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Measuring Time
Histories of chronology building in archaeology

PhD Program in Analysis and Management of Cultural Heritage

XXX Cycle

By

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2016	Arte'm (Napoli) – Exhibition Project Manager and Scientific Secretary. National Archaeological Museum of Paestum – Possessione: trafugamenti e furti a Paestum
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2017-present	Spazio Geco (Pavia) – Accounting Manager (until 2019), Project Manager and Content Manager:

@ Museo delle Grigne di Esino Lario
- 4 video-guides with gamification and storytelling

@ Museo Archeologico di Cremona
- interactive *forma urbis*

@ Museo Etnografico di Tirano
- interactive map of the territory

@ Musei Civici di Pavia
- multimedia installations in the Sala del Collezionista

@ Museo Valtellinese di Storia e Arte - Archaeology
- immersive installation (audio, lights, video)
- smart audioguide

@ Parco Archeologico di Paestum
- temporary exhibition "Poseidonia città d'acqua"

@ Musei Civici di Cuneo
- multimedia boardgame (with V. Maggi)

@ Comune di Montefranco
- didactic texts
- reconstructive illustrations of the 'Umbrian sanctuary' of Monte Moro

@ Museo Archeologico di Piadena (in progress)
- interactive installation (cremation tomb)

@ Divina Sorrento SRLS (in progress)
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Publications

1. M.E. Oddo, "Appunti per un'analisi dello schema di Endimione in ambito greco-ellenistico" in *Engramma* 122, Dicember 2014.
2. M.E. Oddo, "Measuring Time. Historical Perspectives on the Birth of Radiocarbon Dating" in *Metrology for Archaeology. Proceedings of the First International Conference,* 88-94. Benevento, 2015.
3. M.E. Oddo, "Un'accurata combinazione di autentici e falsi. Lekythoi a fondo bianco e vasi italioti a figure rosse" in G. Zuchtriegel (ed.) *Possessione. Trafugamenti e falsi di antichità a Paestum* (Catalogo della mostra, Paestum 2 luglio - 31 dicembre 2016). Napoli: Arte'm, 2016.
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5. M.E. Oddo, "La Tomba del Tuffatore. Una bibliografia ragionata" in G. Zuchtriegel (ed.) *L'immagine invisibile. La Tomba del Tuffatore nel cinquantesimo dalla scoperta* (Catalogo della mostra, Paestum 2 giugno – 7 ottobre 2018), 137-145. Napoli: Arte'm, 2018.
6. M.E. Oddo, "Tre vasi apuli con l'uccisione di Reso. Una questione di metodo" in in M. Cipriani, A. Pontrandolfo and M. Scafuro (eds.) *Dialoghi di Archeologia Mediterranea II,* 395-406. Paestum: Pandemos, 2018.
7. M.E. Oddo, P. Ricci, D. Angelici, F. Fantino, E. Sibilia, M.F. Alberghina, S. Schiavone, C. Grifa, M. Mercurio, C. Germinario, F. Izzo, A. Langella, E. Massa, S. Bracci, D. Magrini, R. Costa, A. Pelagotti, G. Zuchtriegel, C. Lubritto, "Results of diagnostic campaign promoted by AIAr in the deposits of the Archaeological Museum of Paestum" in *HeriTech Conference* 2018.
8. G. Zuchtriegel, P. Carter, M.E. Oddo (edd.) *Poseidonia città d'acqua. Archeologia e cambiamenti climatici* (Catalogo della mostra, Paestum

04 ottobre – 3 maggio 2020). Paestum: Pandemos 2019.

9. M.E. Oddo (in press), "Perspectives in the first person. A questionnaire on museum semiotics" in *Archeostorie Journal of Public Archaeology* 3.

10. M. E. Oddo, Th. Devièse, D. Comeskey, D. Querci, S. Brown, K. Douka, T. Higham (in press), Grotte de la Verpillière, Germolles - ORAU Radiocarbon Dates. Laboratory Report. In Harald Floss (Ed.), *Au carrefour des territoires – Derniers Neanderthals et premiers Hommes modernes en Bourgogne méridionale, France. Résultats du PCR « Le Paléolithique supérieur ancien en Bourgogne méridionale»*, Éditions Mergoil, 2020.

11. R. Hopkins, M. E. Oddo (submitted), *Working with 'old' radiocarbon dates. Guidelines for archaeological applications*. Journal of Archaeological Research and Methods.

12. M.E. Oddo (submitted), "La Tomba del Tuffatore. Cinquant'anni di studi" in *Atti del Seminario sull'Iconografia e il Rituale funerario, Collana del Dipartimento di Storia Culture Civiltà dell'Università di Bologna.*

13. Alberghina, Germinario, Bartolozzi, Bracci, Grifa, Izzo, La Russa, Magrini, Massa, Mercurio, Mollica Nardo, Oddo, Pagnotta, Pelagotti, Ponterio, Ricci, Rovella, Ruffolo, Schiavone, Spagnuolo, Vetromile, Zuchtriegel and Lubritto (submitted) "Non-invasive characterization of the pigment's palette used on the painted tomb slabs at Paestum archaeological site" in *HeriTech 2020*

Presentations

Here I only list talks and presentations that did not become publications. I do not include teaching assignments, such as classes and lectures for university courses.

1. M.E. Oddo, "The Killing of Rhesus on Three Apulian Vase-paintings: iconography and iconology" at First Interdisciplinary Symposium on the Hellenic Heritage of Southern Italy. Arcadia University, Syracuse (Itay) 21 -23 May 2015.

2. M.E. Oddo, "The Radiocarbon Revolution in Archaeology: a Sociological Investigation" at AIAr IX National Congress. University of Calabria, Arcavacata di Rende (CS, Italy), 9-11 March 2016.

3. M.E. Oddo, "The Stubborn Illusion of Time" at IMT Lucca, Lucca (Italy) 25 May 2016.

4. M.E. Oddo, "Comunicare la complessità del tempo: potenzialità dei parchi archeologici" at MusumDià. Chronos, Kairos e Aion: il tempo dei musei. RomArché VII Salone dell'editoria archeologica, Roma 26-28 May 2016.

5. M.E. Oddo, "Just on Time. Some insights for a Historical Analysis of Time and Chronology in Archaeology" at Research Laboratory for Archaeology and the History of Art, Oxford University, Oxford (UK) 23 May 2017.

6. M.E. Oddo, "How many hands has a clock? Integrating chronological records: a semiotic approach" at Theoretical Archaeology Group Conference, University of Cardiff, Cardiff (UK) 18-20 December 2017.

7. M.E. Oddo, "*Apologhìa* for chronology. An appraisal of chronology as a multi-layered problem" at Theoretical Archaeology Group Conference, University of Cardiff, Cardiff (UK) 18-20 December 2017.

8. M.E. Oddo, "Giovanni Patroni e il 'mito funebre' come prodotto dello 'spirito italiano'. Nazionalismi e localismi al crocevia tra pittura vascolare lucana e urne etrusche" at Giornata in onore di Giovannangelo

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Abstract

Histories of archaeology are usually either cultural histories (i.e. histories of archaeological thought) or histories of progress describing the advancement of the discipline in a specific field or geographical area (e.g. histories of archaeological discoveries).

Only a small number of histories of archaeological methods have been written. They are normally 'histories of progress' and do not leave great space to the investigation of the intellectual context in which methods where conceived and applied, or the academic *milieu,* in which their results were used and interpreted.

My dissertation uses the approach of intellectual history to examine the historical development of a field of archaeological research – chronology – that usually generates expectations of objectivity. Analysing it from the perspective of its cultural and historical conditions of possibility is an entirely novel endeavour.

This topic is inspected through four case studies, two of which regard long-standing chronological controversies, and two of which concern the invention and early adoption of dating methods. The research presented studied the main publications and excavation/laboratory reports against the backdrop of contemporaneous politics, propaganda and intellectual disputes.

The four case-studies show how ideologies, political conditions, sub-discipline mindsets and intellectual identities are relevant to the invention and adoption of dating methods, to the selection of variables deemed to be time-dependent, and to the reliability assigned to different methodologies in different contexts.

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Introduction

“Historians write in chronological order, antiquarians in systematic order”: this statement was written by one of the most influential scholars of the last century in a foundational article on the history of classical scholarship.[1]

The above citation is emblematic of the nodal role that chronology has been assigned in historical disciplines.[2] Establishing the date of an event, text or object is considered a primary goal in several academic fields, from art history to diplomatics. The following dissertation will be focusing on archaeology. However, it will not disregard the interplay with other disciplines, which is often part of chronological controversies.

0.1 Archaeological chronologies

The direct referents of archaeological chronologies are objects and strata. Archaeological dating methods are usually applied to empirical objects.[3] However,

[1] A. Momigliano, 1984: 5. Translation by the author.

[2] Besides Momigliano (1984), see at least Febvre (1968).

[3] Cf. handbooks such as: Carandini 1991; Renfrew-Bahn 2006; Manacorda 2002 and Fornaseri 2002.

chronological controversies often concern an historical event or a transition.[4]

Chronology is often intuitively used as a basic datum, on which interpretation – of the object, the event, the iconography – is supposed to depend.[5] Nonetheless (or maybe because of it) chronological determinations are frequently the object of harsh controversies. The history of archaeology is full of debates on the chronology of historical transitions and artefact sequences: these *querelles* evolve with the discipline, responding to the introduction of new archaeological methods and theories.

In this dissertation chronology building and its methods will be investigated from a cultural-historical perspective, trying to unveil the assumptions and interpretations that lay behind the choice of certain dating methods, the intellectual contexts in which such methods were developed and the

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[4] Cf. the examples of chronological controversies in Bickerman 1968, Bäbler 2005 and Lehoërff 2008a.

[5] This idea and its implications on the reality of history has been the object of ample debate in several disciplines: cf. Pomian 1984 and the notorious dispute between Hayden White (1973, 1987, 1992) on one side, Arnaldo Momigliano (1981 and 1987) and and Carlo Ginzburg (1988, 1992) on the other one. Post-processual archaeology has produced a vast literature on the topic: see at least a monographic number of the *Archaeological Review from Cambridge* on time and archaeology (1987), Gosden 1994, Thomas 1996, Karlsson 2001, Lucas 2005 and Bailey 2007.

conditions under which they were received and applied to specific chronological problems.

0.2 Histories of archaeological chronologies

Monographs on the history of archaeology usually include some paragraphs, or even chapters, on the invention of dating methods: almost invariably they are presented as stepping-stones in the history of the discipline, achievements and/or discoveries that end up affecting the everyday practice and the theoretical framework of archaeology.[6]

Only few attempts have been made in the opposite direction: investigating how cultural and historical factors have affected the development of dating methods. These have mostly concerned specific periods and/or geographical areas. This is the case for the works of O'Brien and Lyman,[7] who thoroughly investigated the development of typological dating methods for the American Southwest, especially for pottery. Wider in scope is the collection of essays edited by Nash:[8] it includes several 'hard methods' and one rather compelling article advocating the need for a

[6] E.g. Guidi 1988; Trigger 1996: 121-129 and 382-384; Schnapp 1996: 275-317; Barbanera 1998 and 2015; Calcani 2007; Manacorda 2008; Gamble 2016.

[7] O'Brien and Lyman 2002 and Lyman and O'Brien 2006.

[8] Nash 2000.

sociological study of archaeological knowledge.[9]

The collective volume *Construire le temps* edited by Anne Lehoërff comes close to the idea of an intellectual history of archaeological chronology. The essays focus on a specific spatio-temporal context: the last millennia BC in European pre- and proto-history.[10] However, the field is wide enough to accommodate different perspectives and approaches: the volume includes articles on the history of dating methods,[11] of specific chronological disputes[12] and of theoretical notions of time.[13]

Chronological controversies are usually the subject of a lengthy literature and frequent summaries of past studies are produced in an attempt at resolving them.[14] In these publications the opinions of previous scholars are often contextualised in reference to their philosophical, political, religious or ideological opinions. However, the aim of such remarks has often more to do with discrediting the 'biased' work of colleagues than with a genuine interest in the history of intellectual thought.

A history of archaeological chronology building, therefore, is still to be written. This dissertation provides a first attempt

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9 Croissant 2000.

10 Lehoërff 2008a.

11 Thrane 2008, Lambert 2008 and Evin 2008.

12 Among others, Stig Sørensen and Reba y-Salisbury 2008, Brun 2008, Delpino 2008, Kaenel 2008.

13 See at least Pare 2008, Collis 2008 and Lehoërff 2008b.

14 Levy and Higham 2005 and De Marinis 2005 are exemplary cases.

in that direction. The different case studies were selected by the author to enable the investigation of a wide array of different historical and conceptual elements which have impacted archaeological chronology in the last 170 years.

Moreover, the four case-studies form a coherent complex: both their selection and their analysis are the outcome of a common mindset. Indeed, some readings had a major impact on the study design. They determined the intellectual instruments applied and informed the concepts that will be highlighted in all chapters.

In particular, these concepts can be summarised in three main elements: the structure of archaeological inductions in the form of a bridge, as it was elaborated by Jean-Claude Gardin;[15] the analytical approach identifying archaeological units, their construction and their respective relations, as discussed in Clarke[16] and Ramenofsky;[17] an approach to intellectual history inspired by Ginzburg[18] and Momigliano.[19]

0.3 The selection of case studies

In order to show the validity of such an approach, four case-studies have been selected: two archaeological sites which

[15] Especially Gardin 2000 and 1990.

[16] Clarke 1968 and 1972.

[17] Ramenofsky 1998.

[18] Especially Ginzburg 1986.

[19] Especially Momigliano 1984.

remain at the centre of long-standing chronological controversies and two case-studies focusing on dating methods widely adopted in archaeological practice.

The two archaeological sites are the Grotte de la Verpillière in Germolles (Southern Burgundy, France) and the Fusco Necropolis in Syracuse (Sicily, Italy). Their selection is based on four main characteristics:

- Their centrality in a chronological dispute: they were both at the centre of at least one main controversy, with several complex ramifications that traversed the history of archaeology.

- Their long research history: both sites were excavated for the first time in the 1860s and they continue to be discussed and reanalysed until today. This allows to show how the scholarly discourse developed in time according to academic, political and intellectual priorities of the present.

- Their different intellectual and historical milieu: the geographical location of the two sites and their belonging to different sub-disciplines (respectively Prehistory and Classical Archaeology) ensure that the underlying questions, the cultural contexts and the political forces at stake are very different.

- The array of different methodological issues each site poses: these two case-studies allow to explore several different concerns that are key in chronology building. Not only are different dating methods involved (index fossil and radiocarbon dating in the first one, cross-dating and historical dating in the second one), but also different concerns (towards the intelligibility of a type in Germolles and towards the position of types in a sequence in Syracuse) and different priorities (accuracy in the first case and precision in the second one).

Each site is analysed in a dedicated chapter.

Chapter one focuses on Grotte de la Verpillière I in Germolles. This cave was first excavated in 1869 and soon featured heavily in the discussion on the *question Aurignacienne*: Gabriel De Mortillet used the osseous artefacts found in the cave as a chronological milestone in his *Prehistoire*. Henri Breuil employed them to define the characteristics of the Aurignacian 'revolution'. In this chapter, the chronological discourses on the Grotte de la Verpillière have been analysed against the background of the different theories about the Neanderthal – Modern Humans transition, while assessing the impact of modern concerns on such theories. The Aurignacian technocomplex is defined as a conceptual unit. Then, one of its defining features, the split-base point, is analysed in depth. Finally, its validity as a chronological indicator is investigated, bringing to scrutiny the very notion of 'type artefact'.

Chapter two focuses on the Fusco necropolis in Syracuse. The site was first explored in 1868, therefore it has been part of the archaeological discourse for a similar amount of time than the first case-study. However, the political situation of Sicily was very particular: after the rebellion for independence in 1848, intellectuals were divided: some stayed loyal to the independentist agenda, but the majority embraced the idea of a unified Italy. Therefore, the accounts of the first excavators are to be read in relation to the struggle between independentist and unitarian ideals. The Fusco necropolis very quickly became a key site for the definition of the chronology of proto-Corinthian pottery - one of the most debated topics in Classical archaeology throughout the XX century. The analysis of this debate allows for the scrutiny of assumptions and approximations needed to anchor a typological sequence to historical dates and/or to other sequences.

By contrast, the third and fourth case-studies are used to analyse the development of two dating methods, investigating their intellectual roots and the context of their first reception. The third case-study retraces the first steps of so-called ‘object-based dating methods’ (from typo-chronology to seriation) in the XIX century up until the beginning of the XX century. The fourth case-study concerns the invention of radiocarbon dating in the aftermaths of World War II.

These two case-studies were chosen for their multi-layered history. Two factors have contributed to their selection:

- The disciplinary boundaries they crossed: the birth of radiocarbon dating involved chemists and physicists before it engaged archaeologists. The interplay between 'hard' and 'soft' sciences is to these days extremely dynamic and variable in different intellectual contexts. 'Object-based dating methods' have an incredibly diverse background, from antiquarianism (especially numismatics) to geology and biological taxonomy.

- The different intellectual, historical and political contexts under which they were developed: radiocarbon dating was invented in post-war American society, where politics and propaganda had a strong impact on science (and its funding) and on culture in general. 'Object-based dating methods' were developed throughout the XIX century, in a period of dialectic confrontation between religious beliefs and the Illuminist and Positivist ideas of science and history.

Each case-study is analysed in a dedicated chapter.

Chapter three discusses the development of 'object-based dating methods', starting from the necessary premise of the discovery of the deep past. The chapter presents the debate between Unitarianism and Catastrophism, as well as the relevance of Cuvier's 'static morphology' on the very idea of 'type'. Presenting the role of Ch. J. Thomsen in the development of the three-age system, the connections between Scandinavian archaeology, numismatics and ethnography are highlighted. As for the father of typology,

O. Montelius, his complex relation with positivism and social evolutionism is illustrated in detail. Finally, the adoption of combinatorial statistics to build a multilinear sequence of types, introduced by the mathematician F. Petrie at the very beginning of the XX century, is described as an attempt at revising the concept of time – an intellectual endeavour that, in those years, was very relevant for physicists.

Chapter four focuses on radiocarbon dating, illustrating the history of its invention, validation and constant revisions (C. Renfrew would call them 'revolutions'). The research focuses on the interplay between different academic fields and disciplines. Furthermore, it examines the political and ideological conditions under which radiocarbon dating was developed and largely popularised, as part of the agenda of D. D. Eisenhower's Atoms for Peace program. Finally, the study offers some data on the early reception of the method and how the attitude towards isotope dating in different sub-disciplines could be very variable, depending on their priorities and common practices.

In conclusion, this dissertation identifies several ways in which cultural-historical elements have entered chronology building in archaeology. Some issues appear in several case-studies, though in different forms. This allows to highlight some key themes in the history of archaeological chronology, which can be useful to anyone who would want to embark on an analysis of long-standing chronological disputes or to investigate dating methods from a cultural-historical perspective.

In conclusion, this dissertation intends to highlight the wide array of cultural, political and academic instances affecting archaeological chronologies and the methods used to obtain them. The analysis presented here dissects these instances, reconstructs and contextualizes the multifarious ways of their agency within a number of exemplary cases.

The same cultural historical approach could be used to analyse and contextualise many other long-standing chronological controversies. Conversely, the same type and tools of analysis proposed in this research can be applied to all the methods involved in such controversies and to the intellectual context of their birth and adoption in different archaeological and historical circles. Taking this further, the Appendix provides an in-depth analysis of ^{14}C dating: it exemplifies how all dating methods can be broken down to their components, extrapolating models, theories and assumptions which necessarily underly the ways we measure time.

