy of Buckland's book, *Geology and mineralogy considered with reference to natural theology* (1837), has annotations. The work itself was a scholarly one and took advantage of sponsorship by the Earl of Bridgewater; the bulk of Buckland's work did not discuss religious matters (Calke Abbey: 3043843). Though Buckland is sometimes cast as reactionary in the debate on human antiquity, Lyell was no more enthusiastic about contemporaneity of humans and extinct mammals (Grayson, 1983; Van Riper, 1993).

However, Wilkinson's caution is not necessarily proof that he was unwilling to shift his position due to religious reasons, and there are reasons to believe his ideas were more nuanced. In a subsequent letter in the same correspondence, he informed Lyell "I do not in any way wish to call in question the great length of time which has evidently elapsed since the first deposits took place in the valley of the Nile *nor do I doubt the existence of man at a much more remote period than is generally supposed*".[54] Given that Wilkinson was ready to disagree with Horner on numerous points (see above and below), it seems hard to believe that he would have taken on such an opinion purely to impress Lyell. As such, it is unlikely that Wilkinson's opposition to the ancient dates reached by Horner was due to religious beliefs. It is also worth noting that given Lyell was attempting to obtain an authoritative opinion from Wilkinson, which he planned to publish, it would hardly have been in Wilkinson's interest to voice an opinion which he did not hold.

The most likely reason for the apparent discrepancy between Wilkinson's position in this regard and in his "Remains of Man in Caves*"* (1860) piece in the *Athenaeum* is that Wilkinson held the view that humans and prehistoric mammals had not co-existed at the time he wrote the article in June 1860, but had changed his mind by 1861: many of his contemporaries did this as the consensus on human antiquity grew (Grayson, 1983; Van Riper, 1993).

Were this the case, then one of Wilkinson's closest intellectual counterparts would in fact have been Lyell himself. As Michael Bartholomew has suggested for Lyell, the case for human antiquity was "a field that he had formerly eschewed", which had been "doing quite well without him", before he published his *Geological Evidences of the Antiquity of Man* (1863) (1979: 290). None of this is to suggest that Wilkinson became convinced of an exceptionally lengthy human antiquity of 100,000 or 1,000,000 years as Lyell and Alfred Russell Wallace had suggested respectively. However, the debate was an active one: Joseph Prestwich, one of the scientists who were instrumental in bringing about the change in 1859, favoured a more conservative figure of 20,000 years, and also doubted whether the co-existence of humans and extinct mammals proved a remote human antiquity (Van Riper, 1993: 156-165). Wilkinson's sympathies by November 1861 may well

[54] Wilkinson to Lyell, 14 December 1861, p. 5 (N.T.: Calke). My italics.

have been closest to Prestwich and as such were relatively mainstream. The aforementioned James Forbes and John Phillips, both of whom were critical of Lyell's extreme uniformitarianism in reviews of his new book, took the same view (Van Riper, 1993; Anon. 1863a; Anon. 1863b).

5.2.6 Moving forwards: Lyell

Had Wilkinson simply given Lyell information about the very specific question that he had asked in relation to the Horner investigations, it is likely that the correspondence would have ended early on. But, far from giving any information that might have saved Horner's study, Wilkinson had rebuffed him on nearly every point. However, it was this very comprehensiveness which made Lyell realise that Wilkinson's expertise could potentially be of use to his own book project – a survey on human prehistory and environmental change. It was information which Wilkinson was happy to supply, and which Lyell appears to have taken as straightforward fact, according to annotations he made on the envelopes of Wilkinson's letters (Fig. 5.11).

Images omitted due to copyright restrictions.

Fig. 5.11: Envelope of Wilkinson's letter to Lyell, 1 December 1861, with annotations by Lyell: "Burnt bricks not before Romans in Egypt". Lyell regarded Wilkinson's opinions as entirely trustworthy source of invaluable information. University of Edinburgh Archives (identical to Calke Abbey copies).

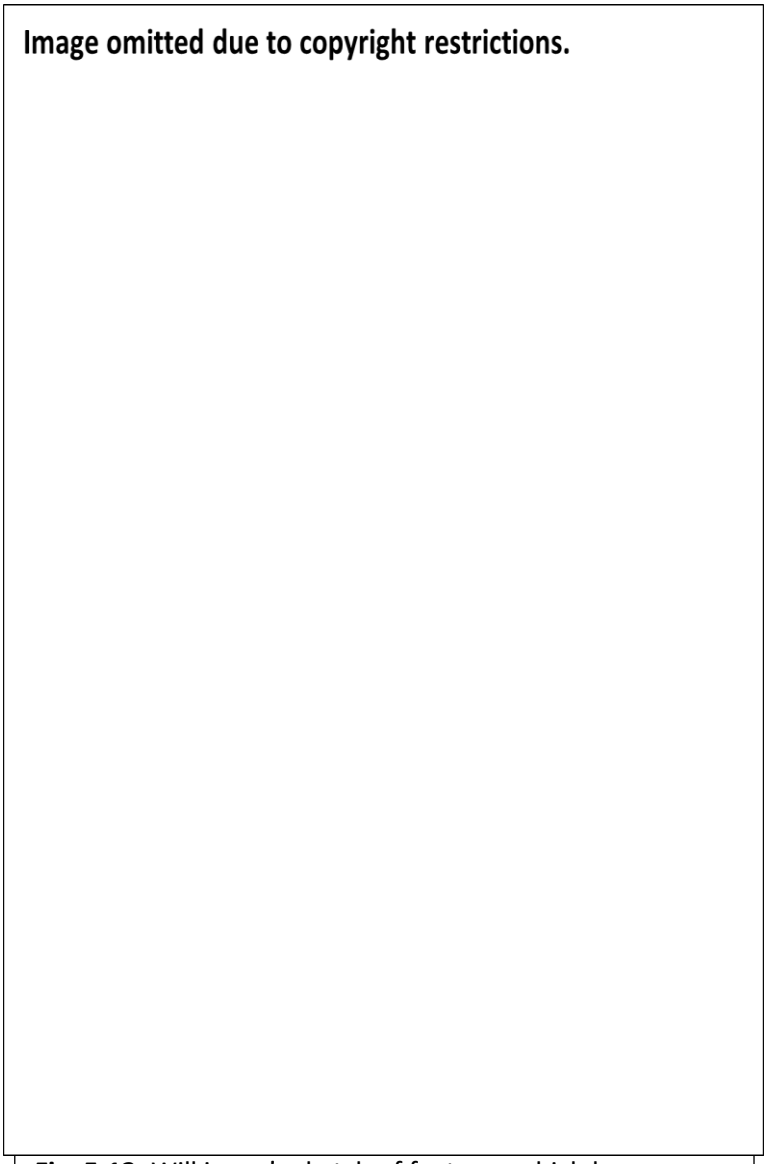


Fig. 5.12: Wilkinson's sketch of features which he presumed were former branches of the Nile, submerged below Lake Menzaleh, in northwest Egypt. Source: Letter from Wilkinson to Lyell, 14 December 1861. University of Edinburgh Archive (identical to Calke Abbey copies).

At about the time of their correspondence, Lyell was working on his *Geological Evidences of the Antiquity of Man* (1863), a huge interdisciplinary work which made the case for a lengthy human prehistory, based on the accumulating evidence (Lyell, 1863; Grayson, 1983, Van Riper, 1993). In several of his chapters, Lyell included studies similar to that which Horner and Hekekyan had done in Egypt: Lyell discussed recent sediment and rock formations, with prehistoric human remains in them, attesting to the early

presence of humans, often in association with prehistoric mammals. With relatively rapid accumulation of sediment, river deltas were prime study sites. His interest was therefore piqued when Wilkinson discussed environmental change in the Nile Delta, and the action of "subterranean agency" along its north coast.[55] Whilst replying to Wilkinson's intellectual meanderings politely and without much in the way of critical judgement (and also checking again to make sure that there was no possibility that Horner's study could be salvaged), Lyell enquired further into this topic.[56] Wilkinson's reply – his last in this continuous correspondence – supplied two case-studies in environmental change, that he had mentioned briefly. One was a structure called "Cleopatra's Baths" – Wilkinson noted that they had originally been above sea level, but had since sunk, with the land they were sited on, so had become partly submerged.[57] The other example was more dramatic: in the eastern Nile Delta, two former distributaries of the Nile, the Mendesian and Tanitic branches, had become entirely submerged underwater. Wilkinson supplied a rough sketch map to illustrate his point (Fig. 5.12).[58]

Both observations were of use to Lyell: he included a reference to them in his *Geological Evidences*, citing Wilkinson as his source for this slowly-acting but continuous natural process, which had also been observed in India: extremely deep fluvial deposits found by borings could not be explained in any other way except that considerable subsidence was occurring in deltas around the world (1863: 35). Lyell also referred to Wilkinson – albeit not by name – in reference to Horner's excavations. While he politely dismissed Wilkinson's explanation (and that mentioned in the *Quarterly Review* as well as those of other critics including Poole) involving wells, he felt forced to admit that overall, Horner's work was "not considered by experienced Egyptologists to have been satisfactory" (1863:36, 38). Finding that Egypt had not lived up to its reputation of stasis – an orientalist image which had evidently influenced Horner's geology – Lyell moved on to discussing mounds in Ohio. Although it was not a criticism that Wilkinson raised, a reading of Lyell's book suggests an additional important reason why he lost confidence in Horner's conclusions. Following the Brixham Cave and Abbeville excavations, those

[55] Wilkinson to Lyell, 5 December 1861, p. 8 (N.T.: Calke).
[56] Lyell to Wilkinson, 12 December 1861, p. 3 (N.T.: Calke).
[57] Wilkinson to Lyell, 14 December 1861, pp. 2-3 (N.T.: Calke).
[58] Wilkinson to Lyell, 14 December 1861, pp. 3-4 (N.T.: Calke).

seeking to contribute evidence to the growing consensus on the antiquity of the human species concentrated on finding artificial artefacts, such as flints, in association with extinct or extirpated mammals. However, in Egypt, the Horner-Hekekyan excavation had failed to find any. In some ways, Hekekyan's meticulous records worked against him: he had only found bones of extant mammals, such as ox and dogs. Not finding human bones might have been excusable, but not finding extinct mammals was unforgiveable, at least at this stage. To those who were active in the years after 1859, the excavation had failed to find the evidence that was valued the most. In his review of Lyell's book, Phillips (Anon. 1863b) keenly pointed out the issue.

Four years later, Lyell published a tenth revised edition of his famous *Principles of Geology* and included a page on Wilkinson's investigation into the expansion of the Nile's alluvial deposits out into the desert, as a result of his fieldwork at the Colossi of Memnon (1867: 433). Though Wilkinson had published this in his books in the 1830s and 1840s, and as a standalone article in the *Journal of the Royal Geographical Society*, it seems that Lyell only found out about it as a result of his correspondence with Wilkinson. Overall, even if Lyell had been disappointed in his original reason for contacting Wilkinson, and subsequently with more or less everything that Wilkinson had to say about Horner's work, it seems that he was able to make use of the correspondence in a small way for yet another project.

Naturally, Wilkinson's contribution to Lyell's work should not be overstated. What is of more lasting significance to the present study is the legacy of Wilkinson's contribution to the debate over the Horner-Hekekyan investigation. To some extent, Wilkinson was a latecomer: the main part of his correspondence with Lyell took place two years after Sharpe raised his objection at the meeting of the Syro-Egyptian Society in 1859. Lyell's attempt to enlist Wilkinson's support was therefore an attempt to rehabilitate the programme, which, as has been elaborated in detail, failed. Horner's hopes for Hekekyan to provide an answer to the criticism were dashed when the latter, despite promises to do so, opted to largely avoid the issue in his *Chronology of the Siriadic Monuments*, which was privately printed for a limited audience in 1863. By this time, both Lyell and Darwin had long since retracted their support: Lyell omitted any reference to Horner in his

address to the BAAS, while Darwin did likewise in a re-issue of *On the Origin of Species* (Lyell, 1860: 93-95; Gold, 2019).

5.3 Conclusion

By today's standards and techniques – which bring the possibility of reconstructing ancient landscapes with relative precision to the level of reconstructing small watercourses – the techniques Wilkinson used in his own investigation were crude, his reliance on Homer and Herodotus misguided, and his interest in verifying aspects of the Biblical narrative archaic. However, these criticisms aside, he appreciated that the River Nile was a dynamic system and that the Egyptian environment was constantly in a state of flux: these are insights that have only been realised by Egyptologists, primarily due to the Survey of Memphis, in the past few decades. As noted, the questions Wilkinson chose to ask – verifying an ancient myth, investigating the depths of Egyptian prehistory, exploring how the Egyptian environment influenced the development of Egyptian civilization, and reconstructing what the ancient landscape in a particular part of the Nile valley may have looked like in Pharaonic times – appear strikingly modern, and are essentially the same in their broad outlines, if not in their precise details, as those asked by many Egyptologists today. In other words, intellectually and cognitively – if not with quite the methodological complexity – Wilkinson anticipated the agenda of today's landscape and environmental archaeology.

Highlighting Wilkinson's use of geomorphological techniques as a tool for investigating a past culture is important at one level because it shows that the assumption made in recent landscape archaeology projects, that previous generations of Egyptologists eschewed research into environmental change, with the exception of one mid-19th-century pioneer, Joseph Hekekyan, is mainly a historiographical artefact with little basis in reality. In fact, scholars and travellers in Egypt were interested in environmental change, mainly in relation to the River Nile, as far back as the early 18th century. In other words, Hekekyan's accolade of being the "first geo-archaeologist" needs reassessing.

However, in addition to this concern as to absolute priority, another issue of crucial importance emerges. Hekekyan's research, which introduced stratigraphic principles

borrowed from geology, into the study of ancient Egypt, was not a straightforward advance on Wilkinson's work. Although it was undoubtedly on a much larger scale and conducted with a very specific aim with great contemporary relevance in mind, it was seriously held back by Horner's lack of appreciation of the specificity of key localities and his impressing of Lyell's extreme brand of uniformitarianism – which was in the process of taking the geological world by storm – upon Hekekyan's fieldwork. Horner assumed gradual as opposed to abrupt change from the outset, and while work proceeded, despite mounting evidence to the contrary. The result was that his conclusions were based as much upon wishful thinking as empirical fieldwork: Hekekyan's otherwise important stratigraphic work – the noting of all features, artefacts and changes in soil composition, at the precise depth at which they are found – was thus interpreted problematically, with excessively old dates imposed on much more recent artefacts.

Although these issues were initially overlooked by leading specialists in geology, including Lyell, Darwin and Murchison, they were recognised by two antiquaries, Sharpe and Wilkinson. Despite the widespread notion that antiquaries were chiefly engaged in intellectual throwbacks or confined their interests to socially fashionable matters of taste, both were not only able to perfectly comprehend the work of some of the leading earth scientists of their day who had conducted and reviewed an ingenious new research methodological approach – Hekekyan's stratigraphy – to investigate the depths of Egypt's prehistory, but they quick to identify the precise and severe problems inherent in it. By contrast, Wilkinson's "catastrophism", was in practice anything but dogmatic, as it allowed him to envisage an extreme event as having created the Nile valley, but for gradual events – annual deposition – to be filling it most of the time, and it allowed him to reach evidence-based conclusions which were at least as valid as those of Horner and Hekekyan: his interest in both cases was in researching aspects of the history of ancient Egypt.

Chapter 6 Dalmatia and Montenegro: Antiquarianism and Living History

6.1 Introduction

In 1844, thirteen years after the end of his decade-long residence in Egypt, and a year after he had finished writing up the last major work to result from that trip, *Modern Egypt and Thebes* (1843), Wilkinson made an extended visit, lasting several months, to several parts of the Balkan peninsula. In the process, he developed a deep new intellectual interest in the region, which culminated in the publication of a two-volume work, *Dalmatia and Montenegro*, in 1848. Coinciding with the year of revolutions, Wilkinson's book came out at a timely moment.

Wilkinson's new work was published by John Murray, who had taken on Wilkinson's previous books, *Topography of Thebes and General View of Egypt* (1835), *Manners and Customs of the Ancient Egyptians* (1837) and *Modern Egypt and Thebes* (1843). Each of these had been written mainly to inform readers, and were firmly in what could be called the antiquarian tradition, in which topographic work and studies of manners and customs were especially prominent. By contrast, the nature of *Dalmatia and Montenegro* was quite different: topographic antiquarianism was present, but when it came to manners and customs, Wilkinson was more interested in the Slavic peoples of modern Dalmatia, Montenegro and Bosnia, than in the ancient inhabitants of the region. Another significant difference that separated *Dalmatia and Montenegro* from his previous works was his occasional use of the first person. Though Wilkinson was not overly keen on narrative – his voice disappears for dozens of pages at a time when discussing historical events – his newest work was partly a travel narrative centred around his own journeying: a genre which Murray pushed many of his writers towards, even those whose primary interests lay elsewhere (Keighren *et al.*, 2015).

In this chapter, it will not be possible to discuss all elements of *Dalmatia and Montenegro* any more than it has been possible to discuss all facets of Wilkinson's work in Egypt, but consideration of Wilkinson's topographical antiquarian research, his depiction of the environmental backdrop in watercolours and engravings, and his representation of the manners and customs of the Montenegrins will reveal that his approach to the Balkans bore strong similarities to his approach toward the study of

ancient Egypt at several levels, both academic and popular. At the same time, there were notable differences, the most important of which was that, besides viewing the region through a classical lens, Wilkinson's vision owed something to a romanticism that represented (or misrepresented) the Balkans as a quasi-Medieval and ancient corner of Europe in which the inhabitants were both freer but less polished than other European peoples.

6.2 A Classicised Country: The discovery of Roman Illyricum in Dalmatia

6.2.1 British visitors to Dalmatia: Adam, Stuart and Allason

Before Wilkinson's visit, the Dalmatian coast had attracted a relatively small number of foreign travellers. In some ways the region was similar to destinations such as Egypt, Spain or southern Italy, which, although they contained numerous antiquities of a range of ages, including classical antiquity, nevertheless lay outside the scope of the traditional Grand Tour, and only became established as tourist destinations shortly before the middle years of the 19th century (Hulme and Youngs, 2002). In the intervening time, several Britons made visits, and some of the most scholarly were George Wheler, Robert Adam, James Stuart (whose visit was written up from notes and published by Joseph Woods half a century later in 1816) and Thomas Allason (Watkin, 2006; Tait, 2004; Soros, 2006; Monnesland, 2001). Some idea of the peripheral nature of the region can be appreciated from Stuart's dismayed remark at Pola (Pula) that "we had the mortification to find that the theatre, one of the principal objects of our curiosities, had been on long time since utterly demolished" (Stuart (Woods), 1816: 1). The structure had actually been dismantled in 1636, over a century before Stuart visited. Clearly if such a change had occurred in Rome or another well-visited city, it would have become common knowledge among travellers long before: Grand Tourists usually had fairly accurate expectations when it came to the physical monuments that they thought they would find (Sweet, 2012). Only in a place well off the tourist trail could a major classical monument disappear and not be missed for over a century.