Chapter 1
La Grotte de la Verpillière I, Germolles (FR)

1. 1 Grotte de la Verpillière I. An emblematic case study

Grotte de la Verpillière I is a cave settlement in the territory of Mellecey, few meters uphill from the bank of the Orbize river. It was first excavated in the mid-Nineteenth century and again several times by different investigators[1]. The most recent excavation began in 2006 and was led by H. Floss of the University of Tübingen, who also joined a *Project Collectif de Recherche* (CPR) on Palaeolithic sites in Southern Burgundy[2]. As many scholars have recognized, the history of excavations is crucial for understanding this site, mostly because of the uncertain and sometimes contradictory stratigraphic reports given by different investigators: it is not unusual, then, for authors to reference previous excavations and collected materials to interpret the

[1]A detailed account of the history of archaeological research at the Grotte de la Verpillière can be found in Dutkiewiz and Floss 2015.
[2] Annual reports on the excavation at Germolles have been published from 2006 to 2016 (Floss et. al. 2006 – 2015b). In 2006 a new cave was discovered, close to the former and with intact stratigraphy: from 2015 Grotte de la Verpillière II became the focus of the mission.

stratigraphy and chronology of the site[3]. In this chapter, the chronological conundrum surrounding this particular site will be analysed in detail: this will show the approximations, assumptions and inferences that lay behind the different chronologies proposed over the last century and a half. Hopefully such analysis will help demonstrate how intellectual factors have affected (and cannot but affect) our chronology building processes and how a reverse process can help us disentangle data from inferences.

One of the most problematic and challenging aspects of this site is that since its discovery it was involved in the complex discussion on the Middle – Upper Palaeolithic transition.[4] In particular, it was used as an argument and exemplary site both for the Mousterian, for the Aurignacian and for the Châtelperronian industries. The chronology and the very definition of all those industries have been the object of intense debate over the last one and a half centuries. What do we mean by Châtelperronian? A human group, a peculiar kind of blades, certain typologies of artefacts, or a combination of the above[5]? Which moment in history can be

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[3] Delporte 1955; Combier 1959; Dutkiewiz and Floss 2015, 19-20.
[4] A list of publications on the topic can be found in the website of the PalaeoChron ERC project, whose aim is dating the transition: https://palaeochron-project.wixsite.com/palaeochron/publications.
[5] For a clearer explanation of the concept of 'cultural unit' through the example of the Aurignacian technocomplex *vide infra* pp. 73-82.

measured through available dating methods (e.g. the making of a stone tool, the death of an animal from which a bone tool was made)? And what event or interval are we interested in dating (e.g. the time of occupation of a certain site, its relative chronology with rapport to other sites, the arrival of a certain human group)[6]? Did concepts such as cultural evolutionism and positivism play a role in the definition of the site chronology[7]? Analysing Grotte de la Verpillière I in Germolles offers a chance to explore these questions and more, while unpacking the epistemological procedures that led to old and current chronological determinations.

1.1.1 Charles Méray

In 1869 Charles Méray publishes a short description of the excavation he had conducted in Germolles over the last year: he informs for the first time the scientific community of the existence of a Mousterian station in the Grotte de la Verpillière[8]. He mentions two levels of occupation in the area in front of the cave: the upper layer had elephant and

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[6] The target event – dated event dynamic has been widely discussed for radiocarbon dating since its first schematic definition (Waterbolk 1971, 1983), but it can be applied to nearly all archaeological dating methods.
[7] The problem of relating variables (e.g. shape, ornament atoms, style, civilisation traits) will be one recurring topic of this dissertation.
[8] Méray 1869.

rhino remains, and traces of fireplaces (calcinated bones); the lower layer was made of stone fragments cemented by red clay and it contained horse, ure and hyena remains, with worked silex artefacts and fragments. It should be noted that Méray carefully registers the number of teeth from different faunal species. This is particularly relevant, because for most of the XIX century fauna was considered a viable option for dividing the Palaeolithic in smaller periods[9]. Nonetheless, Méray does not base his chronological attribution on faunal remains, but on human industry[10]: he finds that the most remarkable pieces are the spear heads, mostly of Mousterian type:

> Elles portent, sur l'un des côtés du taillant, cette surface plate signalée pour la première fois par sir John Lubbock, et qui leur donne un caractère qui

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[9] Edouard Lartet established a system to divide the Palaeolithic in three epochs, according to the relative abundance of faunal remains: the epochs of the bear (youngest), the epoch of the reindeer, the epoch of the mammouth (oldest), cf. Lartet 1861. This method, while less used than typology, was held as an independent confirmation by several scholars for most of XIX century, so that André De Mortillet still mentions it in the 1900 edition of his father's book *Le Préhistorique* (De Mortillet 1900, p. 20-24).

[10] In 1869, the same year when Méray's article was published, Gabriel De Mortillet (1869) had argued for the first time against a periodization based on faunal remains and contended the suitability of human industry for the subdivision of the Palaeolithic into smaller periods. This being one of his main tenants, this concept will return in most of his later works (e.g. De Mortillet 1883, 16 - 23).

> les distingue des formes du diluvium de la Somme[11] .

The Mousterian taxon, in this case, refers to contexts characterised by silex with one flat surface: this trait, together with the complete absence of worked bones, has been the marking trait of the 'Mousterian' for a long time[12]. The word 'diluvium' has in those years a controversial meaning: some scholars use it in its geological meaning, indicating the quaternary alluvial layers; others refer to those same geological strata while attributing them to the biblical Diluvium[13]. Because he is willing to put extinct species' remains in the same context as artefacts, he probably applied the first meaning.[14]

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[11] Méray 1869, 85.

[12] See De Mortillet 1883, 252 – 263 for the traditional definition of Mousterian lithics; cf. Kuhn 2014, 81-123 for a complete recollection of new and old interpretations of Mousterian technology (with extensive bibliography).

[13] De Mortillet 1883, 8-15; for a detailed account of the intellectual and religious themes surrounding the 'diluvium debate' in geology and history see Rossi 1979; for a detailed history of the 'diluvium debate' in French Palaeolithic archaeology, cf. Groenen 1994, 155ss; for a synthetic account see Trigger 1989, 92-100.

[14] On the dispute between intellectuals believing that the biblical Diluvium separated the previous world (i.e. the world where extinct species lived) and the new world (i.e. created as we can witness it in the present) and those advocating the existence of a deep past of humanity *vide infra*, pp. 210-221.

While the only chronological determination is a comparison with the site of Le Moustier (which was already emblematic of an epoch, in De Mortillet's early chronological tables[15]), he admits that osseous materials and fauna are close to those of Aurignac, a site that will soon become very important (and controversial) for the construction of the Palaeolithic chronology[16].

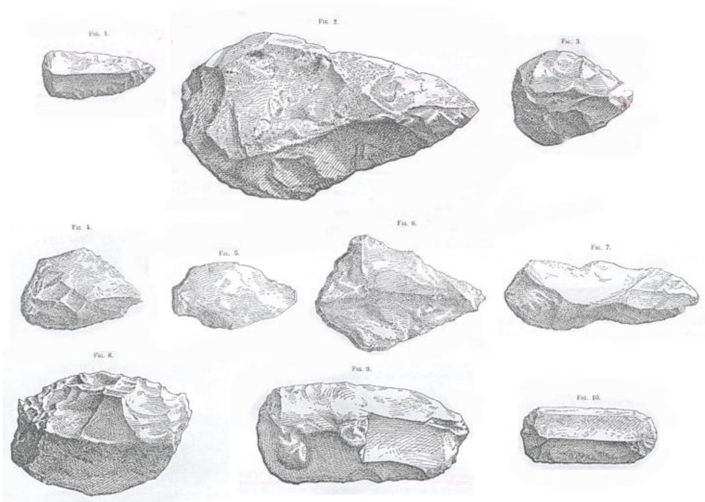

Figure 1a - Drawings of lithic artefacts from Grotte de la Verpillière I (Méray 1876, n. 1-10).

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15 De Mortillet (1869) isolates four epochs in the Palaeolithic and, in accordance with geological academic tradition, names them after an emblematic site: Chellean (older) – Mousterian – Solutrean – Magadalenian (younger).

16 Bouyssonie 1954 provides an exhaustive account of the early history of the Aurignacian as a concept; Teyssandier 2008 collects the most recent and critical discussions on the matter.

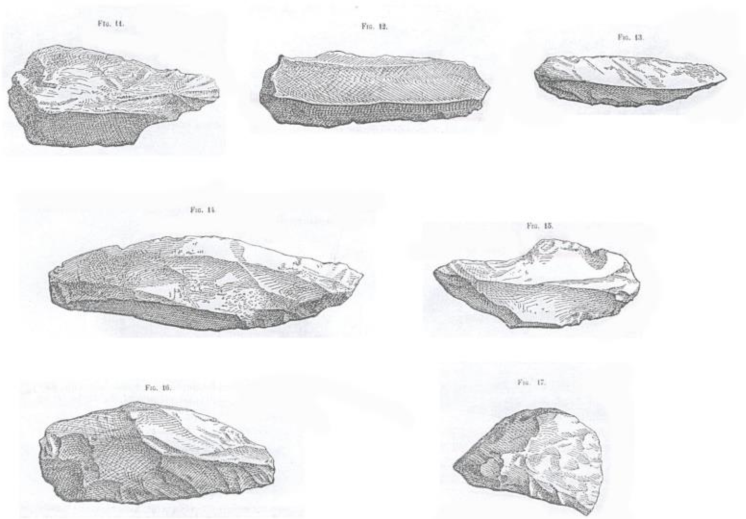

Figure 2b - Drawings of lithic artefacts from Grotte de la Verpillière I (Méray 1876, n. 11-17).

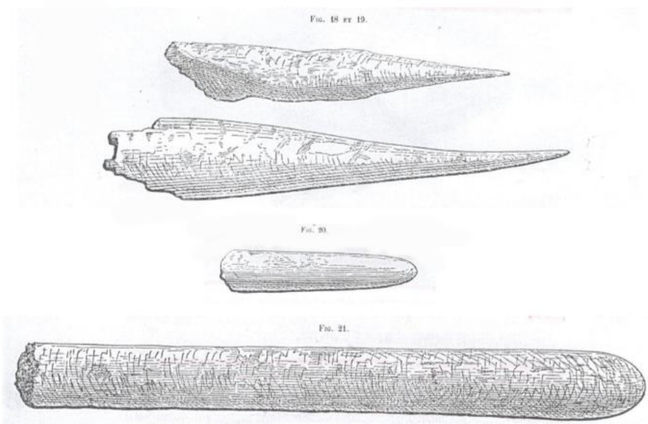

Figure 3c - Drawings of lithic artefacts from Grotte de la Verpillière I (Méray 1876, n. 18-21).

In a later, more comprehensive, publication Méray illustrates in depth his findings in the Grotte de la Verpillière, including several images of silex and osseous artefacts collected during the excavation[17] (fig.1a-c). The description of the archaeological context here is more detailed: the first layer is described as blackish, full of calcinated and broken bones, bearing most osseous artefacts and two greatly preserved mammoth molar teeth; the second layer, turning red towards the base, contains a lot of silex, as well as ox, horse, reindeer and hyena faunal remains[18]. The reference to Lartet's works on the chronological classification of faunal remains is here made explicit: specifically, the association of mammoth and *rhinoceros tichorhinus* is said to be typical of the lower layers of the Diluvium (with a capital D)[19]. In accordance with such chronological determination, the article reports the recovery of typologically and chronologically relevant artefacts. In particular, Méray describes some osseous and lithic tools, according to the taxonomy of Palaeolithic types as it was conceived of in the second half of the XIX century: the so-called Mousterian points; and spear heads of the Saint

[17] Méray 1876; n. 19 – 20 – 21 very likely represent the osseous artefacts whose samples were dated in Oxford: P42476, P42477, P42478 (the latter could not be dated for low collagen yield).

[18] *Ibidem*, 254.

[19] *Ibidem*, 255.

Acheul type, some of which presenting a flat side, characteristic – according to sir John Lubbock[20] – of the Mousterian epoch[21]. A comparison is established between the osseous artefacts found in Germolles and those found in Aurignac and Solutré; it is highlighted, as well, that the presence of ornaments resembles Aurignac, while they are not at all present in Le Moustier[22]. The last pages of the article are an attempt at a chronological classification of the site. The frame of reference is the four-epochs classification of the *Musée de Saint-Germain*:

1) Mousterian (the most ancient): the type site is Le Moustier cave, which gave the name to the points of the same name; it contained several spear heads of type Saint Acheul. The lithic industry is here characterized by the flat surface of one side; osseous industry and ornaments are absent.
2) Solutrean: in the site of Solutré were recovered several beautiful silex worked on two sides. While the axes seem absent, and worked bones quite rare, various sculpted figurines were found.
3) Aurignacian: the epoch takes the name from the cave of Aurignac, where silex is less abundant and their forms less diverse than in Solutré, but

[20] Lubbock 1865, 249-254.
[21] Méray 1876, 258.
[22] *Ibidem*, 265.

instruments made of antler and wood are quite common, as are ornaments and pendants.

4) Magdalenian (the youngest): the name of this period originates from the cave of La Madeleine, where antler and bone objects show engravings and decorations[23].

On the base of these data, Méray attributes the Grotte de la Verpillière to the Mousterian epoch, despite the presence of osseous artefacts and ornaments. Indeed, he notices, the silex is not as beautifully worked as it is at Solutré, nor are the osseous artefacts engraved as at La Madeleine. The Mousterian of La Verpillière – he says – is 'more complete', closer to younger epochs for the presence of worked bones and antler. While this chronological determination only aims at positioning the site in a relative scale, it should be noted that an argument is made for a fast development of the four industries over a relatively short period of time[24]. It appears that in this case Méray implicitly gives more value to silex than other materials: to be more precise, it seems that this chronological determination stands on at least two generalisations. First, silex is related to time in an evolutionary and roughly linear manner[25] and is therefore

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[23] *Ibidem*, 265-266.

[24] *Ibidem*, 266.

[25] The idea of progressive achievements in lithic technology appears quite transparent in Méray's description of Mousterian points as

more suitable for chronological purposes than other materials. Second, Germolles' site – as well as the four sites that gave the name to the four corresponding periods – are to be pigeonholed in one slot of this relative chronological table: indeed, no difference is made among the two excavated layers and it is the site as one unit that is the object of this chronological determination.

1.1.2 Gabriel De Mortillet and *la question Aurignacienne*

In 1883 Gabriel De Mortillet includes the Grotte de la Verpillière I in his comprehensive textbook *Le Prehistorique*[26] and the site becomes a part of a larger taxonomy of European prehistory. In the first edition, he briefly mentions the site as a Mousterian station: besides the typical Mousterian silex artefacts described at length by Méray, he reports the finding of some "coups-de-poing chelléen"[27]. However, there is no mention of the osseous artefacts found in the cave. The fame and authority of Gabriel De Mortillet made his assertions extremely influential at least until the second decade of the XX century. He was, indeed, one of the founders of archaeology as a scientific discipline: he was a strong proponent of the existence of the 'fossil men' and his work

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«véritables acheminement à la pointe de flèche telle qu'on la rencontre aux époques postérieures» (*Ibidem*, p. 259).

[26] De Mortillet 1883.

[27] *Ibidem*, 281.

helped prehistory to escape the constraints of biblical studies; he applied the principles of geology and biology to archaeology, adapting methods and concepts such as stratigraphy, index fossils and evolutionism to the new discipline of archaeology (which, according to him, was born from the mating of history and geology). At the very beginning of his landmark book, *Le Préhistorique,* he reports the different attempts at dividing the Palaeolithic in shorter periods and advocates a chronological system based on a selection of artefact types that, in his experience, could be most effectively related to time, serving as chronological indicators[28]. His periodisation of the Palaeolithic is represented as a table (fig. 2). While he builds the table to include geology, climate, flora and fauna, and human industry[29], he clearly states that for him technology is the main indicator:

> L'industrie humaine, plus variable et plus rapidement renouvable que les êtres organisés ou les conditions atmosphériques, offre par cela même des caractéristiques plus tranchées.[30]

His ideas are mediated by a sincere adherence to positivism (he was the founder of the journal *Matériaux pour l'histoire positive et philosophique de l'homme*) and the belief that cultural

[28] *Ibidem*, 16-23.
[29] *Ibidem*, 127-132.
[30] *Ibidem,* 18-19.

evolutionism can be applied to human industries to develop a sound relative chronology.

Tableau des époques quaternaires.

NOMS.	CLIMATS.	ACTIONS GÉOLOGIQUES.	PALÉONTOLOGIE VÉGÉTALE.	PALÉONTOLOGIE ANIMALE.	INDUSTRIES.
Magdalénien.	Froid et sec.	Formation du diluvium rouge. Dépôt atmosphérique.	Mousses polaires en Wurtemberg.	Homme, race de Laugerie-Basse. Grand développement de la faune du Nord : Renne, Saïga, etc. Extinction de l'*Elephas primigenius*.	Gravure et sculpture. Instruments en os. Déchéance de la pierre. Beaucoup de lames. Burin caractéristique. Double grattoir.
Solutréen.	Température plus douce.	Très courte relativement. Continuation des terrasses. Retrait des glaciers.		Homme (?). Chevaux très abondants. Développement du *Cervus tarandus*, *Elephas primigenius*. Plus de rhinocéros.	Vers la fin apparition des instruments en os. Perfection de la taille de la pierre. Pointes taillées sur les deux faces et aux deux bouts. Pointes à cran. Origine et larges développements du grattoir.
Moustérien.	Froid et humide.	Formation des terrasses. Grande extension des glaciers. Déblaiement des vallées. Exhaussement du sol.	Tufs de Resson. Lignites de Jarville.	Homme, race de l'Olmo. *Ovibos moschatus*, *Ursus spelæus*, *Rhinoceros tichorhinus*, *Elephas primigenius*.	Pas d'instruments en os. Dédoublements de l'instrument chelléen. Pointes, racloirs, scies retouchés d'un seul côté.
Chelléen.	Chaud et humide.	Lehm supérieur. Alluvions des hauts niveaux. Remplissage des vallées. Affaissement du sol.	Plantes du bassin méditerranéen dans la vallée de la Seine et à Canstadt.	Homme, race de Néanderthal et de la Naulette. Développement des cerfs. Hippopotame. *Rhinoceros Merkii*, forme pliocène, *Elephas antiquus*.	Pas d'instruments en os. Un seul outil en pierre, l'instrument chelléen, toujours en roche locale.

Figure 2 - Chronological table for the Palaeolithic - Gabriel De Mortillet, *Le Préhistorique*, 1885: 131.

An absolute chronology of the Palaeolithic was not considered an achievable aim until the invention of radiocarbon dating. At the very beginning of his work De Mortillet states:

> […] il est impossible de rapporter tous ces intéressantes découvertes à la chronologie historique, à une chronologie absolue. Pour le classer il faut forcément avoir recours à une chronologie relative. Thomsen a cherché et trouvé la base de cette chronologie dans le développement de l'industrie. Plus on remonte

> dans le passé, plus l'industrie humaine se simplifie. Thomsen ayant reconnu cette vérité, je dirai même cet axiome, qui s'est confirmé et se confirme partout de plus en plus, en a déduit sa division des temps préhistoriques.[31]

In his view, cultural evolutionism governs both human technology and human societies. Following the path drawn along the chapters, progress appears to be the motor of (pre)history: Tertiary history is the history of the origin of man; Quaternary history is the history of man's advancement to the cultural stage of savagery; then the history of the current era is the history of civilisation[32]. The great importance attributed to typological evolutionism – which, at times, outweighs stratigraphic observations – is probably the reason why De Mortillet's chronological framework barely survived his author.

He divides the Palaeolithic in four periods, which take their names from eponymous sites:

1) Chellean: it takes its name from the site of Chelles; osseous artefacts are not found in those contexts and only one kind of silex tool is used, the so-called 'chellean instrument'.

[31] *Ibidem*, 6-7.
[32] *Ibidem*, 16.

2) Mousterian: named after the site of Le Moustier, this period is characterized by a diversification of silex tools (points, *racloirs*, saws) with one smooth side, a byproduct of the production of the chellean instruments. No osseous industry is found in Mousterian sites.
3) Solutrean: the site of Solutrée gives the name to this period, when silex artefacts are worked on both sides and on both edges, producing sharp points and scrapers; towards the end of it, some osseous artefacts begin to appear.
4) Magdalenian: in this epoch, named after the site of La Madeleine, lithic industry deteriorates: some blades, scrapers and engravers are produced. But the most remarkable artefacts are made of antler and bone: some of those artefacts are engraved and the first attempts at portable art can be detected[33].

De Mortillet is aware that things are more blurred than they are represented in his classificatory effort: obviously the industry doesn't change over-night and variations might happen at different rates and in different ways in various places. Classification needs some degree of approximation, but it is both possible and useful. De Mortillet compares his periods with the most natural temporal division, the one between night and day: sometimes the transition from one to the other is blurred and it does not happen at the same

[33] *Ibidem*, 19.

time everywhere, but night and day are still inherently different[34]. In the same way, periods can be asynchronous, transitions among them can be blurred, and approximations might be needed. However, he believes in their factual and epistemological existence. Indeed, listing Germolles as a Mousterian station requires a certain amount of approximation. Failure to mention the presence of osseous artefacts at the site might be a simplification intended to strengthen his taxonomy.

In 1900 the third (posthumous) edition of *Le Prèhistorique,* edited by Adrien De Mortillet, presents a very different organisation of its content, but it is based on the same theoretical premises as earlier versions. Germolles is again mentioned for the abundant presence of Chellean instruments and as a Mousterian station, while no mention is made of the osseous artefacts and ornaments described by Méray in his 1876 article[35].

In all these editions, De Mortillet's chronological grid partially agrees with the one used by Méray, though with one rather relevant difference: while the chronological sequence of the Musée de Saint Germain included an Aurignacian period between the Solutrean and the Magdalenian, the classification established by De Mortillet did not. This issue would become especially relevant at the

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[34] *Ibidem*, 16-23; see also Richard 1989.

[35] De Mortillet 1900, 581 and 616.

beginning of the XX century, and both of these chronological constructions would be strongly modified in light of the so-called *Question Aurignacienne*[36].

In 1907, Henri Breuil publishes one of his firsts elongated efforts to construct and define the relative chronology of the Aurignacian period[37]. He contends that the Aurignacian is successive to Mousterian and anterior to Solutrean industries. In this crucial paper he discusses the presence of Aurignacian levels in Germolles. Indeed, the site is mentioned – together with many others – to contradict Adrien de Mortillet's assertion that pre-Solutrean (i.e. Aurignacian) assemblages are a local particularity of late Mousterian industries in certain geographical areas. In his argument, Breuil makes a list of sites where he identifies pre-Solutrean assemblages. While Germolles is among them, it is not discussed at lenght. The subtitle of the article might explain why: "Étude critique de stratigraphie comparée". Breuil's argument is meant to prove that most stratigraphic observations confirm his relative chronology of the Aurignacian and that the one site that is often mentioned as an argument against it – Cro Magnon – is the object of contradictory and untrustworthy stratigraphic reports[38]. As it will become apparent in his next publication on the

[36] Groenen 1994, 162-178; *vide infra* pp. 22-39.
[37] Breuil 1907.
[38] *Ibidem*, 209-219.

Aurignacian period ("Étude critique de morphologie comparée"[39]), Germolles' stratigraphic record was not detailed or reliable enough to be considered appropriate for such an argument.

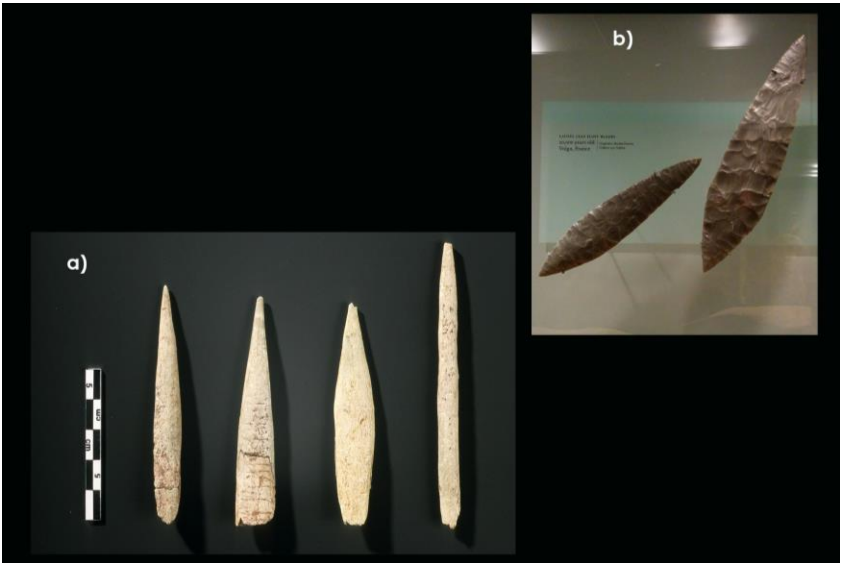

Figure 3 - a) Solutrean laurel leaf blades from Volgu (Musée Denon, Chalon-sur-Saône); b) Split-based points from Trou de la Mère Clochette (Musée des Beaux-Arts, Dole)

It could prove useful to dedicate some time here to the analysis of a methodological problem that is explicitly mentioned in this publication and will often prove relevant – though mostly implicitly – to the chronological

[39] Breuil 1911.

assessments examined in this dissertation[40]. Breuil recalls in his paper the arguments of his opponents. In particular, he recalls the theory of Pierre Girod that Solutrean industries did not include osseous artefacts and came after Mousterian ones, being the material traces of a migration of Eskimos from East to West. According to him, Aurignacian industries must come after Solutrean ones because their type-artefact (the split-base point) is modelled after the leave-shaped silex artefacts that are characteristic of the Solutrean[41] (fig. 3). When Solutrean and Aurignacian contexts present Mousterian and even Acheulian pieces (such as in Châtelperron) he pictures it as the remains of lithics collected by the Eskimos on their trip to the West[42]. This is Breuil's comment on the topic:

> Cette explication, sans aucun doute, solutionne un petit nombre de cas, mais elle est vraiment trop commode pour se débarrasser des choses gênantes, et qui ne cadrent pas avec le 'credo' morphologique de M. Girod. En fait il me paraît incontestable qu'à tous les niveaux de l'âge de la Renne, des formes simples comme les formes moustériennes ont été reproduites, soit accidentellement, soit au contraire très délibérément. […] à des niveaux plus élevés, la même chose garde sa

[40] For a deeper explanation of the problem of 'crossed verification' see Clark 1972.

[41] Girod and Massénat 1900, 13-15.

[42] Breuil 1907, 181 -182.

> signification morphologique, mais cesse d'avoir une portée 'phylogénétique'. Quand faut-il admettre l'une ou l'autre conclusion ? C'est une question que la stratigraphie doit dominer.[43]

This quote highlights one crucial issue that will be of interest throughout this dissertation. It may initially appear as a trivial note on archaeological chronometry: all methods are idealisations of factual reality and/or generalisations inferred from data and theories. In this case, Henri Breuil questions the validity of the phylogenetic theory guiding Girod's generalisations and, comparing inconsistent chronological systems, he selects stratigraphy as the method that should determine the outcome.

While many commentators focus on the order of periods or the role of evolutionism, this is probably the main object of contention in the *question aurignacienne:* for Breuil stratigraphy was ultimately the method that would allow to decide between contrasting chronological constructions; for De Mortillet and his school this role was filled by typology. In *Le Préhistorique,* while using stratigraphy both to confirm the general validity of his periodisation and to fight the ideological battle against the advocates of biblical chronology, De Mortillet admits that strata can mix, for example, when Roman *sigillata* is found with the remains of

[43] *Ibidem*, 182.

extinct animals[44]. Strata can be dug up, mixed and moved. Therefore, he relies on typology to verify the validity of stratigraphic assumptions. Symmetrically, Breuil relies on stratigraphy to verify the validity of typological assumptions. Indeed, typological classification can be, in the worst-case scenario, based on phylogenetic assumptions – an application of biological evolutionism to morphological similarities between artefacts. By contrast, in the best-case scenario it is an approximation that places certain morphological features in a linear sequence based on stratigraphic evidence[45]. Though even in the latter case outliers will be found, not only because things can be conserved and reused and passed over from one generation to another[46]. Outliers can also be the result of certain types (or certain decorations, or certain morphological features in general) not only being used but also being produced for longer than others: according to Breuil, this is the case for the

[44] Gabriel De Mortillet 1883, 8-9.
[45] On the birth and different uses of typology in XIX century European archaeology see Gräslund 1987; for the history of typology in Americanist archaeology see O'Brien and Lyman 2002, 23-58; for an exhaustive account of the methodological debate on typology see Hill and Evans 1972.
[46] e.g. Hochdorf princely grave (Olivier 1999), or any monumental site: e.g. Paestum doric temples were in use until the late Roman empire (Greco 2001 is a useful handbook with plenty of bibliographic references) and the Acropolis in Athens was a palimpsest of visible constructions from many different ages (cf. Pavan 1983 provides plenty of information and bibliographic references on the Parthenon).

so-called ‘Mousterian shapes’, which in his opinion were produced throughout the Middle and Upper Palaeolithic[47]. Outliers are then evaluated through other methods. Stratigraphy is verified through typology (e.g. strata are mixed because roman *sigillata* has been found with Palaeolithic fauna) and vice versa (e.g. the production of so-called Mousterian tools might extend beyond the Mousterian period because those instruments are found in several layers and in association with ‘later’ types). The same could be said of radiocarbon dating: when taking several samples from one site, results are analysed through a Bayesian model (i.e. a model that establishes prior knowledge about the samples, such as their stratigraphic position) to reduce error bars. Outliers are excluded (or weighed down) from models according to stratigraphic constraints and consilience[48]. At the same time, the taphonomy of a site can be established trough the radiocarbon dating of multiple samples from different depths[49]. It should be noted that this paragraph is not meant to argue against the soundness of any of those methods: they are linked together by a series of approximations, and by

[47] While this concept might appear intuitive, some archaeological typologies are built on the unspoken principle of approximation where ‘types’ stand for the same time interval, cf. for example Trendall 1989, 270-271: despite the complex matrix, painters and groups always cover a 25-30 years interval.
[48] Bronk Ramsey 2009.
[49] Wood et al. 2018 with previous bibliography.

challenging these simplifications, the methods have become and are becoming more accurate and precise every day. Instead, through these arguments, one can highlight that in the presence of outliers – which will always be encountered, as our methods necessarily rely on some degree of generalisation – the scholar is called to express a preference towards one method (i.e. towards one generalisation). This preference can sometimes be influenced by cultural and intellectual bias. This is the case, for example, for Gabriel De Mortillet's preference for typology over stratigraphy, which is explicitly dependent on cultural evolutionism[50].

1.1.3 Henri Breuil

Breuil's 1911 publication builds the foundation for all subsequent chronological assessments of the Grotte de la Verpillière I. He analyses several archaeological deposits 'that are neatly characterized as Aurignacian' but still present some distinctive and quite homogeneous industrial traits. According to him, those traits seem to derive in an evolutionistic way from the Aurignacian layer of Abri Audi[51]. The most important among the analysed sites is Châtelperron, but Germolles comes close second. Breuil

[50] *Vide supra,* 22-27.

[51] Breuil 1911, 29.

takes most of his data from Méray, both for the stratigraphy of the site and for the description of artefacts (fig. 4).

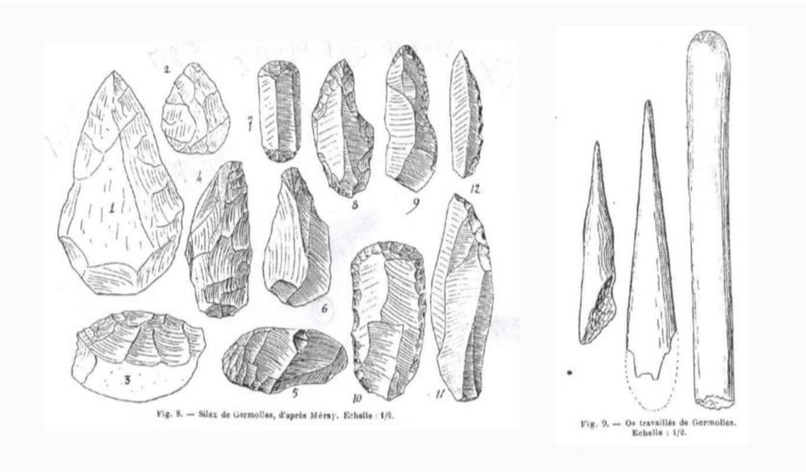

Figure 4 - Drawings of lithic and osseous artefacts from Grotte de la Verpillière I (Breuil 1911, 38-39).

Starting from lithic industry, he notices a peculiar mix of silex types in Grotte de la Verpillière: the so-called 'chellean instruments' are numerous; there are abundant Mousterian tools; some tools typical of the Aurignacian can be counted, especially the carinated *grattoirs*; finally, several blades with retouches on one side (Châtelperronian type) were found. Unfortunately, the position of the artefacts in the stratigraphy is not recorded by Méray, nor is it reconstructed by later authors. Although the presence of Aurignacian types was evidenced since the very first excavation, the deposit generally has a quite archaic look, that – Breuil reckons – is probably the reason why it had been published as a Mousterian station[52]. Osseous industry is rather abundant and prompts comparisons with the Aurignac cave: it includes one actual Aurignacian point (i.e. split-base point); a long *lissoir*; several needles and bones decorated with regular traits. All these are clearly Aurignacian and are to some degree comparable with those found in the cave of Châtelperron. In the same publication, Breuil gives a definition of Châtelperronian sites, which will live longer than its author. They are described as Aurignacian settlements with 'special' characteristics: these include the presence of *coup de poings*, Mousterian instruments and Châtelperronian points; the fauna is quite ancient and osseous materials are still rudimentary. Those variables are the features that define the taxon. However, Breuil does not extend this concept to outside of Southern France, where the transition between Mousterian and Aurignacian can have a different appearance[53]. Indeed, the Châtelperronian and other transitional industries are to this date one of the most debated aspects of Palaeolithic prehistory, mostly in relation to the human species who made them (Neanderthal vs Modern Human)[54]. In Breuil's article one can already find

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[52] Breuil 1911, 39.

[53] Breuil 1911, 75-76.

[54] While the paucity of Neanderthal human remains associated with Châtelperronian lithics has been noticed by many scholars, it is normally assumed that the two are to some extent connected (cf. for example Floss

most of the conceptual nodes that influence the debate on transitional industries. Firstly, it provides some insights on the construction of chronological units: 'Mousterian', 'Châtelperronian' or 'Aurignacian' are names given to periods according to certain taxonomic criteria, that seem to vary over time. At times, one artefact is selected as typical of a certain time interval (and space coordinates), i.e. as an 'index' and a proxy for a certain period: this is the case for split-base points, which are to this day often used as the main indicator for Early Aurignacian occupation levels[55]. Currently, many scholars working on Palaeolithic industries suggest that such distinctions should be based on the technique used to work silex (or osseous materials): instead of the shape and size of tools, they analyse the process of production of artefacts as a diagnostic element[56]. Similarly, Gabriel de Mortillet identifies Mousterian silex tools by their flat surface, because he thinks that they are reworked by-products of chellean instruments[57]. Breuil, instead, seems to think that it is a certain combination of silex and osseous types that allows to classify an archaeological deposit within

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2003; Floss, Hoyer and Würschem 2016; Bar-Yosef 2006, 11-12). Only two sites present skeletal Neanderthal remains in a Châtelperronian level: Grotte du Renne and Saint-Césaire. A recent taphonomic and typo-chronological reassessment of the latter discredited this association (Gravina et al. 2018), reopening the debate.

[55] *Vide infra,* pp. 82-94.

[56] Cf. Goutas and Tejero 2016.

[57] De Mortillet 1883, 252-263.

a specific chronological taxon[58]: following his example, this has become the 'classic' approach to chronological classification in Palaeolithic studies[59]. Breuil's chronological construction, especially the idea of a Middle to Upper Palaeolithic 'revolution', has not been structurally challenged to this day. In 1912 he writes:

> In the present state of our knowledge, it appears established that the arrival of the upper palaeolithics brought about, at the end of the Mousterian, a social and industrial change and a racial substitution so profound, that it will certainly be legitimate in a well-coordinated classification, to separate the Lower Palaeolithic from the times which follow it by a division of equal greatness to that which separates this period from the Neolithic epoch[60].

Since then, chronological disputes have mostly been an issue of precision and accuracy in determining the temporal and spatial coordinates of this *substitution,* while assessing the exact nature of the *racial* component. The passage between the Middle and the Upper Palaeolithic has been called 'revolution', 'transition', and recently 'biocultural shift', but

58 Cf. his definition of Châtelperronian, Breuil 1911, 75.
59 Cf. Breuil 1954.
60 Breuil 1912, 74.

its essential components have not changed: industry, social structures and human groups.

1.1.4 Henri Delporte

The history of Grotte de la Verpillière shows a disconnect between excavations and published data. Indeed, even though the cave was excavated several times after Méray's campaign[61], his publication remained the main reference for the stratigraphy of the cave until the 1950s, when Henri Delporte and – shortly after – Jean Combier, published new

[61] The excavation of Victor Arnon at the beginning of the XX century was published in the *Bulletin de la Société d'Histoire Naturelle d'Autun* (Arnon 1903) but had little academic resonance, as the author maintained that the cave was never inhabited by Palaeolithic men. Joseph Mazenot and his collaborators conducted various excavations in the cave in the first 20 years of the XX century, but they are only documented in their private correspondence and in archival documents (cfr. Dutkiewicz and Floss 2015, 18-21). The excavations of Dr. Lenez, between 1920 and 1930 are only briefly mentioned in his work on the chronology of the quaternary era (Lenez 1940), which we will not examine in depth here, because it does not affect the chronological appreciation of Grotte de la Verpillière I. Nothing was published for the excavations of Olivier Rossé in 1934 (cfr. Dutkiewicz and Floss 2015, 22). In the 1930s, Abbot Guillard excavated the cave to an intact Aurignacian level, but he only mentions it *en passant* in a couple of articles, focusing on the pendants he found there (Guillard 1947, 1954a, 1954b). Finally, a local dentist, Marcel Lafond, seems to have conducted an excavation there after 1946, but his results are only (partially) known through personal correspondence and archival documents.

assessments of its stratigraphy in the light of their own excavations[62].

Henri Delporte was born in 1920 in a family of small shop owners in Turcoing[63]. He sided with the resistance during German occupation and only became an archaeologist after the war, excavating with Louis-René Nougier. In the first years of his career he undertook a series of excavations aimed at clarifying the Middle - Upper Palaeolithic transition, especially the relationship between Neanderthals and Modern Humans. Arguing for flexibility in archaeological classifications, he was an advocate for what we now call 'acculturation theory', claiming that Neanderthals and Modern Humans had contact (especially in Châtelperron) and influenced each other[64]. Unsurprisingly, when discussing the passage between Mousterian and Perigordian, he called for caution in connecting race and industry[65]. The same concerns were the

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[62] Delporte 1955.

[63] Obituary, *Le Monde*, 2 juin 2002.

[64] Delporte 1954, 1955, 1957; The question about the relationship between Neanderthals and Modern Humans is still at the center of the debate on the Middle – Upper Palaeolithic biocultural shift and Delporte's work is to this day a reference for scholars in this field, to the point that those who want to deny the coexistence of Neanderthals and Modern Humans deny the validity of his excavations: cf. Zilhão and D'Errico 2003; Zilhão et al. 2006; Zilhão et al. 2007; *contra* Mellars et al. 2007.