In practice, Stuart had a good reason to be disappointed: he was an architect who had intended on visiting ancient sites – he had initially planned to spend his time in Greece, and his trip to Istria was an unplanned but opportunistic venture – to study the ancient architectural remains and learn about the correct proportions and motifs used by classical-era architects, to create well-built and historically accurate neoclassical buildings in Britain (Soros, 2006). Adam and Allason were members of the same profession, with similar motives for visiting the region. As well as an interest in architectural rules, Stuart, Adam and Allason all made claims to offer new information that was of interest to a broader audience; specifically, as Allason put it, "to the antiquarian traveller" (1819: 6). If "antiquarianism" is interpreted as an endeavour to explore the past through combining textual sources with objects or monuments – the argument of the present thesis – then the term is a useful one to characterise the approach of the three men, to varying degrees.

Adam, the earliest to visit, was primarily interested in one site: the ruins of Diocletian's Palace in Spalato, located further south, on the Dalmatian coast. Although a modern town had been built on the site, many parts of the palace survived in tolerable condition, sufficient to supply Adam with the material he required: he measured proportions and discovered, for instance, that in the "Corinthian Hall", there was evidence that "the architect has observed a rule of Vitruvius" (1762: 10). Adam went on to interpret other rooms in the palace along similar lines, and suggested reconstructions.

At Pola, both Stuart and Allason also used classical sources and examination of the monuments in combination, interpreting material remains according to classical texts. In some cases, they also attempted deductions made by comparisons with other monuments: for one of the Roman temples in the centre of Pola, Allason observed that "in the tympanum of the pediment a circular channel is sunk, in which a medallion of bronze must have been fixed, as appears by the tinge of green in several parts of it" (Allason, 1819: 14-16). Likewise, Stuart, attempting to solve the mystery of the lack of seats at the amphitheatre at Pula – the building was essentially a stone shell – suggested that holes in the masonry indicated that the original building had featured wooden

beams, on which the seats were positioned. In the intervening centuries, they had been lost.

Although the travel books by Stuart and Allason included new themes ("manners and customs of the locals"), it is notable that their remit was still primarily the classical age, despite the impressive Medieval ecclesiastical buildings of the eastern Adriatic. Two passages in Allason's book give a good idea of his priorities. He devoted as much space to noting the presence of "the foundation of a Temple of the Corinthian order, two of the columns of which are in tolerable preservation" (1819: 5) at Parenzo, as he did to Rovigno (Rovinj) Cathedral, which he described simply as "a large Gothic structure" (Allason, 1819: 5), with only its dimensions and resemblance to St. Mark's in Venice being noted before the account moved on. As Allason's account moved on to Pola, his main destination, his interest in the Medieval Gothic remained muted: he described the Roman amphitheatre in depth and then noted that "the walls of the town bore the appearance of a Roman fortification, with battlements, towers etc." (Allason, 1816: 6). Although it is conceivable that Allason's prose indicates that he was struck by a resemblance between Pola's walls and the city walls of Rome, by this time many 19th-century travellers would not have missed this opportunity to draw a comparison to medieval fortifications in such an instance: an enthusiasm for the medieval was well underway among latter-day Grand Tourists in Italy by the early 19th century (Sweet, 2012). This is not to suggest that all visitors to Dalmatia valued only the classical era: the Venetian traveller, Alberto Fortis, was interested in other periods, and for him, the Morlacchi peasants were of especial interest, as a form of European "noble savage" (Wolff, 2001). However, most Britons who visited the region valued the classical over later periods, and an interest in subjects related to the latter took longer to develop.

6.2.2 Wilkinson in Dalmatia and Montenegro

At a structural level, Wilkinson's *Dalmatia and Montenegro* (1848) consisted of a historical and cultural introduction, followed by an account of Wilkinson's journey from town to town along the coast, with a foray into the interior to the north of Spalato (Split) in the fourth chapter, then one into Montenegro, near the end of the first volume. In the

second volume, he recounted a visit to the Ottoman province of Bosnia-Herzegovina, as well as a lengthy historical account of the whole region. From a fold-out map at the end of the second volume – a feature that John Murray frequently included in travel books whether or not the traveller had drawn one (Keighren *et al.*, 2015): Wilkinson's was largely second-hand from Austrian surveys – Wilkinson's readers were able to identify the places Wilkinson visited. Although in practice Wilkinson seems to have used Spalato as a base (Thompson, 1992a), he presented his journey as a linear itinerary, as he had for his Egyptian topographic works (1835; 1843).[1] This began in Trieste (Fiume/ Rijeka) and continued through the Istrian peninsula, before heading south – usually by steamship – along the Dalmatian coast: Wilkinson visited towns with notable classical, Medieval and post-Medieval remains, such as Zara (Zadar), Sebenico (Šibenik), Spalato, Salona (Solin), Trau (Trogir) and Ragusa (Dubrovnik). His short excursion into the interior of what is now Southern Croatia consisted of a journey north along the River Kerka as far as Knin (about 45 km inland), before turning south and following the River Cettina (Cetina) as far as Sign (Sinj), then returning to Spalato. One of Wilkinson's major interests was in finding ancient Roman towns that had been mentioned by classical authors such as Pliny, or had featured on ancient Roman road itineraries such as the Peutinger Table. This was also an interest which he carried into his trip into the interior: Wilkinson noted Roman period remains when they coincided with the likely location of Roman cities, such as ancient Burnum. Along the way, Wilkinson gave some limited political commentary. In his view, the region had formerly been oppressed by the Venetian Republic (he noted that its emblem, the Lion of St. Mark, appeared everywhere except at Ragusa, which had managed to maintain its independence as a smaller maritime city-state despite the odds), but was now under the rule of Austria, and was more neglected than oppressed – a perspective that confirms much recent revisionist scholarship on the Habsburg Empire (Judson, 2016; Sked, 2001; Laven, 2002).

Besides an itinerary of interesting places, Wilkinson added numerous historical and local details. In the preface, he explained that these were vitally important: in Rome there were many "imperfect remains" which nevertheless attracted considerable attention due

[1] "Journal", 1844, Bodleian Library Wilkinson manuscripts dep. d. 57; "Journal", 1844-45, Bodleian Library Wilkinson manuscripts, dep. d. 58.

to their association with historical events and great figures of the Roman Republic, while at Nimes and elsewhere, there were "more perfect remains" that failed to attract people, as they largely lacked associations with great personalities and were seen by most as simply provincial expressions of Roman culture (1848: v). The parallel was an apt one: Pola possessed an amphitheatre and well-preserved Roman temples like the French town, while Diocletian's Palace at Spalato was easily on the scale of the large buildings in Rome such as the Baths of Caracalla. However, both Dalmatian sites were less well-known than the structures in Rome. Wilkinson was also doubtless aware that the less impressive remains, such as those of the ancient city of Burnum, would have benefitted even more from such links. His predecessors in Dalmatia had followed the approach, and so had contemporaries in Britain: Crosby Hall, a late Medieval Hall in London, had been saved from demolition due at least in part to campaigners who had linked the place to notable medieval figures (Sweet, 2017b). Aware that a work on classical topography could strain the attention of his readers and that not all would have found the later periods of Dalmatia's history as interesting as he did, Wilkinson made the case for this approach at the start of the preface to *Dalmatia and Montenegro*.

> I have, therefore, endeavoured to supply the interest, which the mere names of districts or towns would fail to afford, by noticing the events that took place there, as well as the part performed by the inhabitants in the history of their country (1848: v-vi).

In the first chapter of *Dalmatia and Montenegro*, Wilkinson supplied ample local colour by setting up the Balkans as a region in which the ancient past survived into the present day, to a greater extent than it did elsewhere in Europe. Discussing religion, Wilkinson presented the Northern Slavs in particular as a pagan people, with customs such as sacrificing cakes and horses, soothsaying and above all a "lengthened adherence to the rites of idolatry" (1848: 17). To western readers, Slavic deities would have been even more unusual: some had multiple heads, occasionally with animal ones substituted in the place of human ones, as well as multiple arms – the images recall ancient Egyptian and Hindu deities (and in a proof copy of *Dalmatia and Montenegro*, Wilkinson drew a picture

of one such lion-headed Nubian deity).[2] Wilkinson took Asian parallels further and noted another practice, of fire-worshipping: the obvious religious affinity here was with Zoroastrianism, a religion which had formerly been dominant in Persia, but was mainly centred on India by the 19th century. Alongside these examples, Wilkinson also suggested that, at some point the suttee (sati) – what might be regarded as the ultimate (and then highly topical) example of a barbarous custom – was in use. These had been features of Dalmatia's pre-classical and post-classical past (Wilkinson's sources were Roman authors), but they nevertheless gave an impression that the region was not quite like the rest of Europe – a theme which has been detected by Ouditt (2013) for southern Italy, and Todorova (1997) and Hammond (2007a; 2007b) for the Balkans more broadly.

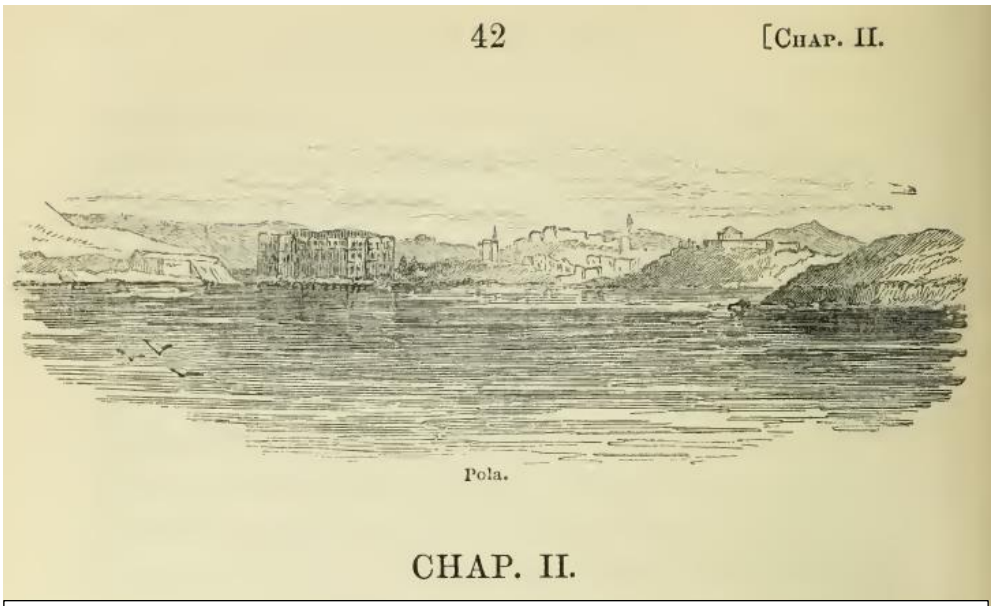
Whereas Wilkinson presented Dalmatia as a place where barbaric customs had once occurred, and quaint ones (such as open-air assemblies) had lingered until relatively recently, he presented Montenegro as a place where old ways (albeit of a different sort to those recorded by ancient Roman commentators) lingered into the present day. The contrast was one that Wilkinson felt reflected reality: several years before *Dalmatia and Montenegro* was published, in letters to his publisher, John Murray, he noted his disdain for "introducing personal narrative & making a great deal of commonplace events" since "there is nothing that happens in Dalmatia to any body worth mentioning".[3] In another letter to Murray, Wilkinson presented Montenegro in a very different light, noting that "no one before me has been through their country & the few who have visited Montenegro have been satisfied with a visit to Tzetine which is only six hours from Cattaro".[4] As will be seen, Wilkinson's view of Montenegro was very different from how he saw the rest of Dalmatia, but it was by no means an exclusively negative one.

[2] "Proof copy of *Dalmatia and Montenegro*", 1848, vol. 1, p. 24, Bodleian Library, Wilkinson manuscripts dep. d. 86.
[3] Wilkinson to John Murray, October 1846, John Murray Archive, National Library of Scotland, Ms. 42177, fols 124-151, 1843-44.
[4] Wilkinson to John Murray, September 1846, John Murray Archive, National Library of Scotland, Ms. 42177, fols 124-151, 1843-44. John Murray's letters to Wilkinson do not appear to have survived.

6.2.3 Classicising Istria and Dalmatia: Pola, Spalato and Illyricum

Several of the first cities that Wilkinson encountered – Pola and Spalato – were already, as has been seen, widely known as a result of the travels of men such as Robert Adam, James Stuart and Thomas Allason, who had investigated the Roman remains. In these places, Wilkinson's interest resembled that of earlier travellers on the Grand Tour: he noted scenic and picturesque views, but also sought to make original scholarly commentary where he could (Hibbert, 1969).

In Pola, the most important Roman site was the amphitheatre, which, like the Colosseum in Rome, was a very dominant feature in the modern town – Wilkinson included an image of the building at the start of his chapter (Fig. 6.1). Wilkinson's first reaction, in *Dalmatia and Montenegro*, was to remark on its physical appearance and setting: "the amphitheatre appears to stand on the shore; the exterior is so perfect, that it scarcely seems to deserve the name of a ruin; and it looks the same to you, as it did of old to the Roman, as he stood in his galley, 1500 years ago" (44-45). At this stage, Wilkinson treated the ancient structure as a feature of interest for the visitor in search of picturesque vistas, similar to Stuart and Allason.


42 [CHAP. II.

Pola.

CHAP. II.

Fig. 6.1: "Pola", showing the town from the sea. The amphitheatre is on the left. (Wilkinson, *Dalmatia and Montenegro*, p. 42).

Wilkinson's account did not dwell solely on scenic concerns, but also made a nod to questions which were of interest to discerning travellers and stay-at-home scholars. One of these was the aforementioned paradox that, despite its very good preservation on the outside, the amphitheatre at Pola was essentially a shell: virtually nothing remained of the interior. Given that those taking an interest in the somewhat peripheral Roman remains of Pola were probably already well-acquainted with the Colosseum in Rome, which featured arches that had presumably once supported tiered seating, there was the obvious question of whether Pola had once featured the same structures.[5] Wilkinson disagreed with a previous (unnamed, but this was probably Stuart) suggestion that the seats had been wooden, as "many of the stone seats have been found, some bearing the names, or initials, of their owners. They measure 1 ft 2 ¾ in. in width". The suggestion was an inspired one, and was typical of his approach, in which he attempted to reconcile material remains with written sources; it likely also owed something to familiarity with members of the gentry possessing reserved pews in many British churches.

Besides the question of seating, Wilkinson noted the unusual corner towers of the amphitheatre, as well as the Tuscan columns. He also described, in lesser detail, the other notable Roman remains of the town. One of these was the Temple of Rome and Augustus. Here, Wilkinson noted the Corinthian columns and that the building was so well-preserved that it was in use as a museum. However, the same could not be said for the ancient forum: much of this had been built over and was occupied by modern houses. More briefly, Wilkinson noted a triumphal arch of Lucius Sergius Lepidus, and the Porta Gemina gateway, as well as a more recently-discovered gateway (1848: 46-47). Although all of these ancient structures were well-preserved and had featured in the books by Stuart and Allason, Wilkinson included no further illustrations of these beyond that which he had used at the start of his chapter – possibly because his major focus was on Dalmatia, as opposed to Istria, which effectively functioned as the gateway to his travels further south.

The other major classical site on Wilkinson's itinerary was Spalato, which was the renowned site of the palace of the Roman emperor Diocletian. Whilst Stuart and Allason

[5] Although some of the arches in the Colosseum in Rome feature stone seating, this was a 20th-century restoration and was not present in Wilkinson's time.

had not described the ruins there, Robert Adam had done so. To set the scene, Wilkinson first recounted the association with Diocletian – renowned not only for splitting the Roman empire in two but for retiring, allegedly preferring the cultivation of cabbages to governing – as well as the subsequent history of the site: the inhabitants of Salona moved to the site to escape the marauding Avars. Having noted these human interest and historical aspects, Wilkinson turned to discussing the notable antiquities of the site.

Wilkinson's first concern was with the dimensions of the place: it was "nearly a square": its sides measured 521 feet (or 598 feet 8 inches including the projecting angle towers) by 705 feet 6 inches. These figures differed from those given by Robert Adam, who recorded measurements of 592 and 698 feet respectively, although Wilkinson modestly noted that "if he used a chain, perhaps he is more correct" (1848: 124). Wilkinson also deferred to Adam (whom he repeatedly, but incorrectly, referred to as "Adams") when it came to describing the original interior features of the palace: the southern half contained two temples and the emperor's living quarters.

Where Wilkinson diverged, somewhat tentatively, from Adam was in establishing which deities each of the temples had been dedicated to. Adam suggested that the south-east "Octagonal Temple", since converted into the Duomo, or cathedral of the town, had been dedicated to Jupiter, while the south-west temple had been dedicated to Aesculapius. From paying attention to the frieze relief scenes – a decorative series of images carved in relief which frequently contained a historic, religious or mythical narrative, situated high on the façade of a building – Wilkinson suggested possible alternative dedications for both temples. In the case of the temple that had supposedly been dedicated to Jupiter, he observed that the frieze contained "sculptures… representing Cupids riding, or in chariots; hunting boars, hares, lions, and stags" (1848: 129). For him, such themes pointed toward Diana (the Greek Artemis), the Roman goddess of the hunt, rather than to Jupiter. Although putting the idea out, Wilkinson himself ultimately dismissed it since textual sources seemed to indicate otherwise: Diocletian was known to have taken on the title of "Jovius", while the Notitia Dignitatum provided further supporting evidence, as did the writings of a 13th century archdeacon.

For the temple of Aesculapius, located 115 feet away, in the south-west quadrant of the palace, Wilkinson noted a possible alternative identification, again based on the bas-reliefs of the frieze which ran around the upper part of the temple façade: they "represent Cupids plucking grapes, amidst trees and vases, and lions and leopards resting their paws on vases; from which this temple might seem rather to have belonged to Bacchus, than to the God of medicine" (1848: 134). Ultimately though, Wilkinson again came down in favour of Adam's identification, on the grounds that Aesculapius was much-venerated in Dalmatia, so it would have been intuitive for Diocletian to dedicate a temple to him. Notwithstanding this, it is evident that the bas-reliefs were a source that captured Wilkinson's attention – he recorded both, in a sketchbook and a journal (Figs 6.2-6.3) – just like those at the Ramesseum and Medinet Habu, at Thebes, two decades earlier.[6] However, unlike in Egypt, he refrained from making a revisionist interpretation, at least partly because textual evidence was against it. As such, Wilkinson – ever attempting his antiquarian-minded endeavour of marrying textual sources with material culture as opposed to exclusive reliance on either – limited himself merely to making a suggestion as opposed to a comprehensive interpretation. His considerable openness to considering other possibilities is further demonstrated by a correspondence with another antiquary, William Martin Leake, who assured him that the building had been a small temple as opposed to a tomb.[7] Ultimately though, Wilkinson's decision to desist from attempting a broader revision probably reflects that whereas in Egypt he had made himself into an established authority, this was much less the case for Dalmatia and the classical age. For this place and time period, Wilkinson was prepared to follow more established scholars and their literature; Johann Winkelmann's interpretation of ancient art as predominantly religious was also likely a major influence (Lolla, 2003).