[65] Delporte 1966, 38.

main topic of his 1955 publication on Germolles' Grotte de la Verpillière findings:

> En ce qui concerne le Paléolithique, l'opinion éclairée en est restée aux systèmes du début du siècle : une série d'industries rigoureusement successives et progressives, étroitement liées à des types raciaux également progressifs ; chacune de ces industries […] est caractérisée par un ou plusieurs fossiles directeurs […]. Ce système ne tient pas assez compte […] de l'individualisation d'une série d'industries nouvelles, pour la plupart parallèles à celles du tableau classique : Clactonien, Tayacien, Micoquien, Levalloisien, Périgordien, toutes caractérisées soit par une technique propre, soit par un matériel industriel original ; […] il représente en somme une conception statique et erronée, maladroitement copiée sur celle des sciences géologiques de 1900, alors que la réalité se traduit par une conception dynamique, vivante, avec ses périodes d'accélération et de stagnation […][66].

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[66] Delporte 1955, 154 – transl. "For what concerns the Palaeolithic, the common opinion still reinforces the systems of the beginning of the century: a series of rigorously successive and progressive industries, strictly linked to equally progressive racial types; each one of these

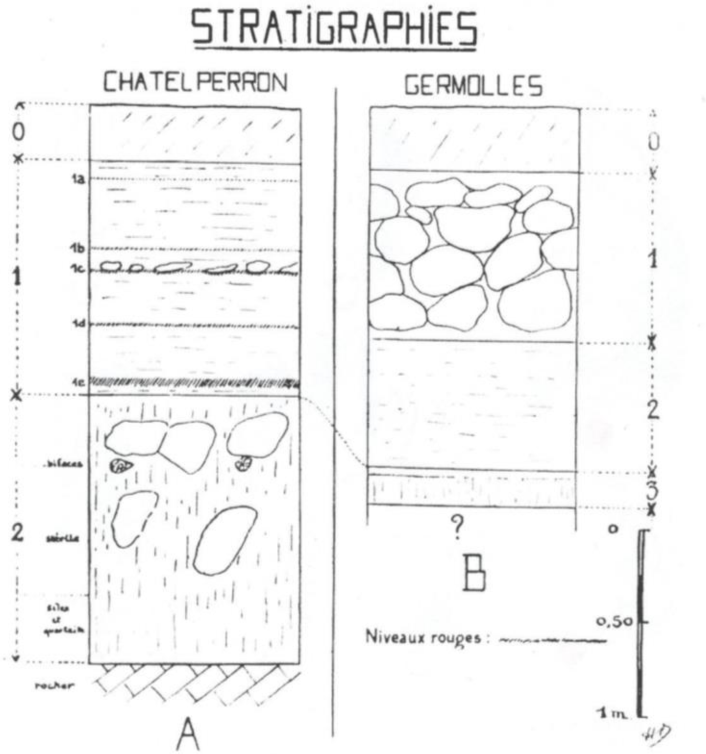

Figure 5 - Stratigraphic cross-section of Grotte de la Verpillière I and Châtelperron (Delporte 1955, 156).

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industries […] is characterised by one or more index fossils […]. This system does not account for […] the identification of several new industries, mostly parallel to the traditional ones: Clactonian, Tayacian, Micoquian, Levallois, Perigordian, all showing a peculiar technique and an original industry. […] It represents a static and erroneous notion, unsoundly copied form the geological sciences of the 1900, while reality should be translated in a dynamic and living notion, with its periods of accelerations and stagnation”.

In this article, he pairs stratigraphic observations from Châtelperron with the ones from Germolles (fig. 5): according to him, the latter gave back three highly reworked and mixed layers (0 – 1 – 2 in his numeration) and one intact and perfectly homogeneous Mousterian level (layer 3) without any trace of Upper Palaeolithic industry. The latter had only partially been excavated at the time of publication[67]. With this paper he intends to advance the hypothesis that there are four kinds of Châtelperronian industries. The oldest with a Mousterian option; the second, 'pure' one, that descends from the first; and from the 'pure' Châtelperronian depart two branches: one influenced by Perigordian and Gravettian industries, and the other progressing towards the Aurignacian facies. In turn, linking the Mousterian to the Aurignacian through the Châtelperronian means denying the migratory explanation of a succession of human groups who brought with them well-defined and individualised industries. He argues for a notion of progress that comes from adaptation to external and internal conditions, especially from the encounter of civilisations and environmental constraints[68]. While a whole paragraph is dedicated to Germolles' stratigraphy and findings[69], the site is never mentioned in the construction of his final argument. Why then was it included? To

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[67] *Ibidem*, 158 – 159.
[68] *Ibidem*, 161.
[69] *Ibidem*, 157-160.

understand this, we should remember that Henri Breuil made Germolles an example of Châtelperronian industry. However, for him, Châtelperronian was a local early version of the Aurignacian that included *coups-de-poing*, Mousterian instruments, Aurignacian osseous artefacts and Châtelperronian points. And Breuil's argument was built on Méray's excavation: while three geological layers (stones, red earth, black earth) could be distinguished, "one does not recognise different (archaeological n.d.r.) levels"[70]. Including Germolles' stratigraphy in his paper, Delporte managed to attribute the co-existence of so many different artefact types to the mixing and reworking of the upper layers, allowing for the construction of the four classes of Châtelperronian industries.

In this paper the link between a theoretical approach and chronological determinations is explicit. In what may be called a 'formation' approach towards culture-historical archaeology, Delporte aims to build a chronological sequence where time still takes the form of taxa (e.g. Mousterian, Châtelperronian, Aurignacian), but has the shape of a spatio-temporal grid. Moreover, the lines in the grid are blurred: in his view, change is not an external, race-dependant factor; it is the internal response of certain societies to both external and internal solicitations. Therefore, changes might happen at different rates and

[70] Breuil 1911, 38.

times in different places. Also, the direction of change – while always part of a causal chain – could be different under different circumstances: this would be, in his view, the case for the parallel (though not necessarily synchronous) development of a Châtelperronian with Gravettian option and of a Châtelperronian with Aurignacian option[71].

1.1.5 Jean Combier

Shortly after Delporte's investigation, the archaeologist Jean Combier undertook a new excavation campaign in the Grotte de la Verpillière. From his private correspondence and personal communications, we know that an initial collaboration with Delporte fell apart because they would not agree on the chronostratigraphic division of the cave[72]. Significantly, Combier never published the results of his excavation: in 1959 he still refers to Delporte's stratigraphic account and – while remarking that Germolles is one of the most important sites for the Upper Palaeolithic in eastern France – he states that no decisive superimposition has been observed. The crucial question raised by this archaeological complex, he says, is the stratigraphic relation of Châtelperronian points with the Mousterian level and the

[71] Delporte 1955, 62.
[72] Combier 10/01/1957; Delporte 15/01/1957 (cf. Dutkiewicz and Floss 2015, 27).

Aurignacian artefacts found in the cave[73]. Information collected from Jean Combier himself in a series of dissertations and excavation reports, and finally published in 2015, can give us a better understanding of the stratigraphic divisions he operated under in his excavation: he found a first layer of debris; a second, reddish, layer with Aurignacian industry; a third dark one with no archaeological material; a fourth level with Châtelperronian industry; and a fifth with Mousterian artefacts (fig. 6).

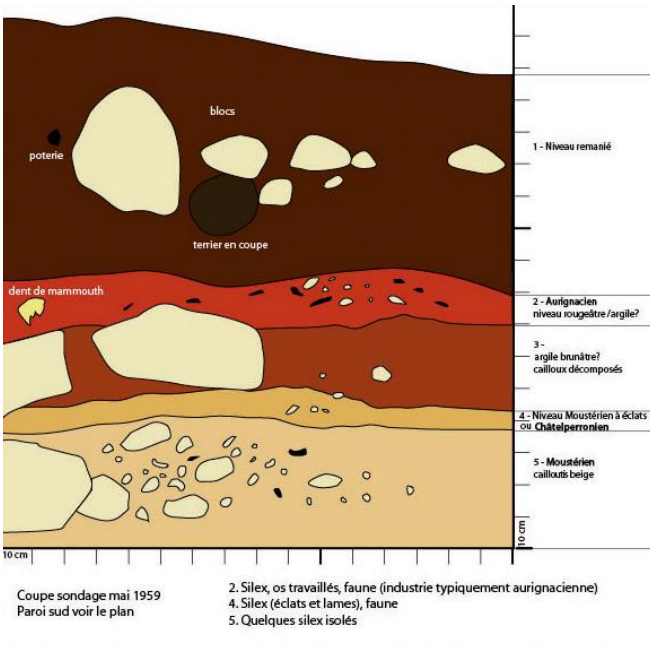

Figure 6 - Reconstruction of Combier's stratigraphy in Grotte de la Verpillière I (Dutkiewicz and Floss 2015, 27).

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[73] Combier 1959, 120-121.

Recently, scholars from the University of Tübingen have tried to find the stratigraphic sequence described by Combier, while checking the connected materials – which until today have been kept in his private house. From the stratigraphic section found during new investigations and after an analysis of artefacts, they argue that the distinction between layers 4 and 5 is probably to be dismissed[74].

1.1.6 Harald Floss

After a long period of inactivity, a new excavation campaign was undertaken by a team of the University of Tübingen, from 2006 to 2015, directed by Harald Floss. During these explorations, some intact Geological Horizons (GH) have been found and some effort to isolate different Archaeological Horizons (AH) has been made. Interestingly, investigators collected and studied archival data from previous excavations for the construction of their chronostratigraphic grid and employed several absolute dating methods[75]. During their 10-years long campaign they

[74] Dutkiewicz and Floss 2015, 27-29.

[75] A useful synthesis of the chronostratigraphic grid emerged from the latest investigations can be found in Floss, Hoyer and Würschem 2016, 151-153.

managed to find some remnants of intact layers, untouched by previous excavations (cf. fig. 7).

Figure 7 - Map of old excavations in Grotte de la Verpillière I, Germolles (Dutkiewicz and Floss 2015, 28).

In Grotte de Verpillière I, the lower level they found is a classical Mousterian layer (GH16) with Levallois reduction scraps and bifacial elements. One sample from this context was dated through ESR/U-Th to between 51000 ± 3000 and

48000 ± 3000 BP[76]. GH16 was found to span several square meters in the western section of the cave interior. In the central part of the cave, an intact Châtelperronian layer (GH40) was found, which was characterized by a strong red colour indicative of the presence of hematite in the soil: the investigators suggested (though it cannot be proven) that this is the layer that both Combier and Méray mentioned in their accounts as being beneath the layer of collapsed stones[77]. One bone sample has been selected from GH40 for radiocarbon dating (OxA 32235) and the result – 49600 ± 3900 BP – has been quite surprising. It is significantly older than other dates from Châtelperronian contexts, especially those from the Grotte de la Renne in Arcy-sur-Cure[78], a site that is considered emblematic of the Châtelperronian industry (even though it has been singled out for its richness, which has been attributed to the influence of Modern Humans[79]). In the central area of the cave where GH40 was found, Tübingen investigators also identified a reliable stratigraphic sequence (fig. 8): at its base there is the virgin

[76] Richard et al. 2016a, Richard et al. 2016b.
[77] *Vide supra* p. 19 (Méray) and pp. 45-46 (Combier).
[78] Soressi and Roussel 2014; Hublin et al. 2012: the older date for a Châtelperronian sample is 40,970±424 BP and a Bayesian model with calibrated dates suggests the interval 40500 – 45000 cal BP for the Châtelperronian occupation. It should be noted that dates were calibrated with OxCal 4.1 and IntCal09: a reappraisal of this data using IntCal13 would be very useful.
[79] Floss 2003, 281-282, with bibliographic references.

rock (GH6), covered by a series of sterile layers (GH2, GH4, GH22); on top of GH4, a Mousterian level called GH15b corresponds to GH16 in the western part of the cave; GH15b is covered by three other Mousterian levels (GH41 b, c, d); in turn, GH41s are covered by GH40, the Châtelperronian level; finally, the Aurignacian layer GH15c that very likely covered GH40 was found collapsed as a result of previous excavations[80].

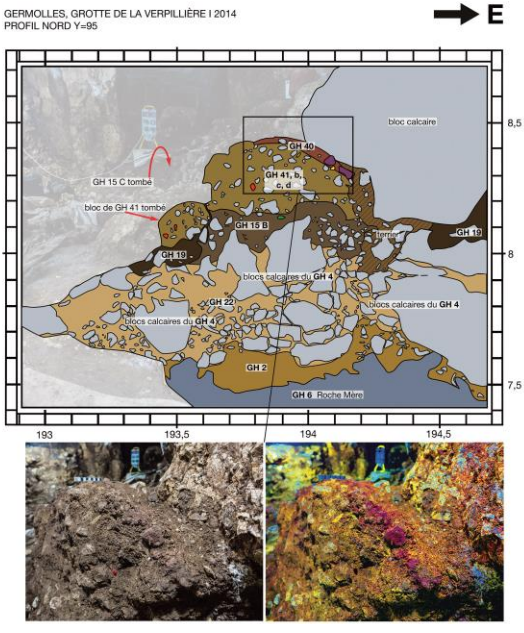

Figure 8 - Cross-section of GH40 in its stratigraphic context (Floss, Hoyer and Würschem 2016, 153).

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[80] It should be noted that Geological Horizons have been divided according to their earthen matrix and the numbers reflect the order in which they were excavated, not their chronostratigraphic attribution (Floss, Hoyer and Würschem 2016, 151).

Besides GH15c, two more intact layers were found in the western side of the cave that contained Aurignacian tools. The excavators, however, are cautious in calling them 'Aurignacian layers' and use the definition 'Upper Palaeolithic with Aurignacian affinities': they did not find any diagnostic Aurignacian tools, as artefacts collected from previous excavations led to expect[81]. One bone sample from GH24 was radiocarbon dated, obtaining an age > 44,330 BP (OxA 32228) [82]. Two intact layers with Gravettian industry were also excavated, one in the cave interior (GH23) and the other outside the cave: this was the first time that a Gravettian occupation was confidently identified in Grotte de la Verpillière I. Four radiocarbon dates were obtained on samples from the Gravettian level outside the cave (GrA-44701, GrA-44702, GrA-45482, GrA-45450) and the results span from 26,010 ± 120 to 28,900 ± 440 BP[83]. In Table 1a, samples selected for radiometric dating are listed according to the Geological Horizon of provenance and their material characteristics.

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[81] Ibidem, 154.

[82] Cf. Heckel et al. 2016.

[83] Floss, Hoyer and Würschem 2016, 154. Table 1a provides a list of samples and results, kindly provided by Harald Floss and the radiocarbon laboratory of the University of Gröningen.

Sample ID	Context				Material characteristics			Comments	Age BP		Lab-code
	Cave	GH	Square	Number	Material	Sample type	Modified?	Comments	CRA	+/-	Sample
P38856	VPI	GH 1	191/098	13.1	ivory	ivory	Yes, flake	Ivory flake made by percussion from mixed humic layer (350 mg)	34,850	650	OxA-32232
P38854	VPI	GH 15	192/097	869.1	ivory	ivory	Ambiguous	ivory flake from intact Middle Palaeolithic deposits (610 mg)	>46,700	-	OxA-32231
P38857	VPI	GH 15	192/096	356	ivory	ivory	Ambiguous	Ivory flake/fragment from intact MP layer (380 mg)	>46,800	-	OxA-32233
P38849	VPI	GH 24	191/097	75	bone	long bone	Yes, percussion	long bone diaphysis fragment with potential percussion-impact points on lateral exterior fracture plane (600 mg)	>44,300	-	OxA-32228
P38866	VPI	GH 40	195/096	34	bone	bone	No	bone fragment (looks chewed-up and digested) from potentially in place UP (171 mg)	49,600	3,900	OxA-32235
P38862	VPI	GH 41c	194/096	213.4	bone	long bone	No	chewed-up diaphysis, from sediments immediately underlying CP (640 mg)	>49,900	-	OxA-32234
P38861	VPI	GH 41b		8.28	bone	long bone	No	Chewed-up diaphysis, from sediments immediately underlying CP (1680 mg)	>46,300	-	OxA-32383
	VPI		197/106	17.3	bone	radius	Yes	retoucher	27.900	170	GrA-44701
	VPI		197/108	14.1	bone	long bone	Yes	retoucher	27.700	320	GrA-45450
	VPI		198/104	16.8	bone	humerus	Yes	retoucher	28.900	440	GrA-45482
	VPI		198/104	18.1	bone	long bone	Yes	Reindeer bone with cutmarks	26.010	120	GrA-44702
GER-75	VPI	GH12	197/93	118	bone	bone	No	Unmodified bone	> 45.000	+ 900/ - 700*	GrA-49116
GER-86	VPI	GH1	195/110	21.1	n.s.	n.s.	Yes	Aurignacian smoothener	29.690	+ 180/ - 170	GrA-49117
GER-88	VPI	GH9	197/091	21	Bone	Bone	No	Unmodified bone	30.090	+ 190/ - 180	GrA-49115
GER-90	VPI	GH 1	197/092	35.1	Bone	Bone	Yes	Pencil type bone point	30.660	+ 200/ - 180	GrA-49127
U-Th/ESR											
	VPI	GH 16-15			enamel	Teeth of herbivore	No		51.000 ± 3000 to 48.000 ± 3000		

Table 1a - Samples of known stratigraphy selected for radiometric dating. Sample ID indicates the entry number of the sample in the laboratory where it was dated; Context indicates the stratigraphic position of the sample, including square and number if available; Matherial characteristics and Comments give information on the artefact or ecofact from which the sample originates; Age is given in years BP (Before Present, where the present is conventionally set at 1950) with an error interval; the Lab Code is the name given to the date and it gives information on the laboratory where analyses were performed and the degree of confidence in the results.

Interestingly, the Tübingen team integrated their stratigraphic data with radiometric dates of artefacts collected during previous excavations. Therefore, the existence of levels of occupation that could not be verified stratigraphically was inferred through artefacts. In this way it has been proposed that a layer with Mousterian tools of Acheulian tradition was originally to be found in the cave. The hypothesis of an actual Acheulian level of occupation has also been advanced based on large bifacial elements found in the collections of the Musée Denon[84]. A Protoaurignacian layer was also tentatively inferred from the presence of Dufour bladelets and nuclei worked with crossed knapping technique[85]. Similarly, the presence of carinated pieces and a distinctive osseous industry (i.e. tongued piece, split-based point) among the artefacts collected and published from previous excavations, has been considered a strong indicator for the presence of a Classical Aurignacian layer in the Grotte de la Verpillière I, even though this horizon could not be found during recent excavations[86]. Some of the osseous artefacts collected from previous campaigns – deemed pertinent to the Aurignacian occupation based on their typological classification or for

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[84] Gros and Gros 2005.
[85] Wegeng and Floss 2016.
[86] On the carinated pieces cf. Floss et al. 2013c and 2015c; on artefacts made of bone, antler and ivory cf. Tartar and Heckel 2016, Floss et al. 2015c.

their technological characteristics – have been radiocarbon dated (GrA-49118, GrA-49120 to GrA-49122, and GrA-49248)[87]. They seem to date to around 32.000 BP: while this result seemed quite young to the investigators, the presence of an evolved Aurignacian occupation layer has been postulated for the presence of some, possibly diagnostic, bladelets[88]. Oxford Radiocarbon Laboratory analysed some samples, both from the excavated layers and from the collections of the Musée Denon. A selection of artefacts found in the early excavations of Grotte de la Verpillière I has been sampled and analysed by the author specifically for this doctoral dissertation. These samples are listed in Table 1b, with other samples of unknown stratigraphy. Analytical results (see Table 2) and further research paths are discussed in the next paragraph. To conclude, it should be mentioned that fragments of laurel-leaf blades have been found in the collections of the University of Lyon I with a 'Germolles' label on them: this finding prompted scholars to postulate the existence of a Solutrean layer in the cave[89].