[6] "Sketchbook", 1844, Bodleian Library, Gardner Wilkinson manuscripts, dep. b. 2, p. 22; "Journal", 1844-45, Bodleian Library Wilkinson manuscripts, dep. d. 58, folio 42.
[7] William Martin Leake to Gardner Wilkinson, 1 March [N.D.], in "Journal", 1844-45, Bodleian Library Wilkinson manuscripts, dep. d. 58, folio 43.

Image omitted due to copyright restrictions.

Fig. 6.2: Sketch of bas-reliefs from Diocletian's Palace, of Cupids hunting and driving chariots, "Sketchbook", 1844, Bodleian Library, Gardner Wilkinson manuscripts, dep. b. 2, p. 22.

Image omitted due to copyright restrictions.

Fig. 6.3: Sketch of bas-reliefs from Diocletian's Palace, of Cupids "plucking grapes", among vases, "Journal", 1844-45, Bodleian Library Wilkinson manuscripts, dep. d. 58, folio 42.

Wilkinson made most of his other observations in relation to the much larger temple which both he and Adam believed was dedicated to Jupiter, and which had later been

converted into a cathedral. Although many visitors to Italy in the late 18th and early 19th centuries had been impressed by the medieval architecture (Sweet, 2012), Wilkinson was somewhat ambivalent about the change of use in the case of this building. Whilst he admired the Campanile tower, especially its great height – a remark frequently made about non-classical buildings and sculpture by those who felt they had few redeeming features at an aesthetic level – Wilkinson was ambivalent: the change of use had altered the interior of the ancient building, and he chose to concentrate on noting the classical features which had survived. Among these were an unusual pediment, with a rounded arch, arches which sprung from the tops of capitals, and a couple of sphinxes. Wilkinson's dismay at Spalato was not motivated only by concern for damage to a Roman building: he made similar unexcited remarks about the medieval architecture of the cathedral at Zara. If he was enthusiastic about medieval stories and associations, and of post-medieval Venice, he was less enamoured by the architecture of the period.

However, Wilkinson's interest was piqued by two Pharaonic-era sphinxes at the palace. With his knowledge of hieroglyphs, he was able to ascertain that one dated from the time of Amunoph [Amenhotep] III and that it was likely from Thebes – the Colossi of Memnon at Thebes had been built by the same monarch. Although the existence of sphinxes in Dalmatia technically would have fitted his argument that ancient Egypt, under Sesostris or Ramesses II, had conquered large parts of Asia and Europe, Wilkinson, aware of the Roman practice of transporting Egyptian antiquities such as obelisks to Rome, made no such argument: he noted that the sphinx had likely been brought over by the Romans during their occupation. Elsewhere, commenting on reports of an ancient cult of Isis at Zara, made by George Wheler (1682) and Allason, Wilkinson again desisted, noting that this in all likelihood dated only from the Roman occupation, during which numerous eastern religions and belief systems, including Christianity and Mithridatism, had spread westward.

Image omitted due to copyright restrictions.

Fig. 6.4: Sketch map of the towns of Roman Illyricum, at the end of a dictionary/ phrases notebook. "English/Serbo-Croat vocabulary, n.d. (*c*.1843-5)", Bodleian Library, Gardner Wilkinson manuscripts, dep. e. 42, folio 51.

Alongside discussion of Pola and Spalato, the two landmark Roman sites which possessed the most impressive civil, religious and military architectural remains, Wilkinson gave attention to dozens of other collections of classical antiquities and sites associated with the Roman occupation. From (apparently) Roman-era place-name and route records such as the Peutinger Table and the Notitia Dignitatum (Talbert, 2010), scholars, Wilkinson among them, appreciated that there had once existed a number of other important cities in what was then Austrian Dalmatia. Salona, not Spalato, had actually been the capital, while Burnum and other places were also important. Wilkinson drew a sketch map of the most important cities of the Roman province of Illyricum, several of which will be mentioned in the below (Fig. 6.4).[8] Throughout his journeying through Dalmatia, he eagerly recorded sites of Roman towns, pieces of Roman

[8] "English/Serbo-Croat vocabulary, n.d. (*c*.1843-5)", Bodleian Library, Gardner Wilkinson manuscripts, dep. e. 42, folio 51.

architecture, and even simply piles of Roman-era bricks – both of the latter categories of antiquities could be used to establish the sites of interest. In some ways, what Wilkinson did now was similar to what he had done in Upper Egypt. Both projects were part of a wider tradition of topographical antiquarianism, which many scholars – among them Wilkinson's mentor William Gell in Italy and his friend Colonel Leake in Greece and Anatolia – had worked on throughout the eastern Mediterranean and beyond (Wagstaff, 1987; Wagstaff, 2001; Allen, 2002; Koelsch, 2013).

Before proceeding, an important point to note is how Wilkinson distinguished whether or not an architectural feature or fragment was indeed Roman. Essentially, he followed an approach that was similar to that which he had used in Upper Egypt: stylistics. By paying attention to the fine details and proportions (which, once learnt, could frequently be distinguished at a glance) of architecture that could be dated due to either inscriptions referring to individuals/ events, or through other independent means such as through texts, these principles could be applied to buildings or fragments of architecture that lacked any other way to date them.

One of the clearest examples of Wilkinson using stylistic evidence as a methodology to date a structure comes from one of his travel journals, and across the border in Bosnia. Although Wilkinson ultimately concluded that the bridge of Mostar was not Roman, the stylistic principle is the same as that which he used in Dalmatia. According to local tradition, the bridge had ancient origins, possibly dating back to the reign of the Roman Emperor Trajan. However, on examining the architecture of the bridge, Wilkinson had doubts. He noted that the stones were small, unlike Roman ones. He noted other evidence too: "the torus under the parapets & the receding arch with a brow of stone are quite Turkish".[9] In his book, he also noted that the spandril projected over the arch, another sign of Turkish architecture.[10] To Wilkinson, the bridge itself was not Roman, although he remained open to the possibility that there could have been an earlier bridge on the site; he also repeated a remark by the local Vizir that the tradition of it having

[9] Wilkinson, "Journal, 1844-5", Bodleian Library Wilkinson Manuscripts, dep. d. 58, p. 17.
[10] The "spandril" refers to the triangular sections on either side of an arch. For easy reference, those on the Arc de Triomphe, Paris, feature two angels carrying a quill and a trumpet.

been built by Suleyman the Magnificent was probably false, and it "was probably built by the Pagans" (1848: 60). Wilkinson also included a footnote to inform his readers that the place-name "Most star" meant "old bridge". Overall, his approach was a logical compromise: it was based on stylistics, but was aware of the shortcomings inherent in the approach.

The northernmost settlement on Wilkinson's sketch map was Zara, which had formerly been the capital of Liburnia: an ancient city-state which had been incorporated into the Roman Empire and had some resemblance to Venice or Ragusa in more recent times. Despite its prominence, there were relatively few antiquities that could be linked to classical times. One of these was "the sea-gate, called Porta di San Chrysogono" (1848: 79). This featured a Latin dedication, announcing that it had been erected by Melia Anniana in memory of her husband, and Wilkinson inferred from the inclusion of the word "emporium" that it had stood in a market-place. Confidence in a Roman date was strengthened by stylistic evidence: the arch featured Corinthian pilasters, which supported an entablature. Wilkinson noted two pieces of material evidence for a Roman presence. One was that there were traces of an aqueduct: besides this method of water conveyance being firmly associated with the Romans, Wilkinson noted that the 18th century Venetian traveller Alberto Fortis had attested to an inscription being found with it, which declared it to have been built under the Roman emperor Trajan, who reigned at the start of the 2nd century AD. Another example were reports that "many ancient pavements have been found below the level of the sea" (1848: 85). Wilkinson did not elaborate on the intriguing reports in any detail – he did no fieldwork comparable to that which he had undertaken at the Isle of Pharos or at Thebes. Instead, he simply prefaced the note by saying that "the land has sunk very much, since the time of the Romans" (1848: 85). Besides being a very brief nod towards the consensus that the environmental backdrop was dynamic, as Lyell in particular was widely renowned for arguing, it is plausible to suggest that this remark provided a sense of enhanced historical distance: Roman remains could be found, but in some cases they were visibly detached from the present.

Wilkinson's account of Roman antiquities in Salona was more extensive, likely due to the far more extensive antiquities and ruins at the site, as well as its political importance in antiquity: although the Emperor Diocletian had resided in his palace in nearby Spalato, Salona had been the capital of Roman Dalmatia and so it was arguably more important in considering the ancient geography of that particular province. However, in the intervening time it had almost entirely disappeared and had become virtually unknown, both to learned society in Britain, and, apparently, to locals too. This latter contention had been made by Francesco Carrara, a local official in the Austrian administration based at Spalato, who had previously undertaken excavations of Diocletian's Palace, and at the time of Wilkinson's visit, and was poised to begin investigations at Salona: he led Wilkinson around the latter site (Carrara, 1848; Wilkinson, 1848).

On the approach to Salona, Wilkinson noted seven arches of an aqueduct – the same sign of a Roman presence that he had seen near Zara. Unlike at Zara, Wilkinson, or rather Carrara, encountered much more substantial Roman remains, in the form of a circuit of city walls. In some ways, the survival of parts of these served as a recompense for the almost total lack of civil remains with the possible exception of an amphitheatre: it was possible to trace out the area formerly occupied by the ancient city, if nothing else. Wilkinson sketched out the city walls at both small and large scales in his journal (Figs 6.5-6.6).[11] In addition to the obvious similarity in the modern name of the city – it was virtually identical to the ancient one that could be written as either Salona or Salonae – the shape of the city enclosed by the walls, and its seaside location, were the key factors which convinced Wilkinson that the ruins were those of ancient Salona, described by Lucan. Strictly speaking, only the northern circuit of the walls remained: there were none on the seaward side and he was left to mark the presumed course with dashed lines. Here Wilkinson held that some geomorphological changes had taken place in the intervening centuries: while the river Giadro had stayed stationary – the survival of parts of the arches of a Roman bridge indicated this since they were unlikely to have been built on dry land – the bay of Salona had once extended further north. Given that his journal only gives slightly more extensive outlines, it seems unlikely that Wilkinson performed

[11] "Journal", 1844-45, Bodleian Library Wilkinson manuscripts, dep. d. 58, folios 45, 50.

geomorphological fieldwork here of the sort that he had undertaken in Egypt: like his remark on coastal change in relation to the “ancient pavements” near Zara, it must be concluded that his observation was based on pure supposition, or else knowledge supplied by Carrara.

Image omitted due to copyright restrictions.

Fig. 6.5: Small-scale map of Salona and the town walls, at centre of image, from “Journal”, 1844-45, Bodleian Library Wilkinson manuscripts, dep. d. 58, folio 45.

Image omitted due to copyright restrictions.

Fig. 6.6: Large-scale map of Salona and the town walls, from "Journal", 1844-45, Bodleian Library Wilkinson manuscripts, dep. d. 58, folio 50.

As well as tracing out the extent of Salona, Wilkinson gave his readers a brief account of why his topographical investigation mattered: it had important historical associations, among them was a visit by Julius Caesar. At the same time as building up Salona's historical reputation, Wilkinson to some extent cut the place down to size: he noted a tradition that it had once been half the size of Constantinople (presumably when it had functioned as the capital of the eastern Roman empire) was a wild exaggeration. However, he still regarded the city, and the excavations made by Carrara after he left Dalmatia as of sufficient importance to forward them to the British Archaeological Association – an assembly of antiquaries who had broken with the Society of Antiquaries of London as a result of their feeling that the existing society was not sufficiently intellectually engaged (Hill, 2021; Evans, 2007) – which published an account of them in the third volume of its *Journal*.

If Zara and Salona represented near-opposite extremes of the amount of material evidence of ancient Roman cities that could be traced on the ground, the ancient city of Burnum, or ancient Liburna – which Wilkinson visited during a visit into the interior of Dalmatia – lay somewhere between the two. On the approaches to this ancient town, mentioned in the Peutinger Table and by Strabo as being on the right bank of the river Titius (the modern Kerka) – Wilkinson noted the familiar sign of an ancient Roman

presence: a series of stone arches, some of which featured Corinthian capitals. Unlike at Zara and Salona, where these features had been interpreted as aqueducts, the debate here was as to whether the arches formed a standalone triumphal arch, or whether they had formed part of the city walls of ancient Burnum. Alberto Fortis, who believed that the shapes of some of the stones showed that they had been unconnected to curtain walls, favoured the former explanation of a five-gated triumphal monument, which either he or another unnamed party linked to Trajan's Dacian wars. Wilkinson, on the other hand, straight after his visit to Salona, became convinced that they were part of the ancient city walls. Regardless of which, the next aim in each case was the same: to ascertain precisely where the ancient Roman city that had presumably been in the vicinity, was located. Wilkinson's opinion was

> It is probable that the city stood on the south [side] of the arches, and occupied the space between them and the river, which is distant about one-third of a mile. On its steep bank I observed the remains of Roman masonry, of small stones; and in the midst of this were two small tubes, apparently conduits for water, running into the rock to the northward, and nearly at the top of the bank (1848: 210).

Essentially, by paying attention to classical sources at a broad scale, and then focusing on antiquities, both the monumental (stylistically Roman-era arches) and those that were far from being aesthetically impressive (Roman masonry), Wilkinson followed an approach very similar to that which he had taken in Upper Egypt in his investigation of the extent of the ancient Egyptian empire to the south. Wilkinson had not been the first traveller or scholar to make the observation in either instance, although in both cases he included the observations in a topographical work: there were wide areas of overlap between his *Topography of Thebes, and General View of Egypt* and *Dalmatia and Montenegro*.

6.3 Visual Cultures of *Dalmatia and Montenegro*

Alongside Wilkinson's text, *Dalmatia and Montenegro* featured a series of images, both woodcuts and lithographs, at a range of scales, similar to many other 19th-century travel

books. As Keighren *et al.* (2015), Koivunen (2009), Bridges (2002) and Heringman (2013) have noted, such images formed a vital component of travel books, and were crucially important for establishing impressions among readers, whether for setting up places as exotic, or just to convey information to complement that which was conveyed in the text. At the same time, images were not entirely unmediated representations of reality. First, travellers tended to draw many images, of which only a small fraction were published, so there was selection – in Wilkinson's case, this was from literally hundreds of watercolours, sketches and line drawings. Second, the images that were chosen were engraved by artists who had not – in all likelihood – seen the original places, and these artists embellished scenes according to standardised practice, so the result was that the images were manipulated (Koivunen, 2009). This is definitely true of Wilkinson's images, as, in many cases, those in *Dalmatia and Montenegro* differ from the originals in his manuscript collection. However, the published images did not simply differ from the originals as a result of unintentional and conventional practice that formed an inherent part of the engraving process: in several, features such as people, vegetation, rocks and – in one case – an entire building were added or were omitted. No correspondence appears to have survived as to who – out of Wilkinson, John Murray, Murray's staff or the engravers – made these decisions.[12] Nevertheless, by looking at them in sequence, it is evident that those in the north of Dalmatia were largely unaltered, while those in the south, especially those close to Montenegro, were heavily retouched: the result is that picturesque imagery steadily morphed into sublime scenery, in which the human scale became almost lost – a gradient which very much reflects the differences in how Wilkinson portrayed Istria/ Dalmatia and Montenegro in his book.

[12] The correspondence between Wilkinson and John Murray in the John Murray archive (National Library of Scotland) makes virtually no mention of the images, and no mention at all of their alteration. At the same time, it is clear that it only represents a sample of all the conversations that Wilkinson and Murray had: it does not include Wilkinson's pitching of his book, nor does it include any letters from John Murray. Only three letters from Murray to Wilkinson appear to exist in the Wilkinson manuscripts (Bodleian Library), none of which are remotely close to the date *Dalmatia and Montenegro* was published, or to those of the aforementioned letters.

6.3.1 Zara, Spalato and Diocletian's Palace: an exercise in the picturesque

Excluding the frontispiece fold-out image of Sebenico, the first full-page images that a reader of *Dalmatia and Montenegro* (1848) would have encountered, on leafing through the book from start to finish, were a woodcut drawing of Zara Cathedral and several images of Spalato. The image of Zara Cathedral (Fig. 6.7) is reasonably straightforward: it shows the west façade, north wall, and some adjoining buildings, as well as a couple of people in local dress standing in front of it. The woodcut image is virtually identical to the wash drawing that Wilkinson drew of the building on his visit to the town (Fig. 6.8).[13]

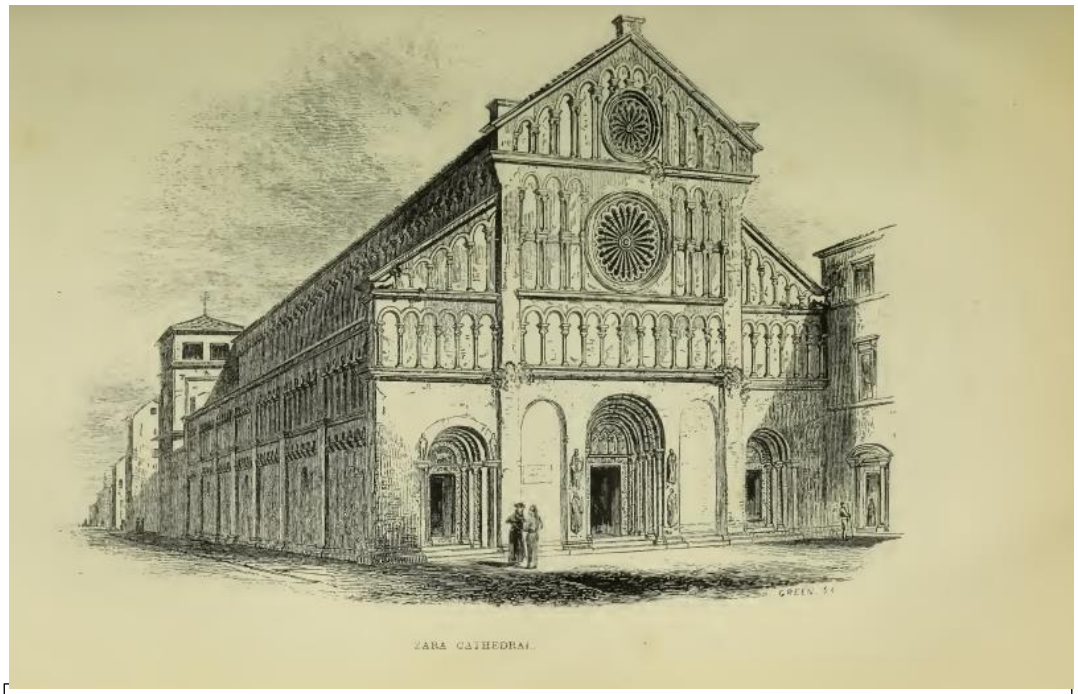


Fig. 6.7: Zara Cathedral, woodcut image from Wilkinson's *Dalmatia and Montenegro*, opposite p. 80.