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[87] Floss, Hoyer and Würschem 2016, 153-154. Table 1b provides a list of samples and results kindly provided by Harald Floss and the radiocarbon unit of the University of Gröningen.

[88]Chiotti 2003, Pesesse and Michel 2006.

[89] Floss, Hoyer and Würschem 2016, 154. However, the Grotte de la Verpillière I is not the only Palaeolithic site in Germolles to have a long history of studies, see Guillard 1920.

Sample ID	Context	Material characteristics			Comments	Age BP		Lab-code
	Cave	Material	Modified	Conserved	Comments	CRA	+/-	Sample
P42487	VP I	Antler	Yes, tongued piece	No	Tongued piece. Ink writing.	33,150	500	OxA-35114
P42488	VP I	Antler	Yes	No	Reworked shed antler. Shiny varnish around the inventory number.	30,800	390	OxA-35115
P42476	VP I	Bone	Yes, smoother	Yes	Smoother - two fragments glued together. Shiny surface. The sample was taken from a fracture that doesn't present traces of conservation.	27,180	250	OxA-35106
P42477	VP I	Bone	Yes, split-based point	Yes	Split-based point. Traces of glue on the reverse surface.	25,270	200	OxA-35107
P42479	VP I	Bone	Yes, smoother	Yes	Smoother. Shiny surface that peels off like a film.	29,400	360	OxA-35108
P42481	VP I	Bone	Yes, smoother	No	Smoother. Inventory number on the reverse surface.	25,200	200	OxA-35109
P42482	VP I	Bone	Yes, smoother	No	Fragment of smoother. Inventory number on the reverse surface.	30,650	380	OxA-35110
P42483	VP I	Bone	Yes, point	No	Fragment of point. White paint and shiny varnish around the inventory number.	33,100	500	OxA-35111
P42484	VP I	Bone	Yes, awl	Yes	Fragment of awl. White paint and shiny varnish around the inventory number.	34,350	600	OxA-35112
P42485	VP I	Ivory	Yes	Yes	Worked rod. Shiny varnish around the inventory number and in localized spots.	37,600	900	OxA-35113
GER-74	VP I stray find	Bone	Yes, point	n.s.	Aurignatian bone point	31.660	+ 210/ - 190	GrA-49122
GER-81	VPI	n.s.	Yes	n.s.	Aurignacian smoothener from Musée Denon	28.570	+ 50 /- 140	GrA-49248
GER-84	VPI	Bone	Yes, point	n.s.	non-diagnostic bone point from Musée Denon	32.130	+ 210/ - 200	GrA-49118
GER-79	VPI	bone	bone	Yes	Aurignacian bone point from Musée Denon	31.490	+ 200/ - 190	GrA-49121
GER-80	VPI	bone	bone	Yes	Aurignacian bone tool from Musée Denon	30.290	+ 190/ - 170	GrA-49120

Table 1b – Samples of unknown stratigraphic origin selected for radiocarbon dating. Sample ID indicates the entry number of the sample in the laboratory where it was dated; Context indicates the site where the sample was found; Matherial characteristics and Comments give information on the artefact or ecofact from which the sample originates; Age is given in years BP (Before Present, where the present is conventionally set at 1950) with an error interval; the Lab Code is the name given to the date and it gives information on the laboratory where analyses were performed and the degree of confidence in the results.

OxA	P number	PCode	Radiocarbon date BP	±	Material	Species	Used	Yield	%Yld	%C	δ13C (‰)	δ15N (‰)	CN
32228	38849	AF	>44300		Bone	Bos/Bison	600	6.75	1.1	41.3	-19.7	9.2	3.4
32231	38854	AF	>46700		ivory	Elephantidae	550	33.54	6.1	42.4	-20.7	11.4	3.4
32232	38856	AF	34,850	650	ivory	Elephantidae	370	5.9	1.6	41.4	-20.5	12.1	3.3
32233	38857	AF	>46800		ivory	Elephantidae	360	25.27	7	42.1	-20.6	12.2	3.3
32234	38862	AF	>49900		bone	Bos/bison	620	14.95	2.4	42.3	-20.1	4.8	3.4
32235	38866	AF	49600	3900	bone	Bos/Bison	590	39.45	6.7	43.6	-20.0	7.0	3.3
32383	38861	AF	>46300		bone	Bos/Bison	610	40.32	6.6	42.6	-20.1	5.4	3.3
35106	42476	AF*	27180	250	bone	Elephantidae	600	21.02	3.5	42.8	-21.3	8.1	3.2
35107	42477	AF*	25270	200	bone	Not identified	700	18.26	2.6	42.3	-18.6	4.1	3.3
35108	42479	AF*	29400	360	bone	Elephantidae	550	15.07	2.7	42.7	-21.3	9.2	3.2
35109	42481	AF*	25,200	200	bone	Elephantidae	880	7.35	0.8	42	-20.9	9.3	3.2
35110	42482	AF*	30650	380	bone	Elephantidae	760	8.12	1.1	41.6	-21.2	8.7	3.2
35111	42483	AF*	33,100	500	bone	Ovis/Capra	780	18.61	2.4	43.2	-18.4	5.5	3.3
35112	42484	AF*	34,350	600	bone	Equus sp.	1000	43.66	4.4	42	-20.2	9.7	3.2
35113	42485	AF*	37,600	900	ivory	Elephantidae	690	5.98	0.9	43.2	-21.4	8.9	3.2
35114	42487	AF*	33,150	500	antler	Ovis/Capra	670	20.77	3.1	42.3	-19.4	7.1	3.2
35115	42488	AF*	30,800	390	antler	Not identified	840	34.3	4.1	43.7	-18.0	4.1	3.3

Table 2 – Radiocarbon AMS dates and associated analytical data from the Germolles site dated in Oxford. OxA indicates the lab code of the date and P number the ID assigned to the sample upon entrance in the lab; Pcode is the chemical treatment applied (cf. Brock et al. 2010) and * denotes a solvent wash. Radiocarbon age BP is the conventional radiocarbon age, expressed in years BP (Before Present) with the present conventionally set at 1950 AD. Stable isotope ratios are expressed in ‰ relative to vPDB with a mass spectrometric precision of ±0.2‰ for C and ±0.3‰ for N. Yield represents the weight of ultrafiltered collagen in milligrams. %Yld is the percent yield of extracted collagen as a function of the starting weight of the bone analysed ("Used" also in mg). %C is the carbon present in the combusted gelatin. CN is the atomic ratio of carbon to nitrogen and is acceptable if it ranges between 2.9—3.5.

1.1.7 Ten new radiocarbon dates at ORAU[90]

A total of thirty-one samples from Germolles have been analysed at ORAU (Oxford Radiocarbon Accelerator Unit) in 2016. The first set of samples came from the Floss excavations (P38849-66) (Table 1a). These samples were taken both from Grotte de la Verpillière I and Grotte de la Verpillière II, an intact site that was found by the Tübingen team close by the first cave[91]. The second set (thirteen samples) were selected from the collections of the Musée Denon (Chalon-sur-Saône, France) and were analysed by the author of this dissertation[92]. The descriptions of the ten samples that yielded sufficient collagen for dating are included in Table 1b. They were excavated in Grotte de la Verpillière I before the Tübingen mission, and stratigraphic data are not available. Three of the samples (42477, 42487, 42488) are considered chronological indicators, as they are index fossils for Aurignacian technocomplexes. Most of the samples were heavily conserved. The time and method of

[90] This paragraph is a reworked version of a report written by the author and Tom Higham as a report for the radiocarbon dating of the mentioned samples, performed in the ORAU laboratory in Oxford.
[91] Preliminary results of the ongoing excavation at Grotte de la Verpillière II can be found in Frick 2015.
[92] Substantial help, training and supervision was provided by the researchers and personnel of ORAU, especially Tom Higham, Rachel Hopkins, Daniel Comeskey and David Chivall, whom I must thank greatly.

conservation remains unknown. This means that radiocarbon dates may underestimate the true age of the dated samples, unless more rigorous pre-treatment methods are applied that successfully remove conservation derived contaminants[93].

Pre-treatment methods and other analytical data are shown in Table 2. AF denotes the ORAU ultrafiltration method whilst AF* indicates an additional solvent extraction prior to the collagen preparation. All the Musée Denon samples were subject to a solvent wash, ABA pre-treatment, gelatinisation and ultrafiltration following the current standard ultrafiltration protocol of ORAU Laboratory[94]. Solvent washing is a procedure used when samples have been conserved with unknown chemicals. The efficacy of this protocol (acetone/methanol/chloroform washes) has been experimentally tested: this method effectively removes aged shellac and Paraloid, while results are not so reassuring for samples treated with vinyl acetate-derived polymers and cellulose nitrate lacquers[95]. Pre-treatment was performed according to ORAU routine protocol. Three samples failed as a result of low collagen yield (i.e. after the pre-treatment, the isolated collagen was not sufficient for measurement). Of the measured analytical data, most results were close to

[93] E.g. single amino acid dating, cf. Devièse et al. 2018.
[94] For a description of ORAU's current protocols cf. Brock et al. 2010.
[95] Brock et al. 2018.

expected values. The CN atomic ratios were within the range of 2.9 and 3.5 accepted for quality control purposes, though three samples had a value of 3.4, which is considered slightly elevated (theoretical value is 3.18). The collagen yields were good throughout with the obvious exception of the samples that failed.

One unexpected result came from a split-base point, an index fossil of the Aurignacian (P42477), whose date was much younger than expected. During sampling we observed that the artefact was heavily conserved. It is very difficult to provide *proof* of absence of contamination and we cannot demonstrate with certainty that all contaminants have been successfully removed. However, for this sample, we decided to look for *evidence* of absence. The collagen extracted from the sample was sent to ICVBC Laboratory of the CNR in Pisa (Italy). It was analysed through pyrolysis Gas Chromatography/Mass Spectrometry (pyGC/MS), checking the resultant spectra against a library of common contaminants. While the technicians in Pisa informed us that they could not find any trace of known contaminants[96], an official report is not available yet.

The lack of information on the archaeological context for the samples taken from the Musée Denon makes it difficult to build a statistical model to spot outliers, which would have

[96] Diletta Querci, pers. comm.

been useful with such heavily conserved materials. In general, dates from conserved and de-contextualized artefacts always require a degree of caution (cf. Appendix I). However, the thorough application of standard procedures increases their overall reliability, especially when the target event is the production of the artefact, instead of the occupation layer. For dates that appear dubious or inconsistent with expectations, it would be possible to test the results by taking a new sample from the artefacts and dating them with a more robust method: in November 2018 a new sample has been taken from P42476, P42477 and P42479 to be dated with single amino-acid method[97]. The hydroxyproline dating of the split-based point gave a much older date than the previous one:

OxA-38321

δ13C=-23.08

34810 ± 590 BP

Because of the lack of stratigraphic information from the site, we have used a novel KDE (Kernel Density Estimate) method[98] to calibrate all the results obtained at the ORAU Laboratory in Oxford. The data is shown in Figure 9. They show the relative probability densities for all of the ORAU

[97] Devièse et al. 2018.
[98] Bronk Ramsey 2017.

collagen dates, calibrated and plotted. The 'greater than' ages are not included. One can see the distribution fits from ~43,000 cal BP, with a significant distribution from 40-35,000 cal BP and one or two later spikes in the data which correspond to the small number of recent results. In order to determine the significance of the plot it would be necessary to determine whether or not these potential outliers are in fact outliers.

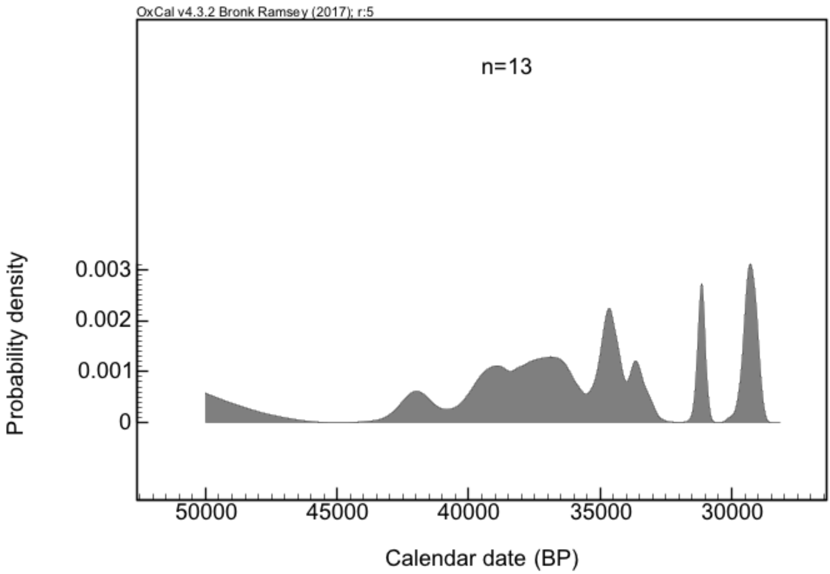

Figure 9 - KDE model for the Germolles data, comprising both museum and archaeological samples from both sites. Author: Tom Higham

The last few pages showed how the chronology of Grotte de la Verpillière I has been the object of intense discussion among scholars for 150 years. But why is it so relevant and what lies behind this quarrel?

1. 2 Analysing the debate

Why is the chronology of Grotte de la Verpillière I so controversial? And why has it drawn so much attention for over 150 years? The intuitive answer to both questions is the lack of a reliable chronostratigraphic sequence[99]. The second, however, is not that trivial. Most investigators highlight the importance of the site for the understanding of the Middle to Upper Palaeolithic transition at the crossroad between eastern and western Europe[100]. While this transition – or 'biocultural shift' – largely resists a univocal definition because of its complex nature, I shall briefly describe the problems it poses for the sake of clarity. The transition refers

[99] This is the case for many European sites crucial to the understanding of the Late Middle Palaeolithic (LMP) to Early Upper Palaeolithic (EUP) biocultural shift. Like Grotte de la Verpillière I, many sites were the object of several excavation campaigns in the XIX or early XX century and only few of those campaigns resulted in a publication or written records of any kind (drawings, maps, excavations' journals). Most often, chronostratigraphic information are reconstructions derived from a combination of archival documents and museum collections (e.g. Szmidt, Brou and Jaccottey 2010 on Trou de la Mère Clochette; Zilhão et al. 2007 on Grotte des Fées, Châtelperron).

[100] Floss, Hoyer and Würschem 2016.

to the following phenomenon: Upper Palaeolithic industries, specifically the Aurignacian, come to substitute the old Mousterian ones all over Europe, with a spatiotemporal distribution still to be determined[101]. Along this variation in archaeological assemblages, a biological shift becomes apparent: Neanderthals leave way to Modern Humans[102]. The traditional, migrationist, hypothesis would equate the new industries with the new human groups. They would identify two main trajectories (south to north, and east to west) for the 'colonization' of Europe[103]. To complicate the picture, the so called 'Transitional Industries' have been identified as the meeting point of the two cultures – or technocomplexes – in different areas: the Szeletian in eastern Europe, the Uluzzian in Italy and Greece, and the Châtelperronian in France[104]. On the one hand, their mixed characteristics pose a serious threat to the automatic association of people and material cultures, so that the notion of acculturation has been called to the rescue. On the other hand, their appearance and geographical distribution

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101 Cf. Davies 2007, 269-272: spatiotemporal patterns might give an indication on the population dynamics over the Middle to Upper Palaeolithic biocultural shift.
102 Mellars 2006.
103 Cf. Davies 2007 for an accurate analysis of this idea and competing hypotheses.
104 For a more extensive account of the distribution of these industries and of their relationship with previous and later technocomplexes cf. Soressi and Roussel 2014, Riel-Salvatore 2009, Allsworth-Jones 2004.

has sometimes been used to map the trajectory of newcomers[105]. To that end, another issue recently raised to the headlines in the Middle to Upper Palaeolithic transition debate: assemblages were discovered all around Europe that – though clearly belonging to the Aurignacian group – were poor in osseous industry and characterised by a limited variety in tools' shape and raw materials. Following a 'simple to complex' evolutionistic paradigm, they were called Protoaurignacian[106]. At the same time, the classical division of the Aurignacian in five phases – constructed by Peyrony in 1933 for southwestern France and then extended to all of Europe – was disproven by new findings, as artefacts thought to be index fossils for those five phases were found conjointly[107]. The absence of reliable and sufficiently dense data on the spatiotemporal distribution of key industries (i.e. Protoaurignacian, Aurignacian, Châtelperronian) leaves the question of their synchronicity and their distribution in a relative chronology unanswered[108]. Moreover, the uncertainty of the association

[105] A very heated debate surrounds the hypothesis that Châtelperronian industries are the results of acculturated Neanderthal groups (cf. Harrold 1988, Pelegrin 1995, D'Errico et al. 1998, Zilhão 2001).
[106] Benazzi et al. 2015; Conard and Bolus 2015.
[107]Cf. Clark and Riel-Salvatore 2005 with extensive bibliography.
[108] This problem has been the subject of a large number of publications, but a good synopsis of the issue, including multiple sometimes opposite perspectives, can be found in the collective volume edited by Zilhão and D'Errico (2003).

of human remains with Palaeolithic industries – and in some cases the uncertainty surrounding the attribution of rare and fragmented human remains to either Neanderthals or Modern Humans – heavily contributes to the complexity of the problem[109].

In this context, Grotte de la Verpillière I is a crucial site whose accurate dating would strongly impact the mosaic of the Middle to Upper Palaeolithic transition at the crossroad between the Jura region and southwestern France. Indeed, Germolles is still the easternmost site to present a Châtelperronian facies[110]. Moreover, it seems to have had several Aurignacian occupation levels and a Protoaurignacian assemblage has been tentatively reconstructed[111]. Establishing the chronostratigraphic relationship between these technocomplexes – and the internal articulation of the Aurignacian itself – is therefore the main preoccupation of investigators[112].

It seems apparent that the debate on Germolles tackles at least three more general issues, which both influence and are influenced by its chronology. First and foremost,

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[109] On human remains cf. Gravina et al. 2018; the use of ZooMS (ZooArchaeology by Mass Spectrometry) for the PalaeoChron Project in Oxford yielded promising results for the identification and attribution of human remains (cf. Slon et al. 2018).
[110] Cf. Floss, Hoyer and Würschem 2016.
[111] *Vide supra* p. 53.
[112] Dutkiewicz and Floss 2015, 30-31.

chronological determinations are relevant to understanding the relationship between Neanderthals and Modern Humans – pacific, aggressive, of acculturation, of competition for natural resources – and the dynamics by which the latter replaced the former. General ideas about the nature of humankind are inevitably linked to this debate and they seem to highly impact it. Moreover, these chronological problems tackle, in a somewhat less explicit manner, at least two core principles of archaeological investigation and its epistemological system. The first one is the notion that from the artefacts found on the ground we can build categories – 'technocomplexes' and 'cultures', e.g. Châtelperronian, Aurignacian, or Uluzzian – that act as indicators of a human people, or race, or another kind of social/biological group. The second one is the practice of dividing and identifying these categories through index fossils or index technologies: the long lasting and intense debate on split-based points is a good example of this problem, and one that is of concern for interpreting Grotte de la Verpillière I.

1.2.1 Neanderthals and Modern Humans

Since the beginning of the last excavation campaign in 2005, several radiometric dates have obtained from Grotte de la Verpillière I. Some samples were selected among organic artefacts found *in situ* during the most recent excavations (Table 1a). Others were singled out amongst typologically perspicuous artefacts from the collections of the Musée Denon that were originally unearthed during old excavations at the site (Table 1b). Results obtained on the latter raised concerns regarding the unknown chemicals used to conserve the samples and the reliability of their stratigraphic association (tentatively reconstructed from excavation journals, maps, drawings, and old publications). This is particularly unfortunate as the 'target event' for the dating campaign (i.e. the event that the investigators want to date) is the arrival of Modern Humans or – as some would prefer to phrase it – the chronological articulation of the Middle to Upper Palaeolithic biocultural shift in the area[113]. Instead, the 'dated events' (i.e. the events dated by the methods employed) are the deaths of animals, whose bone or antler was used by humans to make tools[114]. Those tools, in turn, are used as proxies for occupational phases in the cave and, by extension, in the region. Petrographic analyses

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[113] Harald Floss, personal communication 06/11/2018; cf. Heckel et al. 2016.