The images of Spalato are more numerous: there are three whole-page woodcut images of scenes from the city, mostly centred on the ruins of Diocletian's Palace: the first shows the Porta Aurea gateway, the second shows a court of the palace, and the third shows the "Temple of Saturn" which had been converted into a cathedral dedicated to Saint Dominus. In addition to these close-up views of urban scenes, the section features a

[13] "Sketchbook", 1844, Bodleian Library Wilkinson manuscripts, dep. e. 7, folio 18.

lithograph view of the whole town of Spalato from offshore, with hills rising above, with the tallest slightly higher than the tower of the cathedral.

Image omitted due to copyright restrictions.

Fig. 6.8: Image of Zara Cathedral, "Sketchbook", 1844, Bodleian Library Wilkinson manuscripts, dep. e. 7, folio 18.

An interesting feature of some of the images is the presence of vegetation in association with some of the buildings. Whilst there is none visible in the image of Zara Cathedral, two of the three woodcut images of Diocletian's Palace feature vegetation: the image of the Porta Aurea features a stand of rushes growing in front of, and partly obscuring it (Fig. 6.9), while that of the Temple of Jupiter features small traces of vegetation growing out of the stonework itself (Fig. 6.10).

At a straightforward level, Wilkinson's depiction of buildings partially obscured by vegetation documents that parts of Diocletian's Palace were out of use, with the near-inevitable result that they would appear neglected – unlike Zara Cathedral, which was in constant use. At a deeper level though, Wilkinson's decision to include vegetation in his drawings when he could easily have excluded it, as Robert Adam and James Stuart

occasionally did, hints that he was conditioned by an aesthetic sensibility common at the time amongst those with the time and expense to travel for pursuing it: an enthusiasm for the picturesque (Andrews, 1989). Instead of viewing an abandoned building in a negative light, a detached viewer could take delight in the presence of vegetation on or framing it; the Reverend William Gilpin regarded these landscapes as the most pleasing ones due to the contrast, which positioned them between the beautiful and the sublime. In Britain, Bodiam Castle had attracted visitors due to its appearance as an ivy-clad ruin as much as it had due to its historic or other associations, and other sites such as Tintern Abbey and Netley Abbey were similarly viewed (Townshend, 2017; Watt, Townshend and Watson, 2017). Sometimes a political layer was present: in the 18th century, Uvedale Price saw ruined castles and abbeys as demonstrations that absolutism and religious dogma had lost their power, although this was not an essential element (Carter, 2017; Watkins and Cowell, 2012). Overseas, Rome in particular attracted visitors for its historic associations and its pleasing aesthetics (Pemble, 2017).



Fig. 6.9: Porta Aurea gateway, Diocletian's Palace, with a stand of vegetation, woodcut image from Wilkinson's *Dalmatia and Montenegro*, opposite p.126.

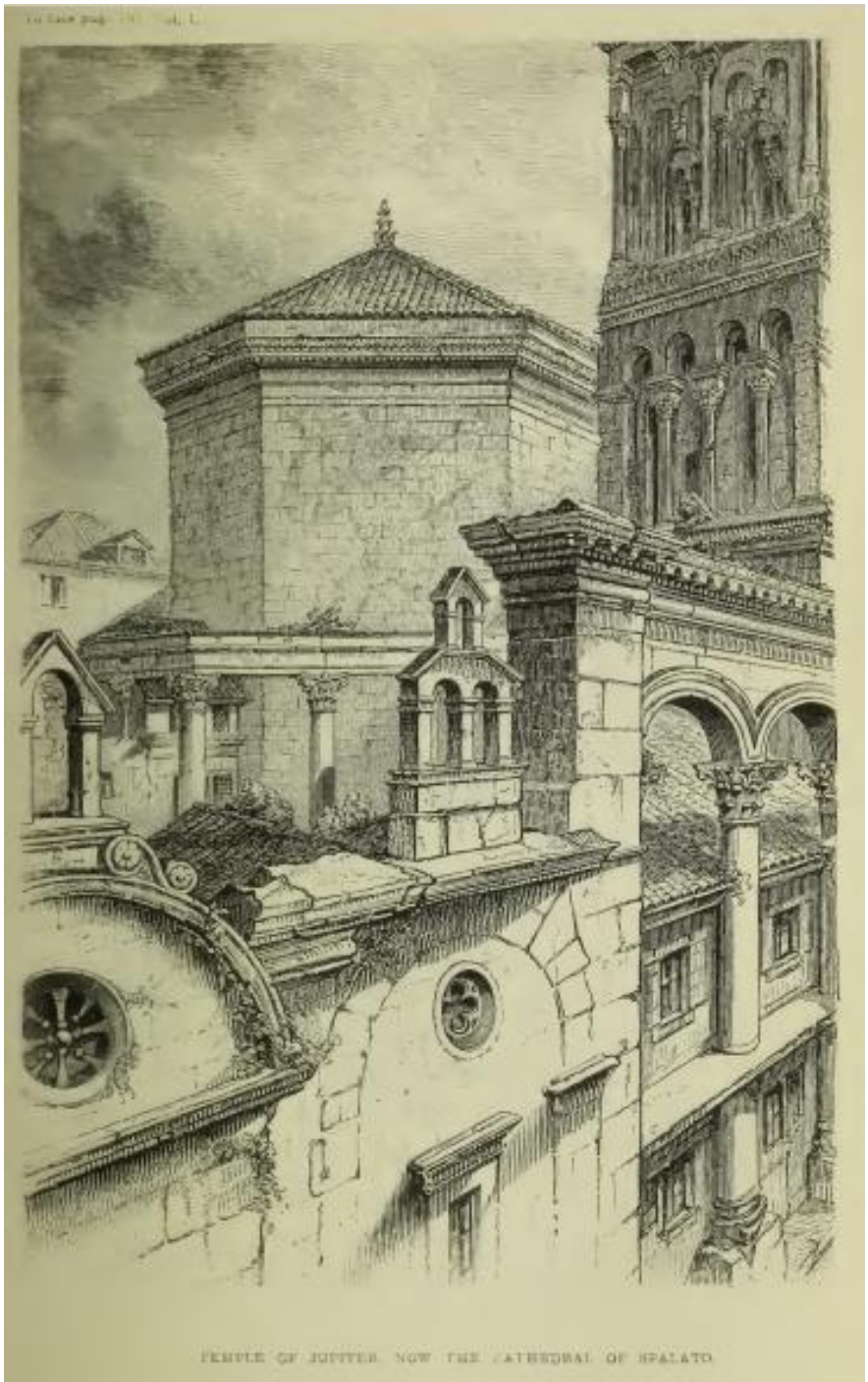


Fig. 6.10: "Temple of Jupiter [Saturn], now the Cathedral of Spalato", with a very small amount of vegetation on the walls, see in particular those at the lower left, woodcut image from Wilkinson's *Dalmatia and Montenegro*, opposite p. 130.

The extent to which this way of seeing was a particularly attractive one is demonstrated by instances of travellers and artists going far out of their way to find or even invent it.

The Search for the Picturesque, as Malcolm Andrews' (1989) book is titled, was a very real and now well-known cultural practice in Georgian Britain, in which early tourists favoured particular viewpoints in the Lake District and other upland regions, and where these were not available, they could use a portable Claude Glass to make them more appealing. Where picturesque landscapes did not exist, travelling artists were quite happy to make them up: such literary and artistic license was even recommended by William Gilpin. An example of this in practice was James Bruce, who emphasised that his pictures of ruins in North Africa had been drawn with precision since he had used the camera obscura, though at the same time he freely admitted that he had added numerous small bushes to the scene, where none existed, to make it appeal more to a public which favoured picturesque representation (Leask, 2002). Wilkinson's friend, fellow traveller in Egypt, and an engraver by profession, Edward William Lane, admitted to similar liberties in his own work (Thompson, 1992a). In other cases, this artistic licence was institutionalised at the employment level; Koivunen (2009) notes that in the 19th century it was common for travellers in Africa to hand over their drawings to professional engravers, who embellished them with additions that they thought would appeal to their picturesque-obsessed audience. These frequently went over and above differences which would inevitably have arisen during the engraving process when the engraver had not seen what they were depicting. Common additions included atmospherically dark or clouded skies, heightened mountains, costumed figures and most commonly of all, the addition or removal of vegetation, to give maximum appeal.

Such devices are definitely present in some of Stuart's (1756) pictures and nearly all those by Allason (1819). The overall result is to suggest that as far as illustrations were concerned, it was important to be accurate, but truth could sometimes be relegated to secondary importance behind aesthetics, with the most important aesthetic being the picturesque.

In the case of Wilkinson's illustrations in Spalato, there is reasonably little difference between the plates in the published book and the original manuscripts when it comes to vegetation: the stand of vegetation shown in the image of the Porta Aurea was one that Wilkinson recorded on his visit: the only discernible difference is that whereas Wilkinson drew a largely undefined mass of vegetation, the engraver distinguished the fronds of the

plant to a higher degree (Fig. 6.9; Fig. 6.11).[14] The amount of vegetation shown in the image of the "Temple of Jupiter" is nearly identical in each case. This effort, when the stand of vegetation could easily have been cut out altogether, suggests that the engravers were conscious of and aware of the appeal that the picturesque aesthetic would have for readers, as was Wilkinson himself.

Image omitted due to copyright restrictions.

Fig. 6.11: Image of the Porta Aurea gateway, Diocletian's Palace, "Sketchbook", 1844, Bodleian Library, Gardner Wilkinson manuscripts, dep. b. 2, p. 11. Compare with figure 6.9.

Instead, the most significant changes to the images are a series of additions: weather, people and antiquities. In the images of the court and the "Temple of Jupiter", a dark, cloudy sky is a new addition. Perhaps the most significant additions are those of more costumed figures: three have been added to the image of the Porta Aurea, while an additional group of three has been added to the image of the court. Another notable addition is a carved fragment resembling a section of a cornice – but more likely intended

[14] "Sketchbook", 1844, Bodleian Library, Gardner Wilkinson manuscripts, dep. b. 2, p. 11

as a fallen version of the projections visible on the wall behind – in the foreground of the image of the Porta Aurea, likely added to give another point of focus in the image. This last change – of which more will be seen below – is the most manipulative, but interestingly it is not an addition without basis in reality: on the opposite page of his sketchbook of images of Diocletian's Palace, Wilkinson drew a close-up of this precise feature. As such, the engraved image should more properly be regarded as a composite image than an invented one, and there exists precedent for this in Wilkinson's *Manners and Customs*, in which a charioteer is shown with equipment derived from several sources.

6.3.2 In Darkest Dalmatia? From the Picturesque to the Sublime

In addition to four woodcut images depicting notable buildings and architecture in Zara and Spalato, Wilkinson's book also included four more lithographs of buildings amidst their environmental backdrop, along the Dalmatian coast. Additionally, he included two more lithographs of scenes in Montenegro. Whereas his images of Spalato are also of a similar style, albeit obviously of different features, his landscape views show an interesting trend: as Wilkinson (and the reader) moved south, the landscape became more rugged, and the presence of human activity, whether of people, vessels or buildings, rapidly diminished. Overall, the result is that his audience is treated to a spectacle in which the picturesque gradually gives way to something approaching the sublime.

Wilkinson's lithograph plate showing Spalato from the sea (Fig. 6.12) – the largest, best-connected and, but for Sebennico the most northerly of the four Dalmatian landscape scenes – presents in many ways an intermediate image between the aforementioned urban scenes and the subsequent landscapes. In it, the town takes up nearly the whole length of the picture, with the tower of Saint Domnius' Cathedral especially prominent. Many ships by the quayside can be taken to imply that there is trade going on, so the place is of some economic significance, although these are not the main focus of the engraving. At the same time, the picture contains new themes which later become more prominent. In the background, a range of hills is depicted slightly higher than the much

closer cathedral tower, while there are some jagged rocks and a small lone ship in the foreground. The effect of these two framing devices gives the impression that Spalato is an oasis of civilization in an otherwise desolate and uninhabited area. It may not be coincidental that Wilkinson's first foray into the hills of the Dalmatian interior behind the town was from Spalato.

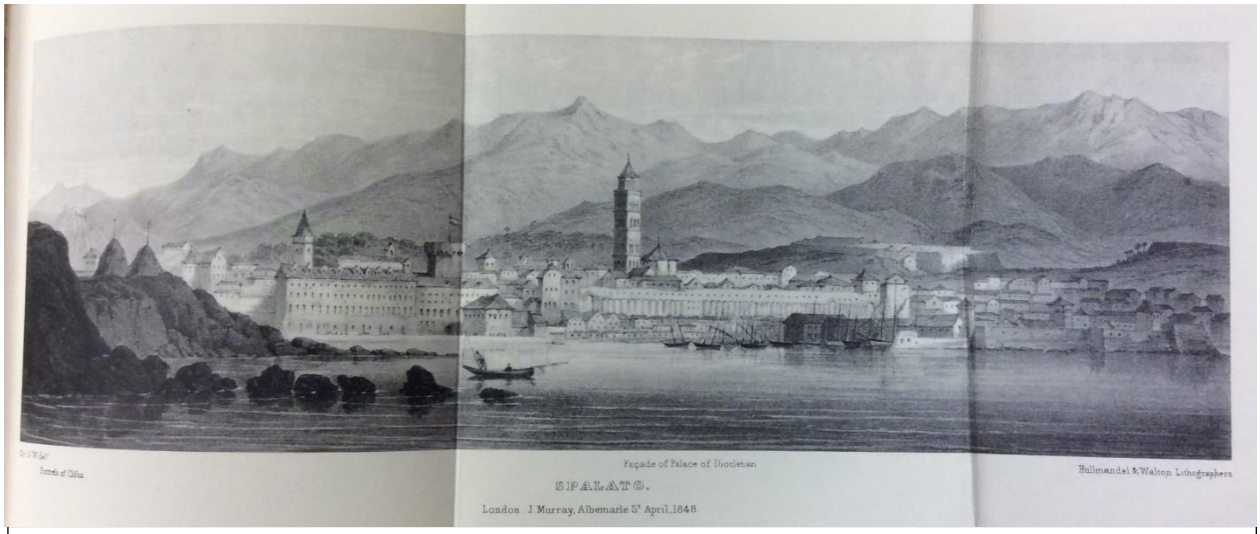


Fig. 6.12: Spalato, lithograph image from Wilkinson's *Dalmatia and Montenegro*, opposite p. 114.

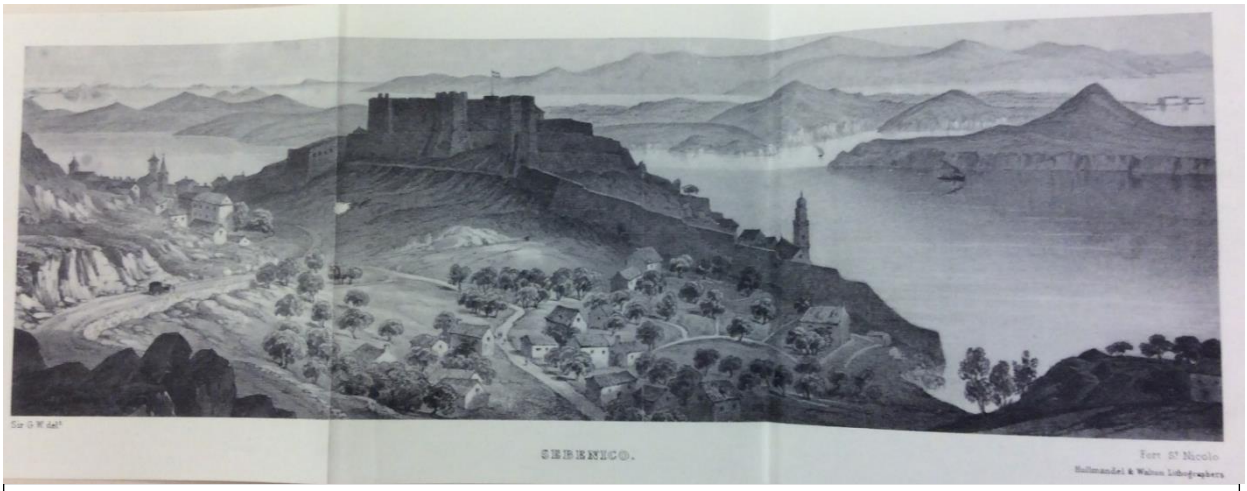


Fig. 6.13: Sebenico, lithograph image from Wilkinson's *Dalmatia and Montenegro*, Frontispiece.

A second example of this intermediate type of view is Wilkinson's plate showing the Fortress of St. Anna (1848: 95), overlooking the harbour area of Sebenico (Fig. 6.13). At one level, the image shows a scene which is clearly dominated by human activity, not least by the castle itself and the substantial buildings to the left and right of it. On the left

of the image, there is also a well-defined engineered road, cut through a hillside, as can be seen from the vertical rockface cutting. There are also a horse and stagecoach on it.

In addition to these signs of civilization, the image also has themes common to picturesque ones. Like the previous image of Spalato, there are some jagged rocks in the foreground and a barren background, with several offshore islands, such as Zlarin, on the horizon. However, these arguably go further than the image of Spalato, as the harbour is almost empty of ships, while the land appears far more barren; there is also less of the urban area visible. Though Wilkinson reported the town as having a population of 3,000 people, the image makes this figure seem unlikely. This is because most of the built environment is between the harbour and the castle; the latter essentially hides the town and makes Sebenico look far smaller and insignificant than it really was: the scene is a countrified one. This is helped even more by the central part of the picture. In contrast to the engineered road to the left, this features a smaller, narrower track, with even smaller paths leading to a collection of seemingly randomly arranged cottages, interspersed with what could be an orchard of fruit trees. These cottages, though several appear to have chimneys and so are permanent buildings, are very different to the larger substantial buildings at the left of the picture and in fact have more in common with the scenes of the countryside of the Dalmatian interior and Montenegro, almost as if Wilkinson has lifted a rural scene and substituted it into what should have been an urban scene.



Fig. 6.14: Image of the Fortress of St. Anna, Sebenico, “Sketchbook”, 1844, Bodleian Library Wilkinson manuscripts, dep. e. 7, folio 22.

A comparison with the original sketch (Fig. 6.14), which is in fact a comparatively small and insignificant one compared to those of Spalato, shows that this is not the case; the scene is almost the same.[15] However, one noticeable change, far larger than anything in comparison with the scenes of Spalato, is that the cliff that the road has been cut through is fictional, if the assumption that the on-scene drawing is the more reliable of the two.[16] The impact of this change is not entirely straightforward, since it may be taken to lessen Sebenico's isolation by implying that some other agency cared enough about it to create a linking route at presumably great expense. More likely perhaps, the change was intended to emphasise that this is the only connecting route and that it was won at great difficulty, a bit like a miniature "pass" from the mountains into the town.[17] If this is indeed the case, then this alteration enhances the likely function of the engraving, to create a strong visual prompt to the reader that this is a liminal place where the traces of civilization are wearing thin, possibly just clinging on and will soon disappear as Wilkinson heads further south.