[114] More accurately, radiocarbon dating measures the moment when the exchange of carbon between the organism and the atmosphere ceased. This might affect the 'dated event' – 'target event' interval: in the case of wood, for example, external tree-rings stopped exchanging carbon with the atmosphere several years, and even centuries, before the death of the tree. A sufficiently detailed report of radiocarbon dating basic assumptions is provided in Appendix I.

of lithic material show that several exchanges occurred between Germolles and south-eastern Germany[115]: chronometric data would help assess the hypothesis of an east to west trajectory of Modern Human migration, following the rivers as a guide for movement. While it could be suggested for an error bar to account for the distance between target event and dated event[116], the main objective of this paragraph is to highlight the reasons that lie behind the selection of the target event itself. If 'all history is contemporary history', it is mostly because the questions we ask of our evidence are influenced by our present[117]. Therefore, these questions are likely to reveal what David L. Clarke would have called 'latent theory' and/or 'controlling models', two concepts that will be crucial to several aspects of this dissertation. In his words:

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[115] Frick 2016.

[116] *Vide infra,* Appendix fig. 5.

[117] The impact of present concerns on our understanding of the past has been the object of several theoretical reflections, coming from otherwise very different schools of thought, e.g. Benedetto Croce (1938, the notorious quote can be found on p.5) to Marc Bloch (1998, 29-39), to Collingwood (1946) and the postmodern literature (e.g. Foucault 1966, White 1973). Several essays have also been written on ideological approaches that have influenced certain archaeological disciplines over time (e.g. Barbanera 1998, 125-154 on the fascist interpretation of the Roman Empire; De Francesco 2013 and Harari 2015 and 2012 on the impact of Italian nationalistic ideology on Etruscan studies).

> Hypotheses are generated from the model expression of a theory.
> Explanation comes from tested hypotheses.
> Hypotheses are tested by using relevant analyses on meaningful categories of data[118].

In his opinion, archaeological hypotheses depend on (latent or explicit) theories and on models used to represent those theories. Some models are operated by archaeologists to interpret data: spatial distribution analysis, radiocarbon calibration, Harris matrix, etc. Clarke calls them 'operational models'. Others are dependent on education, disciplinary conventions, historical and intellectual conditions under which investigators operate: they can be unconscious and are called 'controlling models' as they affect scholars' approaches, research questions and the selection of instruments deemed adequate to analyse data[119]. Identifying those models would be helpful to answer questions about ourselves and our perception of the past. Moreover, this exercise in self-awareness could possibly help us escape the control of the models themselves.

The chronological articulation of the Middle to Upper Palaeolithic transition has been the subject of several studies

[118] Clarke 1972, 3.
[119] Clarke 1972, 5-11.

published over more than 150 years[120]. They all have a common denominator: they aim to identify tipping points – or lack thereof – that may constitute the boundaries of cultural and temporal units. In doing so, they are implicitly affirming or denying the idea that there are moments in time when every aspect of life (society, population, culture, arts, etc.) is altered simultaneously[121]. For the period at hand, the crucial question underlying chronological enquiries is the following: is there a moment in time when everything changes and humanity as we know it is born? Chronological assessments seem to be part of a larger problem: what makes us humans? Are we inherently different from animals? If that is the case, how and when did that happen?[122] Many answers have been provided to those questions and most of them are relevant to the discussion on the authorship of the Aurignacian and of Transitional industries.

Indeed, certain aspects of the material record associated with Modern Humans have been the subject of long-standing

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[120] Several volumes have been published in 2011 to celebrate one hundred and fifty years from the discovery of the first Neanderthal fossils (cf. Conard and Richter 2011, Clark Howell, Condemi, and Weniger 2011); the ERC PalaeoChron Project at the ORAU (University of Oxford) has brought a lot of new data, that are being published and modelled to provide a spatiotemporal distribution.

[121] On periodisation see Pare 2008.

[122] Cf. Zilhão 2001 on the various way in which our present questions and concepts affected both the academic and the popular image of Neanderthals.

debates. While a full analysis would be out of scope, a brief mention of these disputes might help recognise the extremely complex questions that lay behind certain chronological endeavours.

The question of the characteristic(s) separating Modern Humans from Neanderthals has been addressed in several ways. One answer refers to art and, more generally, to symbolic behaviour[123]. Only few months ago, the journal *Science* issued an article on U-Th dating of calcium carbonate that formed on top of paintings in three different caves of Spain, establishing a *terminus ante quem* of 65.000 years BP for the murals[124]. Given that the arrival of Modern Humans doesn't seem to predate 45.000 years BP, the paintings should be the result of Neanderthals' activity. While it has been largely acknowledged that Neanderthals showed symbolic behaviour[125], the idea that they could produce actual figurative images has been strongly criticised – along with

[123] The discussion on the topic is extremely complex and starts with the very definition of art, ranging to its value as an indicator of a linguistic brain and/or of symbolic behaviour and structured society (cf. Zilhão 2001, 31 -42.

[124] Hoffmann et al. 2018.

[125] Cf. D'Errico et al. 2003 and the rather heated discussion on the interpretation of dates from ornaments found in the Châtelperronian layers of Grotte du Renne: Higham et al. 2010, Caron et al. 2011, Higham et al. 2011.

Hoffmann's analytic methods – by an impressive number of scholars[126].

A second stream of research focuses on the relationship between biological modernity and certain cognitive abilities.[127] Which abilities exactly? Recently researchers have focussed on learning capacity. The ability to learn from other than experience (i.e. abstraction) has been postulated to be one aspect where Modern Humans were at an advantage[128]. Similarly, the study of Neanderthals' learning behaviour has led to believe that Modern Humans were at an advantage in producing innovation and creative thinking.[129] The two issues of symbolic/artistic behaviour and learning/innovative thinking are closely connected, as art might be considered the

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[126] Slimack et al. 2018; another letter has been drafted and waits to be published: it has been signed by more than 50 scholars (H. Floss, pers. comm.).

[127] Several disciplines are involved in this endeavor: among others, theory of mind (cf. the emblematic book by Wynn and Coolidge 2012), DNA studies (most recentrly Namba et al. 2020 and the interesting comments of Hevner 2020 on the genes responsible for cerebral cortex expansion) and cognitive anthropology (recently Wynn and Coolidge 2019).

[128] The article published by Klein et al. (2003) on *Science* appears emblematic of certain cognitive approaches to Neanderthals' bio-history. Zilhão 2001 calls it the 'blame it on the victim' attitude: according to this theory, reduced cognitive abilities are the reason for Neanderthal extinction.

[129] On the topic see the collective volume edited by Nishiaki and Jöris (2019, with extensive bibliography) and especially Wynn and Coolidge 2019.

tangible trace of a creative and inductive mind[130]. A third element that is commonly taken to separate Neanderthals from Modern Humans is their social behaviour: a recent genetic research, for example, has linked Neanderthal DNA with Autism[131]. Implicitly, this would attribute to Modern Humans the exclusive development of modern social behaviour.

The search for one (or more than one) inherent quality that defines 'modern humanness' enters the broader problem of anthropocentrism[132]: are humans the most relevant living beings on the planet? Are we 'more adapt' than any other organism? Is the evolutionary process progressive? Or is it rhizomatic?[133] And if that is the case, is humanity just one branch of the coral, same as any other species?

Some of these considerations might silently enter archaeological debates on the Middle to Upper Palaeolithic transition as 'latent theory'. Indeed, the interest of funding bodies, academic communities and the public mostly refers to

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130 Cf. Mithen 2001, or Burke 2010; Mellars et al. 2007 edited a very extensive volume on Neanderthals behaviour.

131 Cf. Oksemberg et al. 2013 on autism genes, Johansson 2013 on Neaderthals proto-language, *contra* Barceló-Coblijn and Benítez-Burraco 2013.

132 While a comprehensive discussion of anthropocentrism in philosophy and in the history of historiography could be the topic of a dissertation by its own right, an updated summary of the problem and a thoughtfully selected bibliography can be found in Domanska 2010.

133 Bredekamp 2008.

these general questions on human nature. For example, scholars debating the possibility of prolonged contacts – and even acculturation and mating – between Neanderthals and Modern Humans have sometimes shown very strong opinions that may have been affected by the abovementioned theoretical positions[134]. Evidence of Neanderthal's DNA surviving in our own[135] and the recent discovery of a child that had one Neanderthal and one Denisovan parent[136] prove that different human races came into contact and interbred. However, there are scholars who would insistently deny the possibility of even a partial influence being exerted on Châtelperronian industries by Aurignacian people, or vice-versa[137]. These authors would see Modern Humans as an exogenous and homogeneous group – manifested in the Aurignacian facies – that entered Europe after its formation somewhere (and sometime) else. It should be noted that geography plays a fundamental role in the debate: chronological determinations are spatiotemporal coordinates, to be conceived of in four dimensions.

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[134] On this debate *vide supra,* p. 37 footnote 56.

[135] Cf. Sankararaman et al. 2012, Vernot and Akey 2015, among others.

[136] Slon et al. 2018.

[137] Zilhão 2001, p. 42-54; Mellars et al. 2007 strongly criticise the point of view of Zilhão and D'Errico and their 'short chronology' (cf. Zilhão and d'Errico, 1999) as they ascribe it a 'theoretical agenda' that denies the mixing and mating of different human species.

1.2.2 The Aurignacian: unpacking a conceptual unit

Current and old studies on the chronological articulation of the Middle to Upper Palaeolithic transition can be (and have been) intertwined with fundamental questions about the social and biological features that distinguish Modern Humans from Neanderthals[138]. This claim is not intended to undermine the research that has been done in this area. On the contrary, if anything, it is the reason why it is so interesting. The following comments should therefore not be mistaken for a sceptical critique arguing against the possibility of acquiring knowledge on the deep past[139]. Archaeologists use heuristic tools such as 'technocomplexes' (Châtelperronian, Uluzzian, Aurignacian), phases (ProtoAurignacian, Early Aurignacian, Late Aurignacian) and index fossils (split-base points, carinated pieces), whose boundaries and definitions are to some extent arbitrary[140]. The arbitrariness of chronological

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[138] *Vide supra* pp. 69-72.

[139] "Lo storico lavora sul presupposto di essere capace di ricostruire e capire i fatti del passato. Se un epistemologo riesce a convincerlo del contrario, lo storico deve cambiare mestiere. Se un epistemologo gli dimostra limiti invalicabili della conoscenza (per esempio che non si possono conoscere le intenzioni o che esiste solo la probabilità e non la certezza), lo storico dovrà tenerne certo conto, ma solo per definire più rigorosamente i limiti della sua ricerca" (Momigliano 1987, 15-16).

[140] The debate on typology has been particularly intense since the 1980s: the arbitrariness in the definition of types has been duly highlighted, strongly criticising those who use types as 'true' entities (cf. Miller 1985; Sørensen 1997, 2015); others suggest using technological skills or habits

boundaries has been acknowledged by the founding father of prehistoric periodization, at the very birth of the discipline. To his critics, Gabriel De Mortillet responds that, while this may affect periods' precision, it does not hinder their accuracy:

> Les adversaires de la paléoethnologie, comprenant qu'une bonne classification assoit la science nouvelle sur une base des plus solides, contestent la possibilité d'en établir une. Suivant eux, il n'existe pas des divisions sérieuses. Non seulement il y a des passages et des transitions entre toutes les divisions, mais encore et surtout elles s'enchevêtrent ; elles ne sont pas synchroniques dans les divers pays ; elles sont plus ou moins longues, suivant les régions. Tout cela est très vrai, mais ces objections n'en sont pas moins sans valeur. Pour le démontrer, il suffira d'un exemple. Qu'y a-t-il de plus différent, de plus tranché, de plus facile à caractériser et à reconnaître que le jour et la nuit ? Eh bien, l'argumentation des adversaires de la classification préhistorique, si elle avait quelque

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as chronological indicators in place of index fossils (cf. Arrizabalaga Valbuena and Maíllo-Fernández 2008 for the Aurignacian); periodisation has been the object of fierce critique in several disciplines (e.g. Pare 2008 with bibliography).

valeur, conduirait à conclure que le jour et la nuit n'existent pas!...

En effet, entre le jour et la nuit, il y a des transitions, des passages plus ou moins longues, le crépuscule et l'aurore. Le jour et la nuit, au lieu d'être synchroniques, s'enchevêtrent suivant les régions et arrivent même à être diamétralement opposés. Leur longueur est très variable tandis qu'elle est en moyenne de douze heures chez nous, elle est de plusieurs mois vers le pôle. Et pourtant, la division du temps en jours et en nuits est très nette, très précise, très pratique. Il en est exactement de même de la division du préhistorique en âges, périodes et époques[141].

G. De Mortillet seems to believe that his subdivisions are, literally, 'as real as night and day'. Only their boundaries are an approximation. Nowadays most scholars would rather claim that these periods are not 'real' *per se*, but they are conventional groupings used by investigators to make sense of empiric evidence, coming closer to a probabilistic truth[142].

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[141] De Mortillet, 1885, 20-22.

[142] Atoms can be described as particles or as waves and it doesn't mean they are neither nor both. Similarly, we can't grasp the complexity of empirical evidence in archaeology in its entirety, so we describe it through generalizations such as periods, or types, or cultures. Cf. at least Clarke 1972, Gardin 1990.

In both views, concepts such as periods, cultures and types are generalisations inferred from empirical data. They can be called *conceptual units*[143]. These groups are built by analogy: members of the same taxon share a certain number of characteristics with each other. The boundaries and definitions of these groups are to some extent arbitrary, so that their validity might at times be questioned. The Aurignacian technocomplex, for example, is "an otherwise disparate group of cultures considered to share certain general similarities in technology and artefact type"[144]. The word 'cultures' in this definition prompts another consideration: it is often implicit that 'material cultures' correspond to human groups. The Aurignacian is deemed to be the material manifestation of Modern Human colonists, or the result of an acculturation of Modern Humans already living in the area, or the production of a human group that included both Neanderthals and Modern Humans[145]. While the 'pots are not people' caveat has been a mantra for most archaeologists in the last few decades, it embodies an epistemological dilemma in archaeological research: our interest focuses on people (ethnic, social,

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[143] I adopt here the definition given by Ramenofsky and Steffen 1998, p.5; O'Brien and Lyman (2002, 21-22) call them 'ideational units'.
[144] Oxford Dictionary, s.v. *Technocomplex.*
[145] These three hypotheses and their implications are largely discussed in Davies 2007. He explains that data at our disposal are not sufficient nor well collected and we cannot decide among these hypotheses. Some of his conclusions are still true to this day, but the PalaeoChron ERC Project will probably be crucial to approaching a resolution.

national, religious and human groups of any kind) but we can only observe material evidence (artefacts, deposits, palaeobotanic residues, etc.). Several inferences need to be made to link the latter to the former[146]: Table 3 shows how radiometric dates obtained on samples from Grotte de la Verpillière I are connected to technocomplexes through typology or/and stratigraphy.

Sample ID	Lab code	Provenance		Material characteristics		Dating method	Age B.P.		Periodisation
		GH	Collection	Material	Typology		Age	+/-	
		16-15	-	Teeth of herbivore	No	U-Th/ ESR	51,000 ± 3,000 to 48,000 ± 3,000		Mousterian
P38854	OxA-32231	15	-	Ivory flake	No	^{14}C	>46,700		Mousterian
P38857	OxA-32233	15	-	Ivory fragment	No	^{14}C	>46,800		Mousterian
P38862	OxA-32234	41c	-	Long bone	No	^{14}C	>49,900		Mousterian
P38866	OxA-32235	40	-	Chewed bone	No	^{14}C	49,600	3,900	Châtelperronian
P38849	OxA-32228	24	-	Bone	No	^{14}C	>44,300		Ancient Upper Palaeolithic with Aurignacian affinities
P38861	OxA-32383	41b	-	Bone	No	^{14}C	>46,300		Mousterian
GER-80	GrA-49120	-	Musée Denon	Bone	Tool	^{14}C	30,290	+ 190/ - 170	Aurignacian
GER-79	GrA-49121	-	Musée Denon	Bone	Point	^{14}C	31,490	+ 200/ - 190	Aurignacian
GER-74	GrA-49122	-	Musée Denon	Bone	Point	^{14}C	31,660	+ 210/ - 190	Aurignacian
GER-81	GrA-49248	-	Musée Denon	n.s.	Smoothener	^{14}C	28,570	+ 50 / - 140	Aurignacian
P42476	OxA-35106	-	Musée Denon	Bone	Smoothener	^{14}C	27,180	250	Aurignacian/Gravettian
P42477	OxA-35107	-	Musée Denon	Bone	Split-based point	^{14}C	25,270	200	Typical Aurignacian
P42479	OxA-35108	-	Musée Denon	Bone	Smoother	^{14}C	29,400	360	Aurignacian/Gravettian
P42481	OxA-35109	-	Musée Denon	Bone	Smoother	^{14}C	25,200	200	Aurignacian/Gravettian
P42482	OxA-35110	-	Musée Denon	Bone	Smoother	^{14}C	30,650	380	Aurignacian/Gravettian
P42483	OxA-35111	-	Musée Denon	Bone	Point	^{14}C	33,100	500	Aurignacian/Gravettian
P42484	OxA-35112	-	Musée Denon	Bone	Awl	^{14}C	34,350	600	Aurignacian/Gravettian
P42485	OxA-35113	-	Musée Denon	Ivory	No	^{14}C	37,600	900	Aurignacian/Gravettian
P42487	OxA-35114	-	Musée Denon	Antler	Tongued piece	^{14}C	33,150	500	Typical Aurignacian

[146] Among others, cf. Carandini, 1991, pp. 135-143 and 149-169.

P42488	OxA-35115	-	Musée Denon	Antler	No	^{14}C	30,800	390	Aurignacian/Gravettian
	GrA-49117	GH1	-	n.s.	Smoothener	^{14}C	29.690	+ 180/ - 170	Aurignacian
	GrA-49127	GH1	-	Bone	Pencil type point	^{14}C	30.660	+ 200/ - 180	Aurignacian
	GrA-44701	Out of the cave	-	Bone	Retoucher	^{14}C	27.900	170	Gravettian
	GrA-45450	Out of the cave	-	Bone	Retoucher	^{14}C	27.700	320	Gravettian
	GrA-45482	Out of the cave	-	Bone	Retoucher	^{14}C	28.900	440	Gravettian
	GrA-44702	Out of the cave	-	Bone	No	^{14}C	26.010	120	Gravettian

Table 3 – Radiocarbon dates and their connection with conceptual units. Sample ID refers to the entry number of samples in the laboratory; Lab Code is the code assigned to the date; a distinction is made between samples of known and unknown stratigraphic provenance; Typology refers to the possibility that samples can be placed in certain typological taxa; the last column indicates the period with which the sample is associated stratigraphically or typologically.

Artefacts are linked to deposits and deposits to human groups. We can date artefacts with ^{14}C, U-Th, TL and several other methods[147]. But, depending on the method, we date a certain moment of the life of an artefact, and there is an interval between that moment and its deposition[148]. We can date deposits through OSL, which unfortunately comes with a large error bar[149]. The chronology of conceptual units such as cultures and technocomplexes can only be inferred from data on artefacts and deposits, specifically from those that contribute to the definition of the unit itself.

For example, the chronology of the Aurignacian has been variously established depending on the features that, according to different authors, define the technocomplex (fig.

[147] For a beginners' explanation of those methods, their strengths and weakness cf. Malainey 2011, 91-168.

[148] This interval is called *inbuilt age* cf. Appendix I.