The last town in Dalmatia that Wilkinson showed is Cattaro (Fig. 6.15). Although today part of Montenegro, at the time of Wilkinson's visit in 1844, this was the southernmost town of any size in the Habsburg Empire. It is also the last town that Wilkinson visited before he ventured into Montenegro. Briefly summarised, the image shows the town of Cattaro from a head-on view and from a low vantage-point from across the bay of Kotor, with a series of hills rising to a considerable height above it. Comparisons of the view with the modern town and its setting indicate that there is little reason to doubt that Wilkinson's picture is anything other than a faithful representation in its broad outlines.

[15] "Sketchbook", 1844, Bodleian Library Wilkinson manuscripts, dep. e. 7, folio 22.
[16] This will be the working assumption for what follows. Of course, it is possible that John Murray's engravers were familiar with the inland approach to Sebenico/ Sibenik and noticed that Wilkinson had forgotten to include the cutting, though this seems to be making too many assumptions.
[17] Andrew Paton used similar language to describe the ascent from the snowy mountains to the warmer sea in *Highlands and Islands of the Adriatic* (1849).

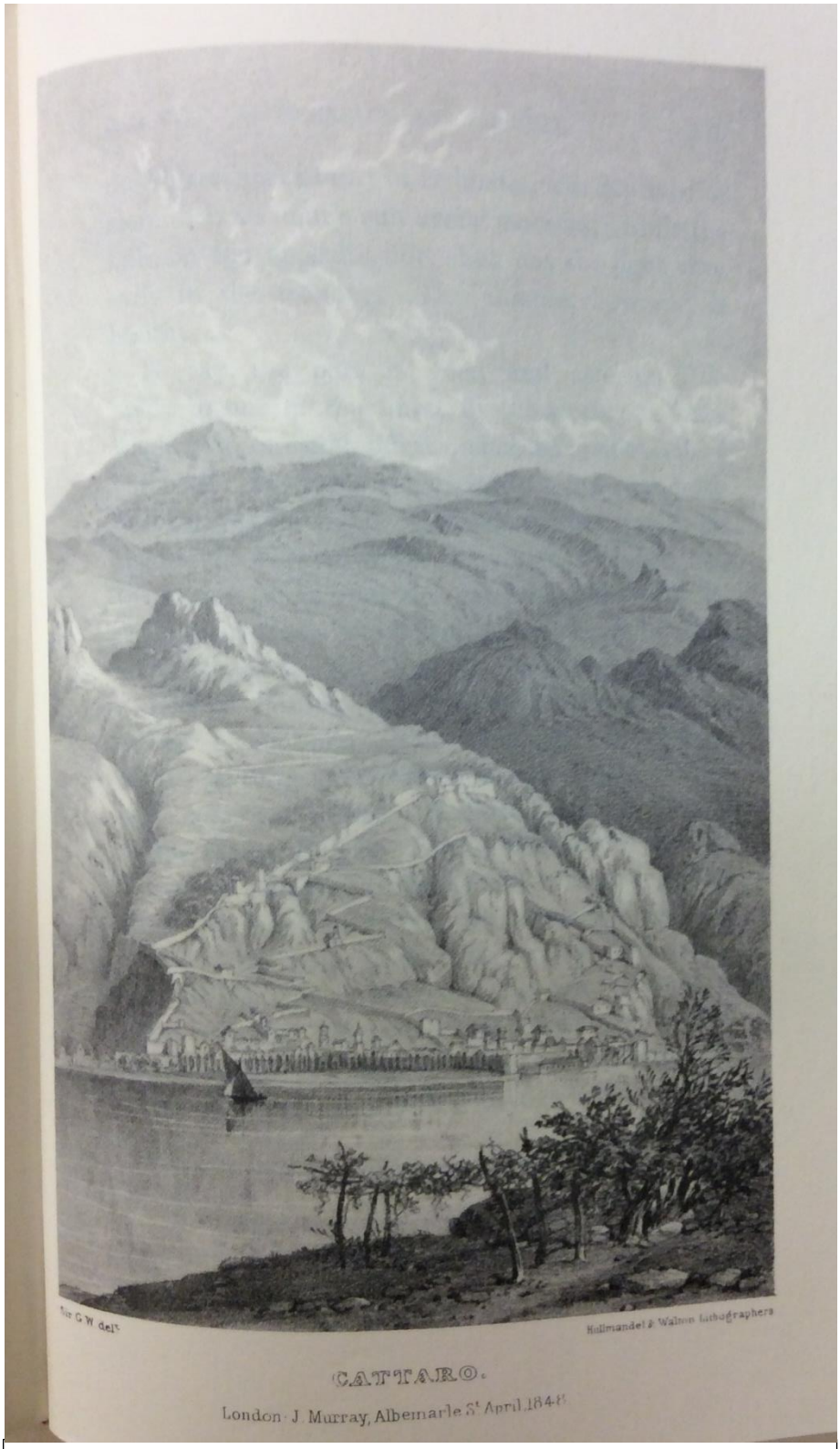


Fig. 6.15: Cattaro, lithograph image from Wilkinson's *Dalmatia and Montenegro*, opposite p. 398.

Instead, the issue is one of perspective: the head-on angle that Wilkinson chose is significant. This does much to diminish the town, which is evidently quite large, according

to a drawing made of it three decades earlier by Wilkinson's mentor, Sir William Gell, in 1811 (Fig. 6.16).[18] Wilkinson's image therefore makes use of a similar trick with perspective to deceive as that of the engraving of Sebenico. In fact, it goes further: whereas Wilkinson's view of Sebenico was from a high and commanding vantage point, his view of Cattaro is from an artificially low point, which has the precise opposite effect: it creates an image that is far more allied to the sublime.

Image omitted due to copyright restrictions.

Fig. 6.16: Cattaro, drawing by Sir William Gell, 1811. William Gell, "Cattaro", c. 1801-1813. The British Museum, 1853,0307.597. Available at: https://www.britishmuseum.org/collection/object/G_1853-0307-597 [Accessed 2nd February 2022].

The reason for these very particular choices appears to be that despite the letterpress purporting to explain that the image depicts a town, and not a particularly well-known one at that, it is actually intended to show the frontier between Dalmatia and Montenegro. Here, the Habsburg Empire is only a very thin veneer of civilization. The battlements extend far less than halfway into the mountains and the town is overlooked by the land held by the enemy. The desolate foreground across the bay, with bare, rocky

[18] William Gell, "Cattaro", c. 1801-1813. The British Museum, 1853,0307.597. Available at: https://www.britishmuseum.org/collection/object/G_1853-0307-597 [Accessed 2nd February 2022].

ground and gnarled trees, as well as the lone boat are the only features which preserve some sort of human scale. Even with them, the impact of this plate is greater than any other Dalmatian scene in the book: it is hard to imagine a greater contrast with Spalato, in which the Cathedral tower vies with the mountains beyond it. Inserted in the corresponding part of the book – as opposed to the print of Sebenico which forms the frontispiece – the image perfectly encapsulates the text in which Wilkinson referred to the mountains above Cattaro blocking out the sun for hours each day, and also plays the role of being a prelude for the reader to the most adventurous part of the book, in which Wilkinson crosses the border into Montenegro, into the unknown.

The real impact of Wilkinson's choice of perspective and artistic techniques can be better appreciated when they are compared with other images which do not use them, but nevertheless are of the same place. William Gell's (1811) sketch of Cattaro is a particularly good example in that it uses a series of artistic techniques precisely opposite to those used by Wilkinson. The result is that Gell's view is closer to the picturesque and conveys a totally different impression to that of Wilkinson's view, which is far closer to the sublime. The biggest difference is that Gell's sketch is from a different perspective: it is more or less at right-angles to Wilkinson's one, which means that the town is in the precise centre of the picture, although it is still too remote to distinguish many details. On the left of the picture, Gell shows the fortifications above Cattaro snaking over the highest peaks. This is in marked contrast to Wilkinson's drawing, which shows these fortifications at a low elevation and although impressive, potentially of doubtful use in the event of an attack from the mountains. Comparing Gell's drawing with Wilkinson's, along with photographs of the scene today, it is clear that Gell omitted the highest peaks, which overwhelm the town, as they do in Wilkinson's image. Though this can be regarded as deceptive, it is also a necessary limitation of recording a three-dimensional world onto a two-dimensional surface.[19] Wilkinson's image is also limited in the same way: the hills on the other side of the bay, shown by Gell, are behind the viewpoint and so omitted entirely. This is important, as these hills are much lower than those above Cattaro and so the latter is in a dominant position. Thus, Wilkinson's omission of them means that the

[19] Without it being a panorama, which was also popular at the time, but not usually for this sort of picturesque scene.

overall impact of his sublime scene is not weakened by their inclusion, whereas Gell's picturesque scene is enhanced by their inclusion, since they enable him to maintain a human presence (the zigzagging fortification walls extending above Cattaro) on the highest peaks, which lessens the image of wild nature as conveyed in Wilkinson's engraving and suggests a more civilized region.

An additional important difference between the two is the foreground. Whereas Wilkinson's foreground is relatively simple, constituting a low-lying area with bare trees and rocky ground, adding to the desolate scene, Gell's foreground is at a comparatively higher level, looking down on Cattaro. The vegetation is of a quite different nature: it consists of bushy trees and plants, also visible on the other side of the bay. Most importantly of all, Gell shows two people in local costume on the left-hand side, possibly a shepherd and a female companion, the latter looking toward the viewer: the inclusion of local people, usually peasants, was a common feature of the genre. In Wilkinson's image, however, the inclusion of such features would have seriously undermined the sublimity of the overall effect.



Fig. 6.17: Cattaro (frontispiece of Paton, *Highlands and Islands of the Adriatic*, 1849).

To further reinforce these ideas, it is interesting to compare a third view of the same scene (Fig. 6.17): the frontispiece from Andrew Paton's *Highlands and Islands of the Adriatic* (1849), which eschews both picturesque and sublime conventions altogether and has an eclectic mixture of both. This has towering mountains in the background, with one range even taller than those portrayed by Wilkinson's image. However, the impact of this is offset by a view of the town from an angle which does not diminish it in the way that Wilkinson does. It also has several large ships in the harbour, which are far more prominent than those shown by Wilkinson in his picturesque engraving of Spalato. Paton's foreground features bushy vegetation, a viewing point and several well-dressed figures. Barring the small size of the people, it is much more like Gell's image, which incidentally suggests that it is valid to compare Wilkinson's and Gell's images as has been attempted here, despite the 30 years of time that separate the two. The main point however is that Paton's image is not so aesthetically consistent as Gell's or Wilkinson's, and so this suggests that the images of the latter two artistic travellers should be critically examined and not simply taken to stand in for reality: in some ways their process of selection – just like Wilkinson's selection of a countrified view of Sebenico rather than an urban one – helped to create it as much as they did accurately portray it.

To settle any doubts about the above contention that the engraving of Cattaro was consciously intended to be closer to the sublime than to the picturesque than any of the previous plates accompanying the pages on Dalmatia, it is useful to compare Wilkinson's engraving to the original drawing in his manuscript collection, which is dated 14th September 1844 (Fig. 6.18).[20] Even a cursory glance reveals substantial differences between the original watercolour and the engraved lithograph facing p. 398 in *Dalmatia and Montenegro* (1848) – differences that are far more pronounced than those between the engravings and Wilkinson's original sketchbook images of Diocletian's Palace and Saint Domnius' Cathedral at Spalato. These go well beyond any unconscious artefacts introduced in the process of engraving: the colours of the original are lost, but so too is much else. New elements are added too. Most notably, the foreground is completely different: whereas the engraving has only bare ground and small, seemingly dead or wind blasted trees, Wilkinson's watercolour of Cattaro, presumably made on-the-spot,

[20] "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. c. 1, Folio 12.

features bushes like William Gell's drawing (Fig. 6.16), a house and a cypress like tree. Of these, the house recalls the frequent inclusion of a country cottage or hovel in picturesque images, while the tree recalls the convention of using a framing device. Together with a large bird on the left side and the landscape orientation (as opposed to portrait) of the picture, this tree totally destroys the towering height which is such a prominent element of the engraved image in *Dalmatia and Montenegro*.

Image omitted due to copyright restrictions.

Fig. 6.18: Cattaro, coloured drawing by Gardner Wilkinson, showing considerable differences with the published version. The lines show the part that the lithograph was made from. The foreground was changed. "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. c. 1, Folio 12.

The contrast of the towering hills has also been heightened in the engraving, much more than in the original. The fact that either Wilkinson, John Murray, or the engravers, thought it was worthwhile to carry out these large alterations to the original – especially the entire replacement of the foreground with what is essentially another picture – so as to lessen the picturesque elements and heighten the sublime elements, strongly suggests that the illustrations were regarded as of fundamental importance in helping to shape reader's attitudes toward different regions; they were not just for decoration.

Even if it cannot be ascertained whether or not it was Wilkinson who was instrumental in making the changes – a proof copy of his book in which he scribbled numerous notes includes the scenes of Spalato but lacks the lithographs including that of Cattaro – it is useful to remember that his drawing of Cattaro was only one of several: on the same day that he sketched Cattaro from across the water, he recorded another image (Fig. 6.19).[21] This consisted of a fair, populated by several dozen colourfully costumed figures, standing at the foot of the hills which so overwhelmed his engraving of the same town. The scene is a tame one, equivalent to the images of Diocletian's Palace, and shows that Wilkinson himself was well aware of diversity across the country, even if (presumably) he did not feel that it was appropriate at this stage to share it with his readers.

Image omitted due to copyright restrictions.

Fig. 6.19: Another view of Cattaro, by Gardner Wilkinson. "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. c. 1, Folio 11.

[21] "Proof copy of *Dalmatia and Montenegro*", 1848, vol. 1, p. 24, Bodleian Library, Wilkinson manuscripts dep. d. 86; "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. c. 1, Folio 12.

6.3.3 Montenegro: the sublime triumphant?

The chapter of *Dalmatia and Montenegro* devoted to the latter territory features two full-page lithograph images: the ones that would have done the most to mould readers' first impression of what Montenegro was like. Both of them are totally devoid of buildings and only feature natural landscape scenes. This is unlike any of the Dalmatia lithographs, including that of Cattaro and Ragusa, the only one not discussed in detail above, all of which show at least a few buildings, even if they are dominated by the tree-covered hills behind them.

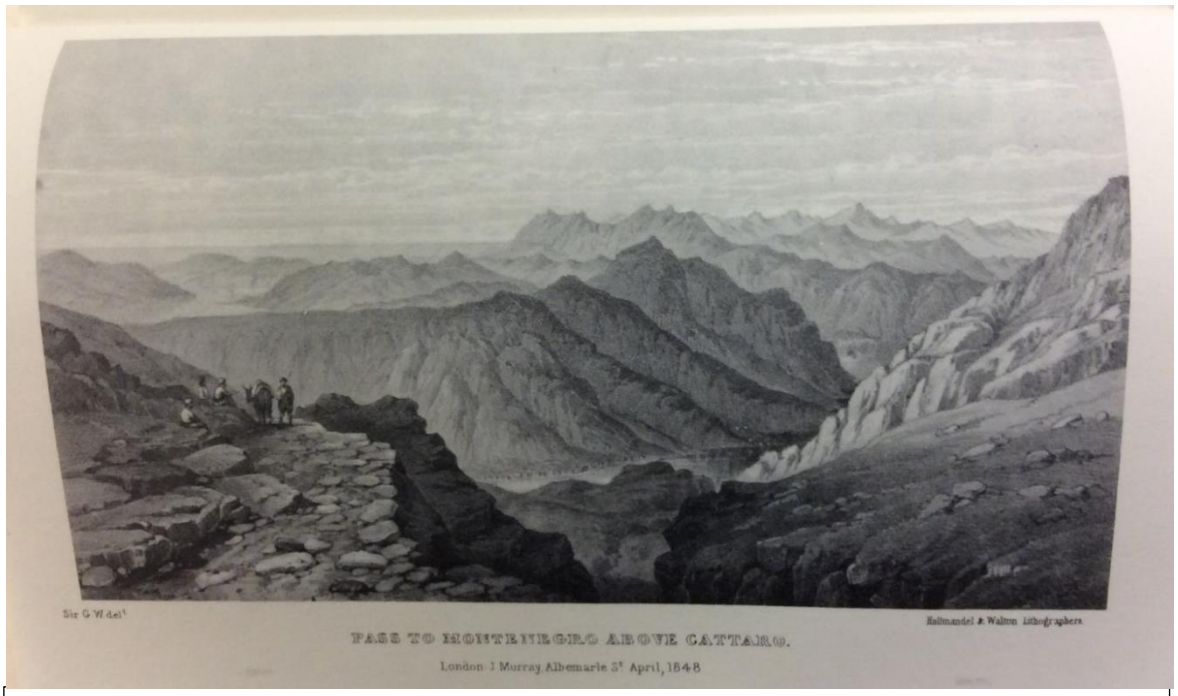


Fig. 6.20: Image of "Pass to Montenegro above Cattaro", Wilkinson, Dalmatia and Montenegro, opposite p. 498.

The first of the images of Montenegro is labelled as the "Pass to Montenegro above Cattaro" (Fig. 6.20). This shows some men and donkeys on a rough track cut through the rock, which possibly runs down the chasm in the centre of the picture, looping back on itself, in the zigzag fashion of the walls and paths shown on the print and engraving of Cattaro. Somewhat ironically, despite the caption claiming to show the pass leading to Montenegro, the image actually appears to face back to Cattaro and the Adriatic, in a similar way to the image of Sebenico discussed above, which actually features offshore

islands in the Adriatic, heading towards Italy. A possible explanation for this puzzle may be that Wilkinson and John Murray were simply trying to choose the most dramatic scene, irrespective of whether or not it made any sense at a literal geographical level. Given that it is necessary to look at the pictures a few times and match them up with atlases or satellite views to identify them, actions which would not have been relevant or available to most readers, it is presumably possible that Wilkinson's publisher did not see any issues with including images which were in some ways misleading.