[149] Malainey 2011, 127-140.

10)[150]. The classical construction of the Aurignacian as a homogeneous culture divisible in five neat periods has been largely dismissed[151]. The validity and usefulness of the Aurignacian as a heuristic tool has been questioned, both in relation to specific areas[152] and in general[153]. Those claims are usually supported with a demonstration of the extreme variability in the contexts we call Aurignacians[154]. However, when it is recognised that 'cultures' or, in this case, technocomplexes are generalisations from empirical instances, one must admit that they need to include some variability. As D. Clarke would say, they need to be treated as polythetic groups, not as nomothetic ones[155]. To construct a technocomplex (e.g. the Aurignacian), one establishes the features that characterise it (e.g. bladelets, carinated pieces, osseous industry, ornaments, etc.). Then one assigns a certain value to each one: some are necessary and sufficient, some are necessary but not sufficient, some are sufficient but not necessary. Some others are neither necessary nor sufficient to

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150 Cf. Clark and Riel-Salvatore 2005 for a schematic definition of the Aurignacian identifying features.
151 The classical division was constructed by Peyrony (1933) for Southwestern France and then extended to Europe and the middle East; this classification has been shown to be erroneous (cf. Davies 2007, 205; Clark and Riel-Salvatore 2005).
152 Goring- Morris and Belfer-Cohen 2006.
153 Clark and Riel-Salvatore 2005.
154 *Ibidem*.
155 Clarke 1968, 38-40.

assign a certain context (e.g. Grotte de la Verpillière I, GH 15c) to a certain technocomplex (e.g. the Aurignacian). However, the occurrence of several of the latter in the same context may be sufficient evidence for an attribution[156]. Clearly the definition of such features and their validity for the identification of the technocomplex is a complex matter that is constantly under revision. For the Aurignacian, it seems that the appearance of osseous industries and ornaments are very general features that tend to clearly distinguish it from previous industries (cf. Fig.10).

AURIGNACIAN TYPOLOGICAL DIAGNOSTICS

- carinated, keeled and 'nosed' endscrapers
- ordinary endscrapers made on blades (usually) or flakes (less commonly)
- big blades with invasive, scalar retouch (Aurignacian, strangled blades)
- busqué and/or Vachons type burins
- Dufour bladelets, Font-Yves/Krems-type retouched bladelets
- split-based bone 'points'
- lozenge-shaped, biconical points in ivory, antler and bone

Table 1. Aurignacian Typological Diagnostics.

NON-UTILITARIAN INDICATORS OF 'AURIGNACIANNESS'

- personal ornaments (beads made on organic blanks)
- tally-marked bone and antler objects; porTable art in general
- earliest examples of parietal art
- 'well-organized' campsites, with pits, constructed hearths and other features

Table 2. Non-Utilitarian Indicators of 'Aurignacianness'.

Figure 10 - Indicators of 'aurignacianness' (Clark and Riel-Salvatore 2005, 108-109).

[156] To a certain extent, the debate over the chronology and the nature of the Proto-Aurignacian is due to a lack of agreement over the number of 'aurignacoïd' features that need to occur for a site to be classed as such, cf. Floss, Hoyer and Würschem 2016, 154.

All these features, however, continue to be common in subsequent industries. Dates obtained from those artefacts – or dates associated with them, if the association can be corroborated stratigraphically – are *terminus ante quem.* Some artefact types have been identified as index fossils, chronological indicators used as proxy for the all period: carinated pieces[157] and split-base points[158] are the ones that are most commonly mentioned. In the literature they appear to be treated as sufficient but not necessary conditions for the definition of the technocomplex: sites that do not include only one of those features can be still labelled as 'Aurignacian' (this is the case for many sites in Spain, with very little bone points if any at all[159]); on the other hand, sites that contain one of those features are sometimes automatically assigned to this period[160].

For this reason, the validity of split-based points as an index fossil for the Aurignacian is crucial for building the chronology of the Middle to Upper Palaeolithic transition. It is, indeed, the one chronological indicator that can be directly dated.

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157 On the role of such artefacts in the definition of the Aurignacian cf. Bar-Yosef 2006, 13-15.

158 Cf. Liolios 2006.

159 Ibidem, 38-39.

160 Ibidem, 37.

1.2.3 Split-base points and the nature of 'index fossils'

During the last radiocarbon dating campaign, the accuracy of the date obtained on sample P42477 (fig.11a-c), a split-based point found and published for the first time by Méray (Fig. 11b), was questioned[161]. A drawing of the same artefact was also published by Breuil in his article on Germolles' Aurignacian technology[162].

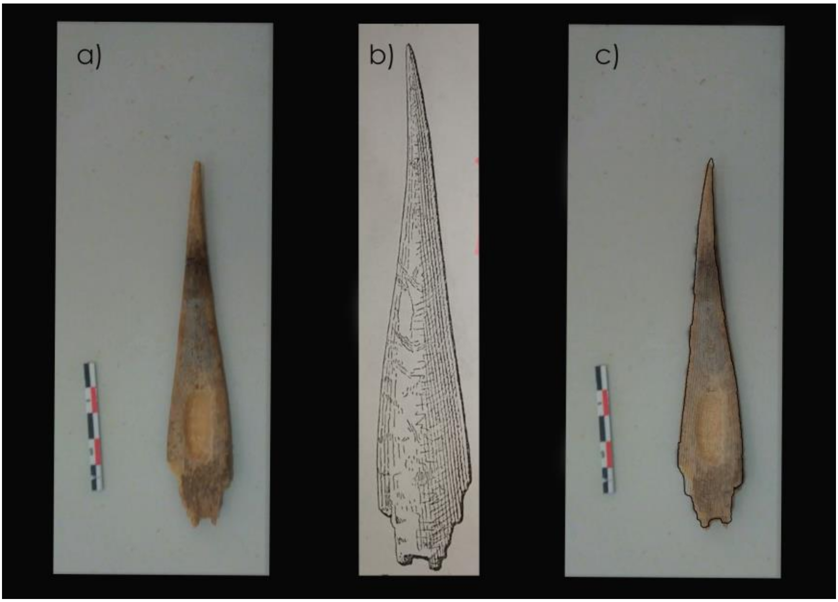

Figure 11 - a) split-based point from Musée Denon (sample 42477) after sampling in 2018; b) drawing of split-based point from Grotte de la Verpillière I in Germolles, after Méray 1876, 264, fig. 19; c) Méray's drawing and 2018 photograph: overlapping render.

[161] Méray 1876, fig. 20.

[162] Breuil 1911, fig. 9.

Indeed, this type of artefact is considered an index fossil of the Early Aurignacian, even though recently it has also been found in association with Protoaurignacian lithics[163]. However, the date obtained in 2016 through radiocarbon dating (OxA-35107 = 25270 ± 200 BP) falls outside the time-interval normally considered to be consistent with the Aurignacian (ca. 45.000 - 28.000 BP)[164]. The artefact had been conserved with unknown chemicals that a standard pre-treatment might have failed to remove. Therefore, in November 2018 a new sample was taken and dated through hydroxyproline radiocarbon dating, optimizing the chance of contamination removal[165]. The results are the following:

Old AF* (OxA-35107)
d13C=-18.56
25270 ± 200 BP

New HYP (OxA-38321)
d13C=-23.08
34810 ± 590 BP

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[163] Trou de la Mère Clochette is the most notorious case (cf. Szmidt, Brou and Jaccottey 2010).
[164] This time interval is the one usually given in scholastic handbooks (cf. MacIntosh 2009, p.351).
[165] Deviese et al. 2018.

The new date fits well in the expected time interval for the Aurignacian technocomplex.

However, it should be noted that the validity of split-based points as chronological indicators is being debated[166]. The controversy surrounding this specific typological taxon is indeed part of a larger debate that has shaken the very concept of 'index fossil' and its use in archaeological practice[167]. While this is not the place to recount this *querelle* in its very long and complex development, the work of David Clarke can be of help in highlighting one aspect of the debate that has particular relevance for chronology building as a process:

> Let us imagine that we have a multiple-layered site at which the repeated visits of the same group employed an identical assemblage in every layer of the site. Then let us suppose that a varying 60% sample was itself accidentally left for selective sampling by *sondage* excavation. […] although the successive assemblages were in fact identical, the sampling effects are such as to make uncommon artefact types seem to appear and disappear in successive levels […]. Now it is a well-known

[166] Cf. Liolios 2006.

[167] 'Index fossils' are types. The debate on their validity is therefore fueled by a corresponding controversy on the role of typology in archaeological practice, Cf. O'Brien and Lyman 2002, 185-188; Beck 1998; Adams and Adams 1991, 220-221.

> archaeological vice to nominate and classify assemblages by the presence of absence of rare 'type fossils'[168].

While the issue of rarity is relevant to the case of split-base points[169], this is not always the case for other index fossils. Types selected as chronological indicators might be as abundant as red-figured pottery in Paestan graves[170]. On closer inspection, the question raised by Clarke does not pertain to the usefulness of 'index fossils' *per se,* but their 'construct validity'. This concept refers to the degree of correspondence between the phenomena that can be directly observed through an instrument, and the construct that is the actual target of research[171]. A classic example of controversial construct validity is the use of IQ tests to measure intelligence, as intelligence is a complex and rather equivocal concept. An interesting feature here is that "Getting people to agree that a particular *measure* has high construct validity requires that they agree that the construct is valid in the first place"[172]. Index fossils are taken to be proxies of a unique social unit and of its chronological and geographical location (be it called culture,

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[168] Clarke 1972, 26-27.
[169] Cf. Liolios 2006, fig.1 for a map of known split-based points.
[170] Trendall 1987.
[171] Bernard 2011, 45.
[172]*Ibidem.*

horizon, or technocomplex)[173]. Therefore, even a subtle divergence in the definition of the construct of interest (in this case, of the Aurignacian) can be a source for major controversy. The validity of index fossils is crucial to establishing the chronological articulation of Grotte de la Verpillière I. Recent excavations could only investigate what was left by almost 150 years of legal and illegal digging: numerous *facies* whose presence was suggested by artefacts collected by previous investigators could not be found. Some of those artefacts – and especially index fossils for the Aurignacian – were selected for radiocarbon dating in an attempt at confirming the presence and establishing the chronology of a typical Aurignacian layer at the site (Table 1b)[174]. Following the flow-chart provided in Appendix I (Guidelines for the use of radiocarbon dates), results in Table 1b should not be used to assess the chronology of any occupation phase at the site, as contextual information are lacking or incomplete[175]. They could be used, however, to answer a different research question: are those artefacts – and particularly the split-based point and its by-product, the tongued piece – reliable indicators of the Aurignacian technocomplex and/or one specific phase thereof?

A sensible answer to this question requires to take a step back to consider the very definition of 'index fossil' as a heuristic

[173] O'Brien and Lyman 2002, 185-216.

[174] Floss, Hoyer and Würschem 2016,149-154.

[175] *Vide infra* Appendix I.

tool. Originally derived from the corresponding concept in geological sciences, index fossils in archaeology are types or classes of artefacts used as chronological indicators to mark the synchronicity of stratigraphically unrelated assemblages (cross-dating)[176]. Following this definition, at least three aspects of an index fossil should be investigated further.

First, an index fossil is a class of artefacts or a type, e.g. split-base points. Types are conceptual units: after finding a certain number of similar artefacts, scholars group them to build types and classes, establishing some defining characters[177]. In our case, pointed antler tools with a split at their base have been classed together. Defining characters are the raw material and the functional/technological peculiarity of the split. However, many morphological – and maybe technological[178]– differences can be noticed amongst artefacts classed into this taxon[179]. Both size and shape of the points

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[176] Cf. O'Brien – Lyman 2002, 190.

[177] A wider discussion of types as conceptual units can be found in Adams and Adams 1991, 27-95; it should be noted that several disputes on the validity of specific types – and on their chronology – actually revolve around the variables used to define them (decoration, shape, technology, measures); cf. Lucas 2005, pp. 95-113 for an attempt at assessing the diachronic variation of those variables in an independent way.

[178] Liolios 2006 *contra* Tartar and White 2013.

[179] "These objects, typologically similar, are technologically different. They share function, raw material, chaîne operatoire of blank production, and hafting system; but they differ in the underlying concept of what an efficient point should look like" Liolios 2006, 42.

vary and the material can sometimes be different (fig. 12). Is that variation chronologically relevant? Or is it connected to some other factor, such as local tradition, or the part of the antler that was used to fabricate it?

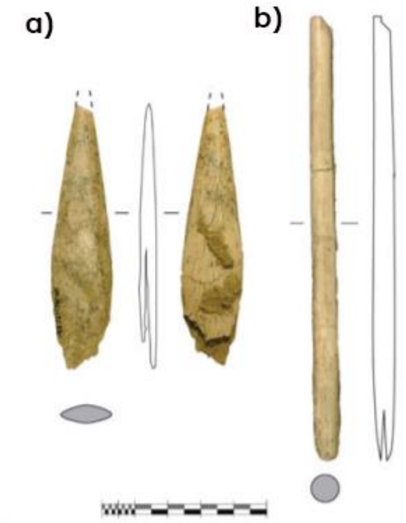

Figure 12 - a) split-base point in antler from Labeko Kova; b) split-base point in ivory from Labeko Kova (Tejero 2016: 57)

Second, another useful aspect to assess the reliability of an index fossil is the biunivocal correspondence between the selected type or class (e.g. split-based points) and the time-unit it represents (e.g. the Aurignacian)[180]. As it happens in the case discussed here, an index fossil is often taken to represent more than a time interval: it becomes a proxy for a 'culture' or

[180] O'Brien and Lyman 2002, 189-191.

'technocomplex'[181] that is thought to be synchronous over space. This supposed synchronicity might be relative (e.g. in any given site the Aurignacian is younger than the Châtelperronian) or absolute (e.g. different Aurignacian sites shared the same time interval). In any case, when an index fossil is used to represent a 'culture', a latent theory of historical change is silently at work. It is implied that history is made of periods of *stasis* when things are left unchanged, and moments of *change* when everything (politics, philosophy, art style) is altered at the same time[182]. Following this line of thought, one specific aspect of human life or technology (e.g. osseous materials' artefacts) can be used as proxy for all the others. However, when abandoning this perspective, it seems conceivable that different aspects of life, society and technology (e.g. lithics and osseous industries) change at different paces.

Finally, to assess the validity of an index fossil as chronological indicator a third issue must be considered. A good index fossil occurs over a short time interval and has a large spatial distribution[183]. The latter is a strong point in

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[181] The habit of labelling a context 'Aurignacian' and even 'Early Aurignacian' just for the presence of one split-based point has been criticized by many scholars, e.g. Liolios 2006, 37; Davies 2007, 263.

[182] This is a rough simplification of a very complex problem in the philosophy of history and archaeological theory i.e. the nature and pace of change. For archaeology, see at least Lucas 2005, Thomas 1996 and Gosden and Kirsanow 2006.

[183] O'Brien and Lyman 2002, 191-199.

favour of split-base points, as they are found from the Balkans to the Iberian Peninsula. The time interval over which they occur, however, is more controversial. Traditionally, split-based points were considered as index fossils for the Early Aurignacian (or Aurignacian I) but more recently they have been found also in association with Proto-aurignacian lithics[184] . Available direct dating would indicate that this type was used from 48.000 to 35.000 CalBP[185]. However, these dates should be treated with some caution, given that most samples were conserved or presented other analytical difficulties. The process through which index fossils are defined is usually one of trial and error in order to find the type that has the largest spatial distribution and the smallest time interval associated with it (based on site distribution and stratigraphy). Then, the validity of the taxon is confirmed through independent methods, such as radiocarbon dating or Thermoluminescence dating (TL), which also help to refine its precision[186]. However, in the case of split-based points, the type was constructed very early in the history of the discipline and for most artefacts we don't have stratigraphic information. Independent confirmation has been carried out via radiocarbon dating, whose refinements have significantly improved its reliability over the years, especially for dates that approach the

184 Szmidt, Brou and Jacottey 2010; Tejero 2016.

185 Wood et al. 2018 especially fig. 6. Calibration curve of reference: IntCal13.

186 O'Brien and Lyman, 2002, 191-199.

radiocarbon limit[187]: assessing the reliability of old dates can be difficult. At the Research Laboratory for Archaeology and the History of Art (RLAHA) in Oxford, the author of this dissertation had the chance to work with Rachel Hopkins, developing a flowchart to help with this task (Appendix 1). The large interval of time over which direct dates of split-base points are distributed (Chart 1) might reflect the long timespan over which split-based points were in use, a failure to remove contamination from conserved samples, an error in the identification of the samples as split-based points, or any combination thereof.

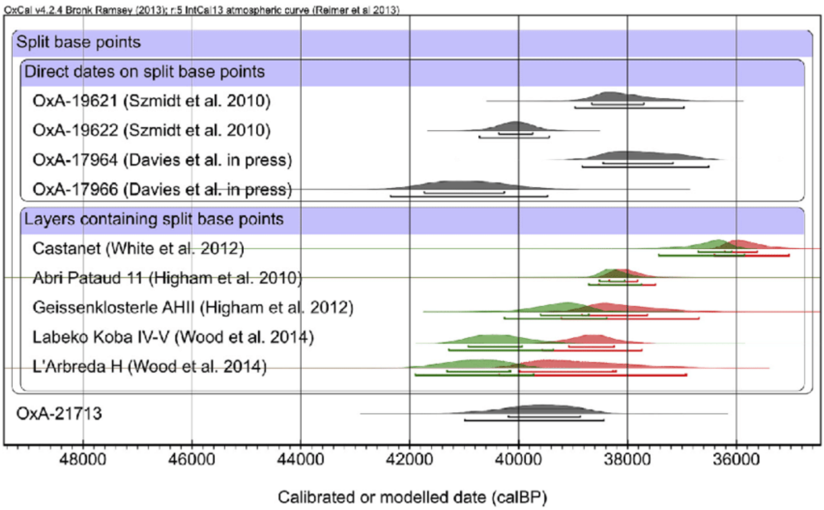

Chart 1 - Radiocarbon date on an antler baguette from El Castillo Aurignacian Delta (OxA-21713) compared to modelled ages for the start (green) and end (red) of assemblages containing split base points from Aurignacian assemblages across western Europe (Wood et al. 2018 fig. 6).

[187] Cf. Wood 2015.

In light of these considerations, the validity of split-based points as index fossils for the Aurignacian is to be treated with caution. The reason is not, as most commentators would suggest, linked to the morphological variations of artefacts pertaining to this typological taxon, as they do not appear to be chronologically relevant. The precision afforded by this taxon, however, is lower than investigators had hoped for. Moreover, the internal periodisation of the Aurignacian, and the validity of lithic types for its definition, are under constant review[188]: it is impossible to assess the validity of the index fossil if the construct it represents is not agreed upon. Finally, following Clarke's quote, the rarity of split-based points would magnify the effects of selective sampling.

Even if a certain correspondence might be established between this typological taxon and the Aurignacian in its broader sense, the association should not be extended further. Direct dating of split-base points is not to be used as an indicator for the arrival of Modern Humans in Europe, as some scholars have recommended[189]. Especially when decontextualized artefacts are sampled, direct dating can only be useful as an independent confirmation for split-base points' validity as chronological indicators.

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188 E.g. Clark and Riel-Salvatore 2005; Bar-Yosef and Zilhão 2006.

189 Davies 2007, 271-273.

1. 3 Conclusions

This analytical appraisal of Grotte de la Verpillière I in Germolles is useful in at least two ways: first, it allows to break chronological labels to see the elements they represent; consequently, it highlights the connection between the dated objects and the chronological question that we ask of them. In doing so, it becomes clear that some chronological controversies are influenced by concepts of human evolution. In turn, this awareness can help us to adjust the question asked, and/or to choose alternative proxies to answer them.