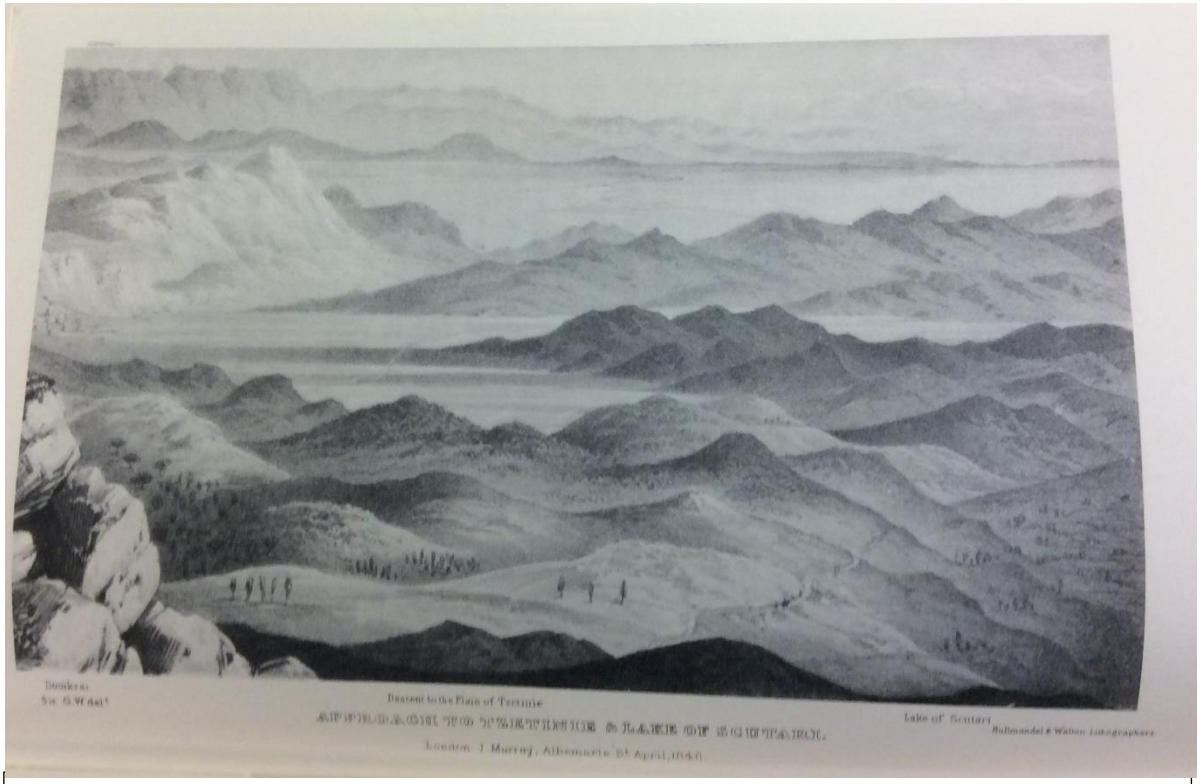


Fig. 6.21: Image of "Approach to Tzetine and lake of Scutari", Wilkinson, Dalmatia and Montenegro, opposite p. 503.

The second lithograph image of Montenegro, labelled "Approach to Tzetine and Lake of Scutari" (Fig. 6.21) shows a range of bare hills, which suggest the land was used for rough grazing, a form of pastoralism common across much of Mediterranean Europe (Grove and Rackham, 2003). The view also features a small number of trees, including two groups of pines on the tops of hills, a point normally occupied by people in such views. It is difficult to ascertain whether the features in the extreme left side of the image are meant to be a sign of human activity, just as it is to establish whether a feature in the middle of the path at the lower right is a person on horseback. Regardless of these points, the overall

impression conveyed is very much one of a landscape devoid of human activity – the artificiality of the pastoral landscape, which could easily pass for being natural, itself excepted. Whilst the image is framed – a convention more typical of the picturesque – by rocks on the lower left-hand side, the overall impression is an overwhelming one, with the human elements very much subordinate to the natural features – the track in the foreground differs hugely from the engineered road in the engraving of Sebenico – in other words, it is closer to the sublime.

Image omitted due to copyright restrictions.

Fig. 6.22: "Pass from Montenegro to Cattaro", "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. c. 1, Folio 19.

Both of these engravings differ slightly from the originals in Wilkinson's manuscripts, to enhance the sublime elements. In the first, the path has been made rougher, with the addition of an uneven surface and the occasional boulder: a modification equivalent to the technique used in the engraving of Sebenico with the enhanced cliff cutting (Figure

6.22).[22] Like the engraving of Cattaro, the mountain peaks have been made more jagged and the contrast greatly enhanced. In the second engraving, of mountains in front of the Lake of Scutari, very similar techniques have been used: the hills are more rugged, the contrast is altered and, in the case of the hills nearest to the viewer, shading has created a new feature of a foreground, as if the view is from a height appropriate for viewing such a vast area of emptiness (Figure 6.23).[23] As with all of the previous examples, the perspective, artistic technique, editorial manipulation and selection closely match up with Wilkinson's textual descriptions, which present a gradient of increasing desolation, as he moves southward, to culminate in Montenegro.

Image omitted due to copyright restrictions.

Fig. 6.23: "Valley of Tzettine… Lake of Scutari", "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. e. 7, Folio 32.

In addition to the larger lithograph engravings, Wilkinson's chapter on Montenegro – despite occupying only half the space of the chapters on Dalmatia – features nearly as

[22] "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. c. 1, Folio 19.
[23] "Sketchbook, 1844", Bodleian Library Wilkinson manuscripts, dep. e. 7, Folio 32.

many smaller images. Altogether, the Montenegro chapter has about ten in all, the precise number depending on which territory the frontier drawings are placed. These feature a number of subjects: agriculture, local costume, court costume, dwellings and landscape views are the main ones. Compared to the illustrations of Dalmatia, landscape views are more prominent. The portrayals of dwellings also match up with Wilkinson's note in the text that there are no real urban areas and no impressive buildings: the five illustrations featuring buildings depict them as small inconsequential parts of the locality, or literally part of it, such as dwellings perched on or built into a rockface. Somewhat ominously, one shows a round tower, in ruins, with a series of protruding stakes, capped by decapitated heads. Turning to the accompanying text, the reader was treated to a first-hand account

> On a rock, immediately above the convent, is a round tower, pierced with embrasures, but without cannon; on which I counted the heads of twenty Turks, fixed upon stakes, round the parapet, the trophies of Montenegrin victory; and below, scattered upon the rock, were the fragments of other skulls, which had fallen to pieces by time; a strange spectacle in a Christian country, in Europe, and in the immediate vicinity of a convent and a bishop's palace (1848: 513-514).

To find out more about precisely who the Montenegrins were and why they followed such a barbaric practice, the reader had to turn to the accompanying text, which will now be discussed.

6.4 Montenegro: Folk, Deviant Classicism, and Medievalism

For readers interested in finding out about Montenegrin culture, Wilkinson's hundred-page chapter provided such an eclectic array of imagery that both Montenegro and the Montenegrins emerging from it defied easy compartmentalisation or comprehension. At one level, this was due to the unfamiliar subject itself. In reaction to the empire-building and cultural supremacism associated with the French Revolutionary Wars, Gottfried Herder had urged scholars to value provincial, peripheral and vernacular culture on its

own terms and as equally valid to that of the metropole, and Wilkinson's study of the Montenegrins – a people who most Britons would not have been remotely familiar with – was very much in this vein (Meek, 1976). Herder's mode of thinking was not entirely new, and it had a counterpart tradition in Britain: antiquaries such as John Aubrey and Robert Plot had collected details of local traditions in several parts of England, and they were not alone (Piggott, 1976; Piggott, 1989; Hunter, 1975). At a literary as opposed to more strictly historical level, James MacPherson derived considerable standing in British and European society from his recording and compiling (and probable authoring) of the ancient Gaelic bard Ossian, apparently collected from folk tradition in the Scottish Highlands, which he published in 1760. Five years later in 1765, Thomas Percy attempted something similar in England with his *Reliques of Ancient English Poetry*. Several decades later, Walter Scott took a similar approach in the Scottish Lowlands, in the process reinventing a lost world of border warfare, chivalry and Medieval romance (Piggott, 1976; Trevor-Roper, 1969; Hill, 2021). However, an additional new impetus in the first decades of the 19th century drew inspiration from the German lands: the Brothers Grimm had compiled many local traditions, and several Britons, including Thomas Crofton Croker, Thomas Keightley and William John Thoms did something similar, if on a less ambitious scale, for Britain (Dorson, 1968; Stocking, 1987). Wilkinson himself cited Herder in the preface of *Dalmatia and Montenegro*. However, despite apparently following Herder's lead, the image that Wilkinson painted of Montenegro was complex for an additional reason: Wilkinson did not constrain himself simply to documenting what he saw in Montenegro by itself. Instead, he drew on a range of imagery of other cultures: Homeric Greece, ancient Sparta and the Hebrews, as well as elements of Medieval Europe. These intersected in complex ways – notably when it came to the nature of primitive democracy and liberties – but in the discussion below an attempt will be made to trace how Wilkinson was conditioned by two broad cultural themes: associations with ancient Greece and the Medieval period.

The Greek imagery that Wilkinson drew upon was from two, occasionally overlapping ages: Homeric Greece – which was in itself a major part of the heritage of classical Greece – and classical Sparta. In 18th and 19t- century Britain – especially after George Grote published his *History of Greece* (1846)– there was considerable enthusiasm for classical

Athens, which, with its philosophical achievements, relative freedom, limited democracy and an economy partly based on trade, resembled Britain itself to many Victorians. With collections of classical art, and in civic architecture which drew on ancient Greek architecture, the reverse also became true to some extent, with some buildings in British cities resembling an idealised Greece. However, alongside this there was also a parallel enthusiasm for what are now seen as the rougher states of the ancient Greek world: the Homeric age as described by Homer, and ancient Sparta as described by Herodotus and Plutarch. Besides being leader of the Liberal Party and Prime Minister on four occasions, William Gladstone was a scholar of the Homeric age and published a three-volume *Studies on Homer and the Homeric Age* (1858). At least two other Prime Ministers were also keen students of Homer: Lord Derby translated the whole of the *Iliad*, while Lord John Russell translated a book of the *Odyssey* for recreation (Holland, 2018; Jenkyns, 1980).

Although the reputation of Sparta has been quite low since the late 19th century (partly due to libertarian/ right-wing mobilisation of supposed facets of Spartan culture), to the extent that it is sometimes cast as a brutal, brainwashed, war-bent society which helped bring down an apparently freedom-loving Athens, many Georgians and Victorians held Sparta in considerable regard on the grounds that its people were absolutely dedicated to one cause, freedom, which they were willing to defend at any price, most famously at Thermopylae (Kagan, 2005; Morris and Hodkinson, 2012). Although their means may have been misguided (and their lack of high culture unfortunate), the ends were similar to those of Athens and they were potentially even more dedicated to achieving them. Sparta – as depicted by Herodotus and Plutarch – was seen as the ultimate patriotic state, and the possessor of a constitution which mixed the best parts of monarchism and democracy, in contrast to the mob-rule which occasionally characterized Athenian democracy (Rebenich, 2002; Morris, 2004). Less gloriously, and certainly of more relevance to Wilkinson's depiction of Sparta, was that many of its cultural practices resembled those of the cultures deemed primitive by conjectural historians (Meek, 1976).

In the chapter on Montenegro, Wilkinson included a few references to the Homeric age, although more of them seem to relate to the Spartans. Within the first few pages, he

gave an overview of the country's settlement morphology: there were no large towns and there was little in the way of urban infrastructure. He then remarked that "none of them are walled" (1848: 406) and then immediately noted their unusual situation, in "hollows" (1848: 407) rather than on the more defensive hill tops. After this matter-of-fact section, he then included a provocative remark that "the Montenegrin… feels secure in the natural strength of his country, and requires no measures of defence beyond his own courage" (1848: 407). This is highly reminiscent of both the geographical situation of Sparta, located in the valley of the Eurotas, protected by mountains, in the remote Peloponnese, and of one of Plutarch's sayings of famous Spartans that the city had no need for walls due to the courage of its men (Plutarch, 2005; Fitzhardinge, 1980). Wilkinson's claim that the Montenegrins welcomed death in battle (1848: 433) is also highly reminiscent of the reputation of Sparta.

It would be wrong to imagine that Wilkinson made any sort of sustained comparison between ancient Sparta and 19th century Montenegro, but a reader with a passing familiarity with classical history will notice that many of Wilkinson's descriptions of aspects of Montenegrin daily life accord well with accounts of the city-state described in Plutarch, Herodotus, Thucydides and Xenophon. These cover the Spartans' infamous form of marriage: "If the girl objects to the suitor, he induces some of his friends to join him and carry her off" (423). It also covered their dreadful food "Their mode of living is very hardy and primitive, and their food simple" (446). Although in the latter case, Wilkinson suggested that the Montenegrins' poor diet was due to poverty rather than being forced to dine in communal messes like the Spartans; he noted they would have consumed more alcohol if they could have afforded it; it was not heavily restricted as it was in Sparta. Montenegrin cuisine was one theme in which the Homeric age was the obvious parallel. Wilkinson attended grand feasts of the sort that Homer had characterised the Greek warlords as having thrown open to maintain patron-client relations (Wood, 1998), and on one occasion he was invited into a Montenegrin household, where he was treated to mutton, sourced directly from a sheep roasted whole. The other main parallel with the Homeric age was economic: Wilkinson characterised the Montenegrin as living off plunder. Reviewers in *Blackwood's Magazine*

(Anon., 1849) and *The Edinburgh Review* picked up on these implied comparisons, with the latter making the connection with the "heroic ages of Greece" (Anon., 1859: 461).

As well as fairly short or only-slightly-expanded one-liners, which tended to cast the Montenegrins in a morally dubious light – similar in many ways to the portrayal of Arab sheiks in the Near East or the Kurds at the same time (Shepherd, 1987; Muhammad, 2017), Wilkinson made other observations that were more complex, with a particular political appeal. These followed a tradition that scholars interested in how Sparta has been perceived in the post-classical era have traced back several centuries before Wilkinson's time. Two facets will be explored briefly: patriotism and primitive democracy.

When it came to patriotism, Wilkinson repeatedly made his readers aware that the Montenegrins were a nation under threat: their country was sandwiched between the Habsburg and Ottoman Empires. Of the two, the threat came from the latter, and a historical section of Wilkinson's chapter on Montenegro consists almost entirely of a record of military history, with continual incursions of Turkish armies of as many as 180,000 men repeatedly being repulsed by far smaller bands of Montenegrins numbering in the thousands. In many ways, the whole narrative recalls the last stand of the Spartans at Thermopylae, during the Persian Emperor Xerxes' invasion of Greece: rocky Montenegro stood in for the narrow pass at Thermopylae, allowing the reputation of the former to be derived from the latter. King Leonidas, having been informed by an oracle that for Greece to be saved, one of Sparta's two kings would have to die, led three hundred Spartan warriors to fight to the death to resist the Persians for as long as possible. Wilkinson did not specifically draw any comparison with Leonidas, though Lady Emily Strangford, who was much influenced by Wilkinson, and referred to a similar catalogue of border wars in a visit two decades later, made a very explicit parallel. She referred to the Vladika, or ruler of Montenegro, as "a sort of successful Christian Leonidas" (1864: 189): in her view, although the Vladika had not been killed in battle, she believed that he and other Montenegrins were prepared to die to preserve their freedom if it was necessary.

Strangford's – and as will be seen Wilkinson's – admiration for the Montenegrins was not entirely an unmediated one drawn solely from classical sources. As Emma Clough has noted, the renown of Sparta in the 19th century was "somewhat disproportionate to its

original treatment" by contemporaries such as Herodotus (2004: 364), and in many ways reflected a rehabilitation of the Spartans which Richard Glover's had started in his 5,000-line epic poem, *Leonidas* (1737). Glover implied that if King George II, who was then somewhat unpopular, was virtuous like Leonidas, who had taken kingly duty to an extreme level, he would gain unwavering loyalty from his British subjects, and create a self-reinforcing cycle of patriotism. There were similar ideas in circulation elsewhere in Europe. Stefan Rebenich (2002) notes that "in Herder's view, Athens and Sparta represented the two eternal poles of human development, i.e. enlightenment and patriotism" (Rebenich, 2002: 325). Whether or not Wilkinson had been influenced on this particular point by Herder is unclear – it has been noted that Wilkinson cited him in the first chapter of *Dalmatia and Montenegro* in relation to Slavic nationalism – but it would be in keeping with Wilkinson's own nationalist attitude, shaped in part by perceived external enemies in a manner typical of his time (Colley, 1992).[24]

The second political aspect of Montenegro which resembled Spartan society – a form of primitive democracy – was also widely-discussed in the 18th and 19th centuries. Sparta had a mixed constitution, which included a dual monarchy (with a senior and a junior branch), a council and a popular assembly. However, given that most classical sources were written in Athens, at a time when the city was on bad terms or in open conflict with Sparta, the accuracy of much information is suspect, as well as conflicting. There was also comparatively little of it. Together, this has meant that subsequent generations have been able to cite the image of Sparta for a variety of political projects, including those with very little or no resemblance to ancient Sparta itself. As Ian Morris (2004) has pointed out, some, such as Gabriel Mably, a French philosopher whose influential work was translated into English, saw ancient Sparta as a model democracy, superior to Athens, on the grounds that its Gerousia (high council of elders consisting of old men) acted as a filter for the worst excesses of direct democracy which occasionally resembled mob-rule in Athens, especially during the Peloponnesian War.

[24] Wilkinson's anti-Catholicism is evident from a letter in which he expressed outrage at the appointment of Roman Catholic bishops in Britain, see "To The Editor of the Standard" (23.12.1850), "Miscellaneous loose papers, 1827-78", Bodleian Library Wilkinson Manuscripts, dep. c. 22.

Wilkinson's account of Montenegro's constitutional arrangements bears more than a passing resemblance to Mably's view of ancient Sparta, even if he never makes an explicit comparison between Montenegro and Sparta. As far as external appearances were concerned, Wilkinson maintained that Montenegro's government was a "republic" (453). However, internally, politics were "not democratic" (453), and the Vladika, who had formerly wielded only "spiritual power", was cementing his control over the secular sphere as well. Despite this, Wilkinson noted that Montenegrins had a living tradition of making decisions in popular assemblies and that "respect for the popular rights is duly maintained, and every village, or department, has the right of electing its own chiefs" (455). At the national level, the Senate acted as a check on the power of the Vladika. Essentially, executive and legislative dimensions were very similar to the Spartan system, with the king to some extent acting mainly in a military and figurehead role, although the role of the Vladika was changing. Wilkinson further tempered any concerns about the absolutism of the Vladika by noting that an open-air meeting area, where the Vladika was to some extent held to account, was "their Runimede" (456): a phrase which obviously links to a belief, widespread in the 19th century, that what were seen as typically English civil liberties were forged in the medieval period.

Before discussing the medieval imagery in greater detail, it is worthwhile to pause to consider why – if it is not already obvious – the abovementioned ideas about Sparta would have appealed to Wilkinson's predominantly upper-class audience. For most, neither the purported widespread violence nor the Spartan-style patriarchalism would have been particularly appealing – the reviewer of *Dalmatia and Montenegro* in *Blackwood's Magazine* picked up on Wilkinson's observation that women in Montenegro were forced by the men to do heavy work and came close to castigating him for not intervening (Anon., 1849). These features alone would have made the Montenegrins appear primitive and distant to a society which saw itself as law-abiding and progressive when it came to the rights of women. Instead, the Montenegrin (and Spartan) system of governance and its economic background were where the appeal lay. In 1848, Britain possessed a monarchy which had only relatively recently been rendered to most intents and purposes constitutionally redundant, and a limited suffrage in which 80% of men and all women (some wealthy widows had been able to vote before the 1832 Great Reform

Act which limited the franchise to men) were excluded from electing the less powerful of the two houses of Parliament; the more powerful being wholly composed of unelected peers. As such, the British and Spartan political systems bore some striking similarities.

Secondly, at an economic level, Sparta was an agrarian-based society, in opposition to Athens, which owed at least some of its wealth to trade. Whilst Morris and Hodkinson (2012) argue that the positive image of Sparta in the 18th century was starting to sour by the early 19th century – the Whig historian and politician Thomas Babington Macaulay saw Sparta as restrictive compared to libertarian and cosmopolitan Athens – this concern would have been less of an issue to many of Britain's upper classes, both Tory and Whig, who continued to derive much of their money from land, and occasionally viewed overseas trade with suspicion. Having been educated at private schools which ostensibly owed something to the Spartan system, their view of Sparta continued to be positive long after it had declined elsewhere. Wilkinson himself was in fact among their number: he had been educated at Harrow and, though he earned some money from his books, the steady income which allowed him to pursue his academic interests came from estate rentals from farms in northern England (Thompson, 1992a). Given these facets, when those readers who belonged to a similar socio-economic class as Wilkinson himself looked at the Montenegrins, they could see aspects of themselves, or, at the very least, aspects of their own recent history that was following a progressive trajectory. In other words, just as in his landmark *Manners and Customs*, where Wilkinson was able to decrease the distance between his audience and the subject under discussion, with only a few liberties taken in representation, he succeeded in bridging the temporal gap when it came to presenting the Montenegrins to British readers. In some ways, what was deviant classicism to one group was very much the (political) classicism of choice to another.[25]

[25] Wilkinson was not entirely opposed to trade. As was seen in relation to the section on his geomorphological investigation at the Colossi of Memnon at Thebes, he saw commerce as the highest stage of human development, as many Scottish and French philosophers who wrote conjectural history had argued. Nor was Wilkinson entirely against trade in *Dalmatia and Montenegro*: he noted that if Montenegro was given access to the port of Cattaro, the resulting commerce could help civilize the country. As Girouard (1981) notes, the British upper classes might have glorified income from land, but they did not altogether eschew other, potentially more profitable, income streams.

Alongside the deviant form of classicism just outlined – which allowed Wilkinson to portray the Montenegrins as enjoying a constitution bearing some similarity to that which Britain had followed in the recent past, and possessing a political economy which upper-class Britons had some sympathy towards – another historical period that Wilkinson drew upon was medieval Europe. Unlike the use of Spartan and Homeric imagery, the use of Medieval imagery in the Balkans has been discussed at some length, by Maria Todorova and Andrew Hammond. Todorova, who reads a "Balkanism" undercurrent – in many ways a local equivalent to Edward Said's (1978) *Orientalism* theory – in much western (and even Balkan-based) writing about the Balkans in the early 20th century notes that travel writers in the Balkans frequently described the region as trapped in another age – the medieval period. She cites two authors: Arthur Douglas Howden, an early 20th century author, noted "intrigue, plotting, mystery, high courage, and daring deeds" (Todorova, 1997: 14), while another visitor, Herbert Vivian, claimed he felt "like meeting the ghost of Sir Walter Scott" at every place he visited (Todorova, 1997: 14). In a shorter study, Hammond (2007a) identifies a negative "Gothic strain" in much western European discussion about the Balkans, which served as "a medium for hegemonic discourses of race and cultural supremacism" (119). Todorova's and Hammond's points are valid ones, and Hammond's point in particular will be discussed again at the end of this chapter. However, the suggestion by both scholars that Medievalism carried almost exclusively negative undercurrents of poor, rough and uncouth-looking men, who were contrasted to more civilized western European neighbours, underestimates the extent to which the Medieval period was revered by many Georgians and Victorians.

At the most visible, the late 18th and early 19th century enthusiasm for the Medieval period resulted in the Gothic Revival, which saw elements of medieval architecture being incorporated into many hundreds of new buildings, both sacred and secular, and the alteration of at least as many more (Curl, 1995). For some members of the elite, medieval architecture became important to their sense of identity, as a reassuring symbol of the continued dominance of their own sector of society, in the face of the danger of revolutionary change, partly due to fears of events in France being replicated in Britain (Girouard, 1981). Mark Girouard (1981) argues that the social influence of the medieval period went considerably broader than architectural style, acting as a powerful cultural

force which conditioned many British men into trying to uphold, or be seen to uphold, a set of chivalric practices supposedly followed by most medieval knights. These included bravery, loyalty, truthfulness and courtesy, as well as civility to women, respect for enemies and a willingness to defend the oppressed. To some extent, in the face of criticism from Radicals and Utilitarianism, elites may also have desired to present themselves in a positive moral and even pious light, and as protectors of the less well off – exemplified, although with little in the way of result, in the Medieval-inspired Young England movement, *Fraser's Magazine* and the Eglington Tournament (Girouard, 1981) – to justify their rule and win acceptance. Although these projects were mainly launched by and associated with Tories, Girouard (1981) notes that both sides of the political spectrum drew on medieval imagery, including some more ambitious social reformers, such as Lord Brougham, and Christian socialists such as Charles Kingsley. In other words, enthusiasm for the Medieval cut across political (if not necessarily class) groups, so when Wilkinson used it, he struck chords with many readers.

Wilkinson's description of some Montenegrin manners and customs – notably their treatment of women – was obviously a long distance from the chivalric ideal. However, at the same time, there were notable overlaps. In fact, Wilkinson was not the first to combine Homeric and Spartan Greece with the medieval: Kenelm Henry Digby, author of *The Broad Stone of Honour* (1822) – a text which was very influential in defining proper chivalric conduct in the early and mid-19th century, with four revised and expanded editions – presented the Homeric age as a great age of chivalry, second only to the Medieval. Digby had also held the Spartan education system in high regard (Girouard, 1981). One major area in which Wilkinson saw overlap between the Homeric/ Spartan and Medieval period as far as Montenegro was concerned has already been mentioned in passing in the discussion of Homeric and Spartan imagery: the focus on freedom, which had both national and local dimensions. To Wilkinson – and also to Strangford – Montenegro appeared as a lone Christian state that was in the process of taking a heroic stand against Muslim Turkey. The David-versus-Goliath theme was one that very much appealed to the reviewer of Wilkinson's article in *The Edinburgh Review*: the reviewer started their article by noting that Montenegro had risen "like Ararat, above the flood, of Mahommedanism which… threatened to overwhelm all the south and east of Europe"

(1859: 462), something which it continued to do against the odds. Wilkinson, with some prejudice toward the Ottoman Empire – discussed below – took a similar viewpoint, while Strangford's explicit combination of Spartan and Christian imagery has already been noted. At a more localized level, and as noted above, Wilkinson noted that open-air meetings of the Montenegrin ruler and his subjects resembled Runnymede, which was much-celebrated as the location where English liberties had been successfully defended against absolutism. In the context of a country which was undergoing a religious revival, well underway by the time of *The Edinburgh Review* article – and which prided itself on possessing civil liberties unrealized elsewhere, both aspects would automatically have a wide appeal across society.

Of possibly more specific relevance to the Medieval period, Wilkinson assured his readers that, in their struggle against the Ottoman Empire, the Montenegrins maintained an honourable code of practice toward their enemies. As he put it

> It is, nevertheless, a noble spirit… it is the feeling of a brave people, and not the result of blind prejudice, or vanity; for the Montenegrin never withholds from the Turk the merit of great valour, and, like a brave man, has the generosity to acknowledge a virtue in his enemy, which he thinks it his duty, and believes it his habit, to surpass (1848: 442).

Effectively, Wilkinson put the case for the Montenegrins following a chivalric approach toward their enemies, not dissimilar to medieval knights. Although the Montenegrin might not have shared the outward appearances of the Medieval knight, Wilkinson was confident that their conduct compensated for other deficiencies.

In addition to the average Montenegrin warrior, Wilkinson viewed the Vladika – the Prince-Bishop who ruled Montenegro – as the embodiment of the positive aspects of the Medieval image of the Balkans – chivalry in all of its forms, the close relationship between the state and Christianity, and a key driving force in forging the nation-state. His positive opinion stemmed from a meeting with the man, which Wilkinson recounted over four pages, although his discussion of the achievements of the Vladika extended over

several more. Anecdotally, Wilkinson learnt that the Vladika excelled in shooting exercises: to him, this explained support for him ("With a people like the Montenegrins, the merit of excelling in military exercises is a great recommendation in their chief" (472)). At a physical level, Wilkinson saw him as representative, or a better representative, of Montenegrins more broadly: he noted his considerable stature, of 6 feet 8 inches, was "not a little in his favour… [to] command the respect of a primitive and warlike race", while his overall appearance was "handsome, and well proportioned to his height" (472).

In a similar way to that in which he had approached ancient Egypt, Wilkinson meticulously recorded, in text and image, the elaborate costumes worn by the Vladika. He noted

> His general costume is military, like that of the country, though richer, and covered with a scarlet pelisse, bordered with fur. He wears the full short blue trousers of the Montenegrins, with white stockings, and black shoes; and two rather singular additions to his dress are a black silk cravat, and black kid gloves (473).

Alongside this dress more suitable for martial occasions, Wilkinson noted (and sketched) another portrait of the Vladika, in his Pontifical robes, with a "black round upright cap" like that worn by Greek priests, and a black veil over his shoulders (Figure 6.24). If the Vladika was impressive at a visual level, he also made an impression on Wilkinson at a social and intellectual one: he wrote poetry, belonged to learned societies of several European countries, and was a political realist who understood how to maintain good relations with other European countries. Two decades later, Emily Strangford described the Vladika's successor, Prince Nicholas, in similar language. To her, he was "extraordinarily handsome" (145), and she gave further praise for his skills as a musician and as a horseman, as well as for (reputedly) being a just law-giver, and (reputedly) merciful toward Muslims. Both images were of men following the chivalrous conduct which was highly prized in Victorian England.

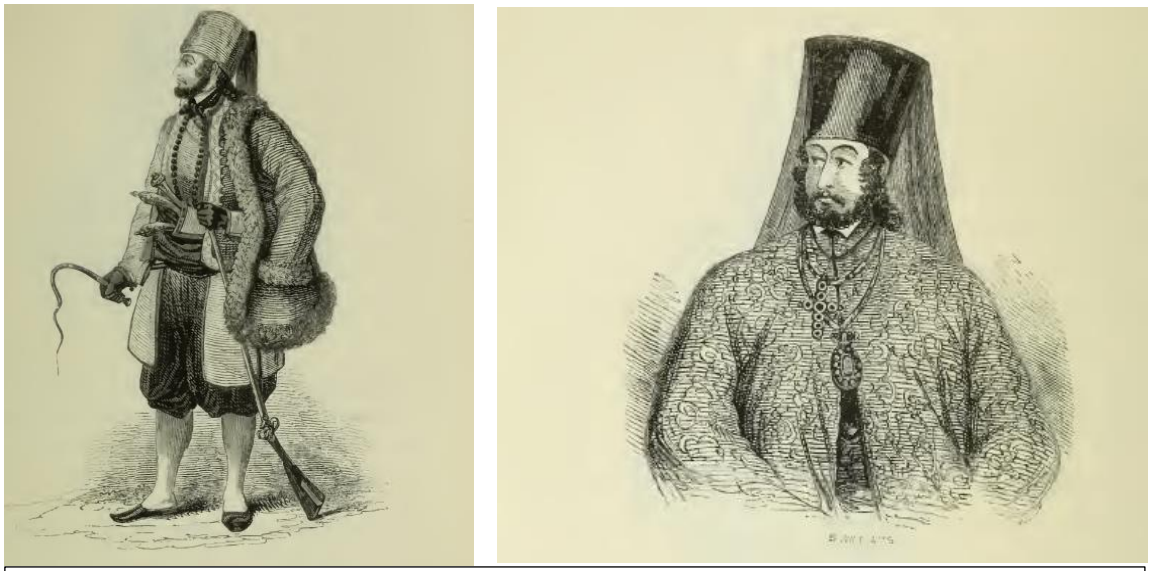


Fig. 6.24: Two images of the Vladika, from Wilkinson's *Dalmatia and Montenegro*, pp. 473-474.

Above all though, Wilkinson saw the Vladika in a good light due to his attempts to civilize his country. Earlier in his narrative, he had noted the establishment of new schools, but in a lengthy passage positioned immediately before his description of the Vladika – but possibly at least in part resulting from the meeting – he noted how the head of state's reforms were making Montenegro a more civilized country: robbery was less common than formerly (or at least it was directed outward against Turkish neighbours than toward fellow Montenegrins). Through the introduction of a new law code, the old problematic tradition of blood feuds and cycles of vengeance was gradually being laid to rest. Overall, if in his personal characteristics the Vladika comes across as a model (or honorary) Englishman, a recurrent theme when upper-class Britons met local notables in the colonies (Cannadine, 2001), this was also true of his conduct in trying to advance the condition of his country more broadly: in addition to the medieval, there are shades of the Tudor and Stuart periods (the "olden time") of English history which became renowned as a formative stage of the English nation-state (Mandler, 1997).

If Wilkinson was very impressed by some aspects of Montenegro's apparently Medieval culture, there were other aspects with which he was notably less impressed. One of these was religion: he saw the Montenegrins, who followed the Eastern Orthodox religion, as

superstitious, after observing many of them repeatedly make the sign of the cross. In the 18th century, fears surrounding Catholicism, the other main form of non-Protestant Christianity that travellers encountered – resulted in suspicion toward those who wanted to rehabilitate the medieval, although by the mid-19th century, at the time Wilkinson published *Dalmatia and Montenegro*, these had dissipated somewhat (Sweet, 2004). Of considerably more concern to him was the Montenegrin practice of decapitating enemies, invariably Turkish, which Wilkinson depicted in an image, as mentioned above. In addition to the published image, Wilkinson sketched a considerably more graphic close-up of one of the heads, at the back of a compilation of Illyrian words and phrases with their English equivalents.

Wilkinson's reason for sketching the decapitated head cannot be known for sure, but he did not treat it as another part of the scenery. In his biography of Wilkinson, Jason Thompson notes that Wilkinson made multiple attempts to lobby both the Montenegrin and Turkish (in practice the local Vizier of Mostar/ Bosnia (1848: 80)) governments, in an attempt to end the practice. As also noted by Thompson, Wilkinson's approach was somewhat naïve – having been won over to the Montenegrin side after his meeting with the Vladika, he was distrustful of the Turkish side and tended to view Muslims in a monolithic light in a manner reminiscent of Edward Said's *Orientalism* thesis.

This is not to say that Wilkinson was an unconditional supporter of the Montenegrin side: the limits to his support, and his romantic medieval vision, are shown by a quite patronising remark, made about a group of Montenegrin men preparing to set out on a raiding party across the Montenegrin-Turkish border, despite a truce

> They seemed to treat the matter, like boys, in "the good old times," who robbed orchards; the courage it showed being in proportion to the risk; and scruples of conscience were laughed at, as a want of spirit (1848: 521).

If Wilkinson had previously presented Montenegrins as fearless warriors, his description of actual Montenegrins was infantilising, a frequent trope in British travel writing on Africa and Asia. When Wilkinson crossed the border into Bosnia and met with the local

governor, his view and language were similar: he thought that the onus was on the Ottoman Empire – the larger and more sophisticated state – to bring an end to the practice. Although his views of the Ottomans continued to be negative, there is a hint that they should have taken a paternalistic approach toward the Montenegrins. On seeing some newly decapitated heads on poles displayed on a tower on the Ottoman side, and a story to go with it, Wilkinson believed he saw the features of the Montenegrin man he had condescendingly observed several days before: his simple comments included no sign of sympathy toward a fallen Spartan or a Medieval knight. Concern over the man's treachery over a truce might have played a part in shaping his reaction, but there were limits as to how far the reality could acceptably diverge from the sanitised ideal of the medieval period.

In addition to the Homeric age, ancient Sparta and Medieval Europe, Wilkinson's readers found Montenegro reminiscent of other cultures: the reviewer who discussed the Montenegro chapter of Wilkinson's book in *The Edinburgh Review* (1859) noted similarities with the Scottish and the Hebrews. They also built on some of Wilkinson's own observations and suggested that Montenegrin warriors were followed by bards who recorded their deeds in poetry (Wilkinson in fact had noted that Montenegro possessed no bards like Homer and that the Montenegrin warriors were sufficiently proud of their own present-day battles that they had no need for epic poetry). If one culture was missing from Wilkinson's chapter on Montenegro, it was ironically the Roman period to which Wilkinson had devoted so much attention to while travelling through the rest of Dalmatia. Whereas Wilkinson had formerly been keen to record every trace of an ancient Roman presence, lest it signify a historic site, he made only one attempt to do so in Montenegro, despite it having once been part of the ancient Roman province of Illyricum. Even in this case, the attempt turned out to be a spurious one: Wilkinson was unable to find any trace of a reported Roman road (1848: 524). To Wilkinson and any classically-minded readers, if Montenegro was a peripheral place remote from any real civilization in the present day, there was also a feeling that things had been little different in the remote past.

6.5 Conclusion

Despite the relatively short distances involved, Wilkinson's journey through the Balkans, recounted in *Dalmatia and Montenegro*, had taken him to two very different territories, and in many senses his book was an exercise in establishing a series of dichotomies between the two. The first, Dalmatia, formed a backwater of the Habsburg Empire, but was renowned for its classical past, as it had once been at the forefront of the Roman Empire, and material traces of this era were still in abundance, notably at Spalato, but also at an array of smaller sites such as Salona and Burnum. To the visitor, travelling by Lloyd's steamers between coastal towns, the region offered interest for antiquarian-minded travellers and those seeking the picturesque: in some ways, it acted as an extension of northern Italy.

By contrast, Montenegro was an independent state, which Wilkinson viewed as engaged in a struggle for survival against the Ottoman Empire. Virtually devoid of features of architectural interest and lacking any of the classical-era sites that Wilkinson had devoted so much attention to in Dalmatia, it made up for these with impressively sublime scenery and the survival of old customs – some of them too old and barbaric for the discerning 19th-century British traveller. The result was that, in many ways, when Wilkinson crossed the frontier into Montenegro, he travelled not only through space, but time as well: the territory was a pre-industrial spectacle. Rather than appreciating it for these qualities alone, as Herder urged, and as Wilkinson had done when he visited the German lands, Wilkinson turned to the medieval and even ancient Greek periods to make sense of the region, and ended up building up a highly romanticised picture of the territory.

Although Wilkinson's research in the Balkans was quite different from his work in Egypt, commonalities nevertheless existed between the two: most notably, Wilkinson's attempts to triangulate between classical sources, material culture and the extant landscape – what has been repeatedly referred to as his antiquarian methodology – were carried over. In some cases, the source material was the same: Wilkinson encountered relief scenes at Diocletian's Palace, and used textual authorities to comprehend them,

just as he had done at the Ramesseum and Medinet Habu. In the background, on the basis of linguistic and cultural similarities first recognised by William Jones, Wilkinson also assumed that the people of the Balkans and the Egyptians – and the rest of humanity – had a common origin (1848: 444-445): this was one of the few genealogical links he attempted for the Balkans, in which many of his classical allusions, especially in Montenegro, became allegorical. As such, if his interest in the Balkans did not grow out of his Egyptian research, then it incorporated similar methodological techniques and a similar outlook.

Conclusion

In histories and commentaries concerning the history of the study of ancient Egypt, it is not uncommon to find romantic, nostalgic and occasionally quasi-orientalist remarks about how, as Egyptology emerged as a consolidated discipline, a previous golden age was lost. According to this construct, before the arrival of the Napoleonic information-gathering expedition in 1798, Egypt was seen as almost totally unknown, and for at least the ensuing half-century, the field was sufficiently open for (wealthy) amateurs to contribute something, before they were squeezed out by professionals who were intent on "dulling down" the field, focusing on less interesting issues and becoming increasingly specialised, over the course of the late 19th and 20th centuries (Baines and Malek, 1980**;** Gange, 2013; Naunton, 2020; Wilkinson, 2020). Perhaps even more frequently, especially in more recent works, little sympathy has been given to Britons such as Wilkinson and other Europeans active in Egypt in the early 19th century, who have frequently been cast as little more than collectors or looters, who spent their spare time engaging in cultural appropriation and worse. In these narratives, there is little to lament about the transition to scientific archaeology – which in more recent literature has itself been critiqued – that occurred in the later years of the 19th century.

This study has attempted to show, through focusing on Wilkinson (a relatively representative individual) as well as occasional reference to his colleagues, that whilst such narratives might be partially correct, their estimation of precisely what was at least temporarily lost, or altogether absent as the 19th century progressed, is highly conservative: what is at stake is the recognition of a previous tradition of scholarly research into the civilization of ancient Egypt. The early 19th century was a time in which serious scholarly research was undertaken, often by self-trained individuals who worked outside museums and universities. Unconstrained by modern disciplinary boundaries, they frequently considered very diverse forms of evidence, which they approached through a range of methodologies, in a manner typical of what has been termed a "predisciplinary" era (although not all succeeded in publishing their work, many nevertheless made a contribution of some sort).

By contrast, the late 19th century saw "Egyptology" become centred around the study of hieroglyphs, while the archaeology of the period staked its disciplinary identity around

digging, with stratified excavation increasingly held as almost the solely valid methodological approach. As Kenneth Hudson (1981) and Philippa Levine (1986) have noted, this process of standardisation was by definition also one of narrowing. As has been seen, when Egyptology (broadly defined) extended into these areas in the late 20th century, especially as far as geomorphological research was concerned, few references were made to 19th century investigators – despite their asking an array of very similar research questions. In their studies of antiquarianism, both Sweet (2004) and Hill (2021) have pointed out that this is very much a feature of historical research. As Hill has noted, "later generations of professional historians have not generally cared to acknowledge their debt to antiquarianism" (2021: 8). She goes on to explain that because "the antiquaries' efforts have been ignored or undervalued for so long they have fallen out of sight of an age which has largely recovered their broader view of history": she corrects Margaret Macmillan's suggestion that the study of everyday life is a new development by noting that the topic was pioneered by 19th-century antiquaries (2021: 8). Parallels are readily available in archaeology: Schnapp celebrates the attempts of archaeologists to move beyond bounded sites and consider the landscape setting, and praises the 1859 moments in prehistory and evolutionary science as the point at which the humanities and natural sciences became reconciled to create modern archaeology – yet such research was routinely the case in the predisciplinary era of the preceding decades and became less, not more frequent, afterwards.

Although Gange (2013) and Carruthers (2015) have begun to consider this dimension, neither give extensive consideration to the role of antiquaries: Carruthers mainly focuses on the late 19th and 20th centuries, whilst Gange puts the cultural context, notably the influence of Christianity, at the centre, and in doing so largely follows the existing tradition that has been highly critical of the antiquaries. Whilst Elliott Colla (2007) has shone light on how Egyptology sometimes alienated indigenous Egyptians from their heritage, such postcolonial lines of enquiry have, in many ways understandably, not considered how archaeologists attempted to distance themselves from their own scholarly predecessors, in a way similar to the "strategic distancing" described by Spriggs (2020) for modern Australian archaeology. Some portrayals of British scholars active at this time, notably of Wilkinson, such as that described by Maya Jasanoff (2006), partially

copied by Gange (2013), have regrettably led to a partial resuscitation of the negative image of antiquarianism – as largely or solely concerned with collecting – created by late 19th and 20th-century archaeologists, which is at odds with scholarship by Jason Thompson (1992a; 2010; 2015) and others who have given detailed attention to contemporary figures (e.g. Tillett, 1984 for Robert Hay).

As has been discussed in the preceding chapters, there can be little doubt that the topics Wilkinson and his contemporaries investigated were exceptionally wide-ranging. In part, this is an observation that has already been made by Thompson (1992a), with the result that nearly all subsequent histories of Egyptology have devoted space to his main contributions to the discipline: his numbering system for the Valley of the Kings, his copying work at Thebes and above for his visual culture-centric *Manners and Customs of the Ancient Egyptians* (1837).

However, in addition to these concerns, this study has sought to highlight a number of other themes which are highly disruptive to existing narratives of early 19th-century scholarly activity in Egypt. In particular, Wilkinson's approach demonstrates a high level of spatial awareness, which fitted antiquities and monuments into their wider landscape context, an approach which has been regarded as synonymous only with later periods of investigation.

In Upper Egypt, Wilkinson's starting point was to attempt to match stations and settlements mentioned in the Antonine Itinerary with ruins scattered along the banks of the Nile in Nubia: an enterprise which rested on measuring the distances between mounds of brick and rubble which were otherwise lacking in any obvious interest. When it came to more aesthetically-impressive remains, such as commemorative stelae and temples, Wilkinson was again as interested in their precise geographical locality as he was in the structures themselves, as he saw them as markers that could be used to indicate the former extent of the ancient Egyptian empire. The most obvious markers that could be used for ascertaining this research question were naturally the colossal fortifications that he became aware of two decades after his initial work, although even with these, Wilkinson was keen to note where the entrances and best-fortified walls lay, which he believed could shed light on their original purpose. Although Wilkinson was ultimately denied the opportunity to carry out similar research in Sinai and the Levant – a task which

as has been noted would likely have been difficult due to the distances involved – his attempts to source information about the location of ancient Egyptian campaigns from temple relief scenes, which he cross-referenced with his knowledge of topography in the Levant and beyond – was an ingenious substitute, at least at a methodological level.

Wilkinson's geomorphological research also had a strong spatial dimension: his observations of the nature of rocks at Alexandria and his carefully-measured transect at Thebes are both clear examples of this, while he attempted to draw conclusions about the ancient size of the Delta and the relative size of the Nile Valley and surrounding desert – although his ultimate goal was to use his research to answer historical questions, rather than reconstruct ancient landscapes, impressive as this dimension was. Wilkinson's entire investigation was also predicated on his belief that the two Colossi of Memnon had stood in their original position since antiquity, in a similar manner to which he assumed stelae and temples had.

Wilkinson was not interested simply in Egypt and Egyptian history and had much more wide-ranging interests. The discussion of his work on the Balkans demonstrates that the antiquarian methodology that he pursued was not specific to place but was more broadly applied. Wilkinson's research in the Balkans both followed his approach in Egypt and differed radically from it. His work on topography closely paralleled that which he had done for Upper Egypt: although he does not appear to have used the Antonine Itinerary to any great extent, he attempted to identify – drawing on the work of other scholars just as he had in Egypt – the location of historically-important localities and settlements by using the testimony of classical authors. When it came to siting monuments in their wider environmental setting, Wilkinson's interest in environmental change was much more muted than for his Egyptian work, although it was present at key localities, notably Salona. Where Wilkinson departed from his earlier approach was in Montenegro: in this territory, his vision can only really be adequately understood in relation to romantic antiquarianism. Whilst this naturally appears quite different from its more academic counterpart, Sweet (2004; 2012), Heringman (2013) and Hill (2021) have cautioned against about drawing a firm boundary between academic and popular antiquarianism, since antiquarian research into the medieval period generated an extensive popular following from the 18th century and went on to become a truly potent cultural force in

the early 19th century. Whether or not this was for the better has been doubted by some (Trevor-Roper, 1969; Heringman, 2013), but when it came to Montenegro, Wilkinson succeeded in discovering – or to some extent even inventing – a space in which the medieval period was present in living form, as opposed to being simply an architectural style. As his reviewers suggest, the overall sense of uniqueness was a convincing one.

In all, Wilkinson emerges as a figure who had a close engagement with the landscape of whichever region that he undertook work in, and one who was ready to appreciate it at a number of different levels.

A second disruptive theme that emerges from Wilkinson's research is the range of material culture that he was prepared to accept as evidence. Wilkinson's use of paintings from tombs in the Valley of the Kings and the Tombs of the Nobles are well-known as he used them as a major source for *Manners and Customs of the Ancient Egyptians* (1837). His use of relief scenes is less well-known, though as they are predominantly visual, they are a similar type of source. However, that Wilkinson extended his interests to considering piles of bricks and rubble, used to establish sites of the Antonine Itinerary in Nubia, and to some extent in Dalmatia as well, are of a different nature, as are his objections to Horner's conclusions based on these artefacts. As such, the full range of material that Wilkinson engaged with challenges the widely-held view that gives the credit for such an approach to Flinders Petrie: a theme that is widely-repeated throughout narratives on archaeology. Similarly, the image of antiquaries carelessly destroying material, in contrast to a meticulous Petrie, is also a false one: whilst Petrie noted the importance of paying attention to the smallest details in his widely-read biography, *Seventy Years in Archaeology* (1931), his earlier work gives a different picture: in *Methods and Aims in Archaeology* (1904), a practical guide to what he enthusiastically propounded as modern excavation technique, Petrie noted that he constantly selected the best finds and threw others away. As Evans (2007) has suggested, although archaeologists may have emphasised the importance of material culture and observation, much was "rhetorical" or even simply "aspirational": there is a real need to avoid antiquarian practice with archaeological practice as opposed to archaeological ideals.

If antiquaries and archaeologists sometimes approached material culture in a similar way, then it is nevertheless important to reemphasise areas in which they diverged. One

dimension which set antiquaries apart from their archaeologist counterparts was in their relative lack of enthusiasm for excavation. Whilst Wilkinson occasionally cleared monuments in order to sketch them, and his investigation programme in the vicinity of the Colossi of Memnon technically involved digging pits to measure the depth of accumulated sediment, it is striking just how little conducting excavations to look for new objects or objects in context featured on his agenda. As has been noted at length, his work depended much more on placing antiquities and monuments (or relief scenes depicted on them) that were on the surface and easily visible, within their geographical context; he then made deductions from these about wider questions relating to Egyptian history, society and so forth or, when he was in the Balkans, about where the historically-important localities of Roman Dalmatia could be found in the modern-day landscape. As such, Wilkinson's approach was in some ways more ambitious – and potentially more reckless – than more strictly archaeological approaches, which emphasise a primary need of reconstructing the histories of individual sites (Koelsch, 2013). Though it would be an exaggeration to deny that Wilkinson had no interest in concerns such as finding out whether original plans were modified during the construction of ancient buildings, putting such concerns at the centre of his enquiry would have been an unfamiliar concept to him. Even in his *Topography of Thebes, and General View of Egypt* (1835), his concern was less about such issues than about extrapolating from them to draw conclusions about wider historical and cultural issues.

Another of Wilkinson's major concerns, which set him apart from his archaeological counterparts of succeeding years was his endeavour to give equal weight to the authority of classical-era texts and material culture, the latter consisting of tomb paintings, relief scenes, artefacts or large-scale monuments. Although he considered different balances for individual cases in which material evidence was clearly at odds with texts, he did not assign either a value of one over the other, an approach which became rarer in later years, partly due to academic specialisation and the consolidation of sub-disciplines. This is not to deny that archaeologists such as Flinders Petrie and Auguste Mariette made use of classical sources (alongside hieroglyphs) to try and locate sites: Mariette supposedly did so to great success when locating the Serapeum at Saqqara (Vercoutter, 1992; Malek, 1983). Rather, it is to say that Wilkinson based many of his research questions on

information supplied by classical authors – notably Diodorus Siculus and Herodotus for the extent of ancient Egypt into Africa and Asia, Homer for the extent of the Nile Delta, and Herodotus for the extent of the Nile valley at Thebes – and, up to a point, as the investigation advanced. If his researches into such issues in the Balkans were on a much more limited scale – in part this reflects Wilkinson having made only an extended visit to the region as opposed to a decade-long sojourn with subsequent repeat visits – then they were present there too in his topographic research and his deference to text-based interpretations of relief scenes. Wilkinson's description of Montenegro might even be regarded as the culmination of his desire to use textual sources: faced with an apparent absence of accessible indigenous ones which he could understand, he was prepared to make use of others from elsewhere, from societies which he appears to have viewed as similar.

Viewing Wilkinson's work from a distance of up to two centuries later, with a substantial expansion of research carried out as a result of the expansion of the museum and subsequently the university sectors, it is inevitable that many of Wilkinson's conclusions no longer match up with those drawn by modern scholars. Wilkinson's investigation of the extent of the ancient Egyptian empire into Nubia depended on an assumption that has subsequently been derided as assuming "pots are people". Partly in reaction to the political uses for which "settlement archaeology" was mobilised in Germany in the 1930s and 1940s (Arnold, 1990) and partly due to the observation that substantial changes in the material culture of the present have resulted from autochthonous change within societies, post war archaeology, especially since Grahame Clark's (1966) seminal paper, *The Invasion Hypothesis in British Archaeology*, has been extremely reluctant to accept arguments which posit a direct relationship between artefact distribution and peoples (Bahn, 1996; Anthony, 2010; Christian, 2011; Arnold, 1990).[26] Other arguments forwarded by Wilkinson would also be viewed as problematic today: his ideas about what might be called "invisible temples", although in part backed by new evidence, would now be regarded as highly speculative. Likewise, assumptions about treating temple relief scenes as straightforward historical representations have

[26] The use of radiocarbon dating, which revealed many prehistoric sites in western Europe were older than their supposed Mediterranean counterparts, was the driving reason for the change (Cunliffe, 2007).

been referred to as naïve in one recent study (Shaw, 2019). As has been noted, Wilkinson's extrapolation from very limited fieldwork conducted at Alexandria and Thebes to form ideas about the whole of Egyptian history would now be viewed as utterly unwarranted, especially as modern studies focus on reconstructing landscapes to the detail of waterways.

Inevitably, Wilkinson's usage of texts in such a way affected his overall way of seeing: it led to him imposing western conceptions of the world – and corresponding approaches in interpreting the past – onto his subject matter. To point out this issue is important, not least because it could scarcely have been otherwise: as has been noted, narratives that posit Europeans as having once seen Egypt as entirely unfamiliar, or assumptions that scholarship into ancient Egyptian civilization took place in a vacuum unconnected to other modes of scholarly inquiry and cultural norms, are erroneous. Instead, Wilkinson's work in Egypt – and in the Balkans – was conditioned, directly or indirectly, by just these factors. As such, Wilkinson's research into topography and stylistics might have anticipated Petrie by decades, but at the same time, neither of these pursuits were entirely new innovations: instead they built on over a century of antiquarian research into the topography of Roman Britain and the minutiae of Medieval architecture, which had formerly appeared just as undifferentiated to scholars. Likewise, Wilkinson's reading of relief scenes on Theban temples (and latterly Diocletian's Palace) reflected an optimistic belief that ancient images – whether the Parthenon frieze, the Bayeux Tapestry, Trajan's Column or others – could be read unproblematically as narratives, historical or otherwise. Whether these instances should best be interpreted as a constant current of western impositions or an extreme willingness to avoid othering other cultures must remain largely a matter of opinion.[27]

Where Wilkinson's imposition of a distinct worldview was more problematic was in his ideas about development. Just as these contain more than a tinge of later ideas about evolutionary development (see Stocking, 1987), they were in fact typical of writing by his scholarly contemporaries who followed theories about conjectural or philosophical history, as it had been propounded by predominantly Scottish and French scholars of the

[27] Gonzalez-Ruibal (2017) gives thoughtful consideration to this issue.

late 18th century. Although they were by no means a dominant theme in his thinking – as is shown by his willingness to quietly jettison them when other forms of evidence came to light, most notably in his discussion of the identity of the Shepherd kings (Hyksos) – such ideas about development nevertheless cut across Wilkinson's work. In Egypt, they underpinned his ideas about how it was possible for the ancient Egyptians to have built an empire, while his conclusions drawn from his work in relation to the Colossi of Memnon at Thebes also made reference to them. If the theme is latent in his work in Egypt, then it is manifest (to use terms borrowed from Edward Said (1978)) in his work on Dalmatia and, above all, Montenegro. In his travels south through the Balkans, Wilkinson gave the impression of travelling back in time to an earlier phase of human development. If he saw Dalmatia as a territory which possessed impressive classical remains and which retained traditions despite being part of the modern world, then he saw Montenegro as a place where the old ways still lingered, some of them too much for anyone's liking. The dominant themes here were the medieval period and older, apparently more primitive societies of the classical age: Sparta and Homer featured rather than Athens and Rome. Such a vision might have provided entertainment to Wilkinson's readers, but whether it was a fair representation of the average Montenegrin is more questionable.

Overall, though, the extent to which some ideas forwarded by Wilkinson (and his contemporaries) for ancient Egypt in particular seem to transcend his time is also worthy of remark. Wilkinson's narrative of early Egyptian history bears more than a passing resemblance to modern ideas about the complex interactions between a dynamic environmental backdrop and subsistence strategies as outlined by Judith Bunbury (2019) and Pennington *et al.* (2021). For later periods, even if Wilkinson had a tendency to see the Shepherd kings as ever-present, his belief that the Egyptian revolt against them acted as the catalyst for further wars of conquest – albeit not outright annexation – remains seriously contemplated (Newby, 1980; Mieroop, 2011). Of more importance, Wilkinson's methodologies remain recognisable: investigation of environmental change is once again a tool for investigating the past, while stylistics remains important to dating: contrary to widespread belief, radiocarbon dating is not always possible, and use of pottery styles remains important for establishing narrow date ranges.

None of these similarities to modern scholarly approaches must necessarily position Wilkinson as ahead of his time: such a conclusion would do an injustice to his contemporaries, many of whom supplied him with information, who have featured relatively fleetingly in this account and about whom more scholarly research is needed, and is indeed being done. Rather, it is to emphasise once again that the scholarly methods used by previous generations of antiquaries were both coherent and allowed them to draw coherent conclusions, and that narratives of linear disciplinary, or even just scholarly, progress can be highly misleading if they neglect such traditions of older research.

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