Indeed, authors who use labels such as 'Aurignacian', 'Châtelperronian' and 'Mousterian' are usually aware of their conventional nature[190]. Nonetheless, the single components that the labels stand for – e.g. silex typology or silex technology, index fossils, faunal remains, human authorship, population – are seldom explicit. The case of Grotte de la Verpillière I in Germolles – a site that played a role in the making of some of these definitions – has the merit that these components (and variations thereof) are easily brought to light. The Aurignacian level, from which the artefacts in the Musée Denon collections are supposed to come from, could not be confirmed during new excavations

[190] Cf. Bar-Yosef and Zilhão 2006.

by investigators of Tübingen University[191]. They did, however, find intact Aurignatian levels at a nearby cave, Grotte de la Verpillière II[192]. Several radiocarbon dates were obtained from samples from the newly excavated cave, from the Grotte de la Verpillière I GH15 'aurignacoïde' layer, as well as from the collections of the Museum.

Admittedly, the target event of the dating campaign is the arrival of Modern Humans in Germolles[193]. The dated objects were selected because they are considered index fossils of the Aurignacian (even though some of them could be Gravettian as well). Putting aside the time elapsed between the death of the animal (measured by ^{14}C) and the construction of the tool from its bone or antler, a large time interval still separates the dated event from the target event. Several logical steps are employed to connect the two: the osseous points (and the tongued piece) are indicative of the Proto-Aurignacian or Early Aurignacian period; Anatomically Modern Humans arrive in Germolles in the Proto-Aurignacian or Early Aurignacian; therefore, the date of osseous points can be used as an approximation for the arrival of Modern Humans in the area. Both the first and the second premise, however, cannot presently be validated.

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191 Floss, Hoyer and Würschem 2016,149-154.
192 Frick 2015.
193 Floss pers. comm.

Chapter 2
The Fusco Necropolis, Syracuse

> τὰς δὲ Συρακούσσας Ἀρχίας μὲν ἔκτισεν ἐκ Κορίνθου πλεύσας περὶ τοὺς αὐτοὺς χρόνους οἷς ᾠκίσθησαν ἥ τε Νάξος καὶ τὰ Μέγαρα. ἅμα δὲ Μύσκελλόν τέ φασιν εἰς Δελφοὺς ἐλθεῖν καὶ τὸν Ἀρχίαν: χρηστηριαζομένων δ᾽ ἐρέσθαι τὸν θεόν, πότερον αἱροῦνται πλοῦτον ἢ ὑγίειαν: τὸν μὲν οὖν Ἀρχίαν ἑλέσθαι τὸν πλοῦτον, Μύσκελλον δὲ τὴν ὑγίειαν: τῷ μὲν δὴ Συρακούσσας δοῦναι κτίζειν τῷ δὲ Κρότωνα.
>
> Strabo, VI, 2, 4

The Fusco necropolis, in Syracuse, is a site known to most classical archaeologists for its relevance to the debate on the chronology of proto-Corinthian pottery, a topic of heated discussion since the mid-XIX century.[1] It was identified by

[1] Before Paolo Orsi built the first chronology of proto-Corinthian pottery based on the findings of the Fusco necropolis, the topic had already been discussed by several scholars: Conze 1870; Dümmler 1887; Rayet and Collignon 1888, 55-68; Wilisch 1892; and Masner 1892. Afterwards, the Fusco necropolis was discussed in all the main publications on Corinthian and proto-Corinthian pottery. Among others: Schweitzer 1918, 1-9; Johansen 1923, 15-16; Payne 1931; Payne 1933; Weinberg 1941; Neeft 1987: 363-365; Dehl 1984; Amyx 1988; Neeft 2012.

its first excavator as the oldest necropolis of Syracuse.[2] The city's founding date is one of the cornerstones of Western Greek chronology.[3] Therefore, the site lies at the centre of a much more complex discussion on Greek colonisation,[4] involving, on the one hand, the reliability of historical sources (and Thucydides in particular) and, on the other, the history of the study of Greek antiquities of Sicily.

Indeed, if the reader wants to understand the following discussion in some depth, a premise is in order. The Sicilian chapter of the 'history of Greek histories' has yet to be written.[5] This is probably due to the opinions of very

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[2] Cavallari 1883, *vide infra* p. 136-162.

[3] As explained by Bickerman (1963, 90-91), the Turin King List and a fragment of Eratostenes allow us to determine the date of the first Olympics, and of the beginning of the Peloponnesian war. Then, Scaliger and Petavius used synchronisms and astronomical controls to anchor Thucydides' relative chronology to these dates, obtaining the 'fundamental dates' (among which the foundation of Syracuse) that were to be used for 'converting' other ancient dates to our system of time measurement.

[4] Available data on this site are analysed in depth, among others, by Dunbabin 1948, 52-64; Villard and Vallet 1952, 331-343.

[5] Only few references to Sicily can be found in Ampolo 1996, 1057-1058; Ampolo 1985 is more concerned with continental Magna Grecia than with Sicily and focuses mainly on the (re)discovery of ancient monuments and sites; Salmeri 1992a and 1992b provide a useful account of Sicilian antiquarian studies of the XIX century (with an incursion in the first 30 years of the XX century). Momigliano 1984a attempts what can be called a 'history of Sicilian histories'; Momigliano 1984b attempts a discussion of XVIII century studies on the Greek past of Sicily. In both

influential modern intellectuals minimising the role of Southern Italy in the study of (or their interest in) Greek antiquity.[6] In 1962 Piero Treves wrote: "il Mezzogiorno nell'Ottocento fu sostanzialmente remoto dallo studio di antichità".[7] Momigliano, while admitting that some of the international scholars writing about Sicilian antiquities were influenced by local studies, wrote: "[…] before, during and after the eighteenth century, the Sicilians refused to identify themselves with the Greeks".[8] In the extremely influential history of classical scholarship by Ulrich von Willamowitz, Sicily is only mentioned to remember the infamous episode of the forged letter allegedly sent by the Virgin Mary to the town of Messina, which fooled so many intellectuals in the XVI and XVII centuries. Introducing the work of Giambattista Vico he writes: "A Napoli, dove gli studiosi di

cases, however, his approach is rather 'etic' and some authors crucial to the local intellectual context are overlooked.

[6] De Francesco 2012 and 2013.

[7] Treves 1962, XXXV. Opposite to this tendency, one should mention a tradition of studies on the history of archaeological research in Southern Italy: two of the annual Taranto conferences (AA. VV. 1989 and 1996, to some extent 2014) were devoted to this topic. De Francesco 2013, Harari 2014, Tagliamonte 2014 highlight the role of scholars concerned with the Greek and pre-Greek history of Southern Italy in the construction of Italian identity (especially the so-called 'pythagorean myth' conflating the *virtutes* of Italic people and ancient Greek wisdom).

[8] Momigliano 1984b, 145.

mestiere erano così poco capaci di mettere a profitto il tesoro caduto dal cielo…”.[9]

While a history of Sicilian antiquary – or a history of scholarship on Sicilian antiquities – exceeds the scope of this dissertation, some aspects of the local intellectual landscape need to be highlighted. At least three main trends can be identified when considering the attitude of Sicilian scholars towards their historical past.

One of them is the ‘negative’ approach to Sicilian identity described by Momigliano: he argues that the histories of Sicily have mostly depicted the island as a land of successive invasions and dominations, while Sicilians defined themselves in opposition to the most recent invaders, without being able to find their origins in one of their pasts.[10] Momigliano finds the roots of this approach in the foundational monograph on the history of the island, *De rebus siculis Decades duae* published in 1558 in Palermo by the Dominican friar Tommaso Fazello.[11] This approach to Sicilian history is, according to Momigliano, the prevailing one at least until the end of the XVIII century and even

[9] Willamowitz 1967, 92.

[10] Momigliano 1984a and 1984b.

[11] *Contra* Calderone 1992: he argues that Fazello promotes the glorification of a peculiar version of Greekness that, through the encounter with indigenous people, was born in Sicily. A perspective that will be encountered in the work of Cavallari. See Pace 1932 and Sanfilippo 1973 for the life and work of Fazello.

later.[12] This perspective is certainly very apparent in the 1745 work *Histoire générale de Sicile* by Jean Levesque de Burigny: he applies Vertot's idea of successive revolutions to the history of the island.[13] The same notion of successive conquests can be found in the *History of Sicily* written in the last decade of the XIX century by Edward A. Freeman, even though he expresses a predilection for the Norman period when, chasing away the Semitic Arabs, Sicily became definitively Aryan (Fig. 13).[14]

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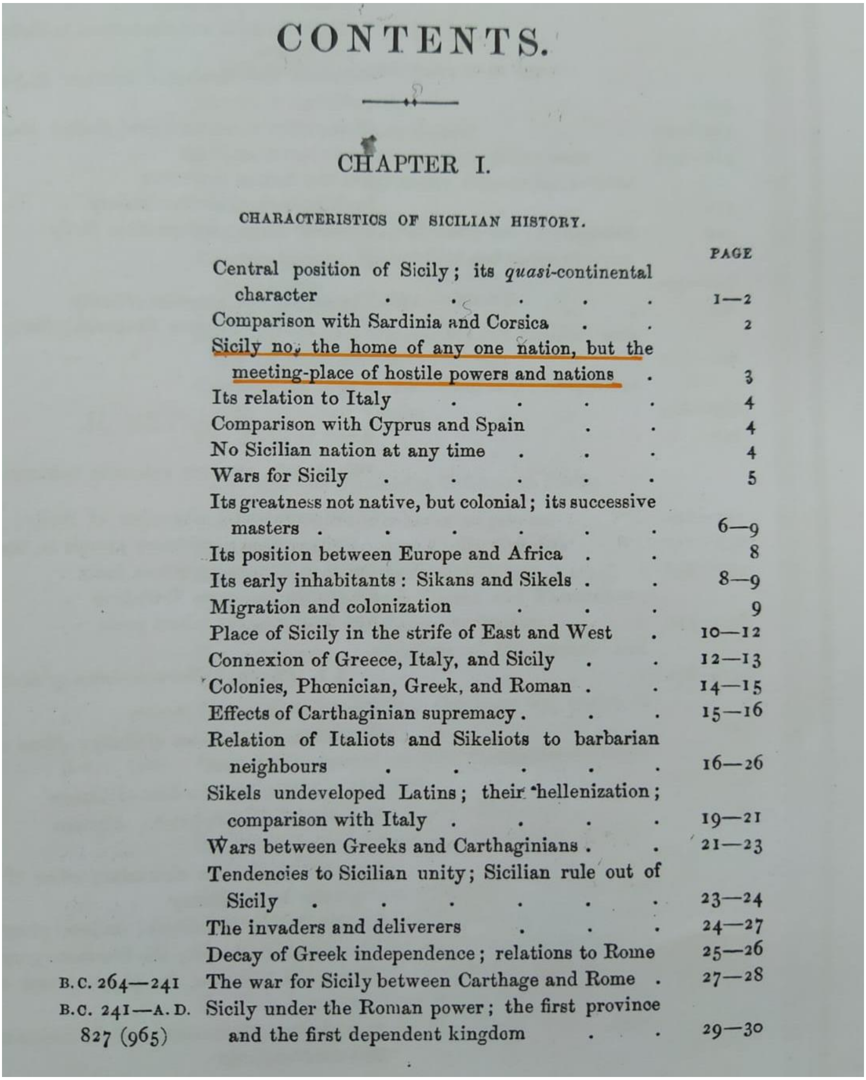
[12] "Fazello propose una interpretazione della storia del suo paese che rimase dominante sino alla fine del Settecento e non ha perduto la sua attrazione anche oggi. Fazello non identifica il popolo siciliano con alcuno dei popoli che occuparono l'isola attraverso i secoli. Per lui la storia della Sicilia è storia di invasioni, di assestamenti, di apporti da cui i Siciliani possono ricevere giovamento o danno." Momigliano 1984a, pp.116-117.

[13] Momigliano 1984a, p. 117. Genet 1881 on his biography.

[14] Freeman 1891. A similar thesis is supported by Finley 1989.



CONTENTS.

CHAPTER I.

CHARACTERISTICS OF SICILIAN HISTORY.

Figure 13 – Table of contents of Freeman 1891 (highlights by the author).

It should be noted that this is only one of at least three different approaches to the history of Sicily. It is the one that is most likely to be seen from an 'etic' perspective, as it was embraced by influential foreign scholars.[15]

If we turn to local intellectuals concerned with the history and antiquities of their town and adjacent territory – they may have lesser merits, but from an 'emic' perspective they were the actual fabric of the Sicilian intellectual *milieu* – alternative approaches appear.[16]

Specifically, there is one approach that tends to glorify the Greek civilisation of Sicily, and another that eulogizes Sicily's prehistoric past: most of the scholars studied in this chapter can be placed somewhere in the spectrum between these two antithetical views. While these tendencies become increasingly widespread during the XIX century, they were already visible in earlier local studies. In Syracuse, for example, the glory of the Greek past was a topic of great

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15 Calderone 1992 highlights that Valla's translation of Thucydides canonized the word 'colony' (*colonia*) for the Greek *apoikia,* conveying through the word an erroneous concept that will be at the center of conceptual and political debate outside and inside Sicily, to the point that Don Fabrizio in *The Leopard* says of Sicilian people: "da 2500 anni siamo colonia" (quote from Calderone 1992, 15).

16 The study of these personalities has often a biographical character and are dispersed in local publications of the Società di Storia Patria or analogous associations: e.g. [Indici delle Riviste in Linea - Archivio storico siracusano - Indici 2000-1991.]

attention, starting with the works of Vincenzo Mirabella Alagona, who founded the first museum of the town in his own house. His *Dichiarazione della pianta delle antiche Siracuse*, published in Naples in 1613, has been the main source of information on the ancient topography of the Greek town until the end of the XIX century,[17] when it was replaced by the topographical work of Francesco Saverio Cavallari. The XVIII century was characterised by archaeological surveys and excavations, conducted by local priests and noblemen, who were the hosts of European intellectuals coming to Sicily for the Grand Tour.[18] Serafino Privitera in the Preface

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[17] The book was reprinted in Palermo in 1717. Mirabella's topographical map is also reproduced in Johann Georg Graevius in his *Thesaurus Antiquitatum et Historiarum Siciliae* (Leiden 1725). In Serafino Privitera's *Storia di Siracusa antica e moderna* (1878-1882) the work of Mirabella was still one of the main sources.

[18] See Cugno 2017 with rich local bibliography. Amongst them, Cesare Gaetani Count della Torre (1718 – 1805) had an agreement with Sir William Hamilton, who promoted his excavations with the ministry of the Neapolitan government, in exchange for drawings, descriptions, artefacts and hospitality for the travellers he recommended (see Sgarlata 1996). Another scholar mainly interested in the Greek past of the town was Saverio Landolina Nava: imbued with Enlightenment ideals, he cultivated both botany and antiquarianism and was one of the local guides of Jean-Pierre Louis Laurent Houël (see Russo 2007). Sicily was notoriously the wildest part of the Grand Tour and even more so Syracuse: the conditions of extreme poverty of the inhabitants and frequent plagues often dissuaded visitors (e.g. Goethe) and prompted unflattering descriptions (Brydone 1806 and Von Riedesel 1771), often featuring a nostalgic comparison to the splendid past of the town. Its history, nonetheless, became an object of major attention

to the first volume of his fundamental history of Syracuse (published in three volumes between 1878-1882), writes:

> "Siracusa, la più bella e la più grande delle città greche, famosa nella storia antica del mondo, dopo le sue cadute, ridotta nella piccola Isola […] trovossi, come tante altre insigni città, di oscurezza coperta e di oblio." [19]

During the first half of the XIX century, the Greek past became a topic of great interest and a source of identity, especially in the works of Domenico Scinà[20] and, in the second half of the century, of Isidoro La Lumia.[21] At the same time, another trend rose to prominence: the celebration of the prehistoric past of the island. This was inspired by the 1810 work of Giuseppe Micali *L'Italia avanti il dominio dei romani* and found an authoritative local proponent in Vincenzo Natale, with his *Discorsi sulla storia antica della Sicilia* published in Naples in 1843.[22] During the XIX century, these historical preferences should be read against the background of political riots and independentist

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among international scholars (Raoul-Rochette 1815; Holm 1869; Beloch 1912).

[19] Privitera 1878, p. 3.

[20] Messina 1974; Salmeri 1992a; for a biography of Domenico Scinà see Brigaglia 2018 with selected bibliography.

[21] Salmeri 1992a, 74; *contra* Mazzarino 1977.

[22] See Salmeri 1992b, p.99-100; De Francesco 2017, p. XXV.

ideals.[23] Throughout the century, many Sicilian intellectuals were strongly involved in politics and often adopted a markedly regionalist perspective,[24] which acquired different connotations depending on their cultural background and on contingent historical developments.[25] The tale of the numerous conquests, as well as the emphasis on Greek colonisation, or the praise of the indigenous Sicels – as well as any combination of the above – can and have variously been employed to affirm the independence of Sicily from the Neapolitan rulers after the Restauration: some scholars would emphasize the Sicel and Sicanian origins of Sicily against the Greek origins of Naples;[26] others would stress the strive for freedom of the Greek colonies, equating it to the fight for freedom and independence of Sicily, while comparing the Neapolitan conquerors to the treacherous Romans of Marcellus.[27] Many intellectuals took part in the riots of 1820 and 1837 and some also entered the independent Sicilian Parliament of 1848.[28] The fight for

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[23] Salmeri 1992a; Girardi 2017.

[24] For a fierce critic of these regionalist tendencies see Gentile 1919.

[25] Giarrizzo 1989.

[26] De Francesco 2017, p. XV.

[27] Salmeri 1992a, p. 74.

[28] Gregorio Ugdulena was confined in Favignana for taking part in the 1848 revolution and, after supporting Garibaldi in 1860, he became a member of the Italian Parliament (De Stefani 1980). Francesco Saverio Cavallari, the first excavator of the Fusco necropolis, came back from Mexico to take part in the riots of 1848 and, when the Bourbon rule was re-established, he had to leave the island, *vide infra* 136-162. Michele

independence could not but be a tacit element of any historical reconstruction at the time.

Later in the century, similar historical arguments – where, behind the Romans, one could see the most recent invaders – were used by scholars disappointed in the behaviour of Bixio's troops,[29] or discouraged by the distance of the Piedimontese authorities: some Sicilian intellectuals felt that their role in the landing of Garibaldi and, therefore, the part they played in the Unification of Italy was being disregarded. The Sicilian Vespers could be recalled as a golden period of the history of the island, a premonition of the glorious, but disregarded, endeavours of the present.[30]

The political and intellectual landscape of Sicilian archaeology changed remarkably between the last two decades of the XIX century and the World Wars: with the arrival of officials selected by the Ministry in Rome (e.g.

Amari, one of the most influential Sicilian intellectuals of the XIX century, was exiled to Paris for his anti-Neapolitan beliefs (see the Proceedings of the Symposium on "Michele Amari storico e politico" 1990).

[29] Verga's short story *Libertà* became the manifesto of disappointed expectations.

[30] Lionardo Vigo, in the *Protostasi Sicula* (where the original inhabitants of Sicily are described as Pelasgians and the island is identified with Atlantis) writes that the Sicilian people "col suo sangue, co' suoi tesori, coll'eccidio delle sue città completò l'unità italiana, rovesciò i Borboni, e rinnovò i prodigi del Vespro! Dio non ne lo faccia pentire" (cf. Girardi 2017, XXXI).

Paolo Orsi and Giovanni Patroni) and with the new wave of nationalism in politics and culture, the regionalist flavour of Sicilian scholarship faded away, or it took a new turn that complemented nationalistic ideals, redeeming the role of Rome.[31] Emblematic of this change of pace is the work of Ettore Pais: in 1888, in his first work on Roman Sicily, he talks of a 'Roman yoke' imposed on the island; in 1893 *Gli elementi sicelioti nella più antica storia romana* builds a connection between ancient Syracuse – and its historiographers – and the history of Rome as it was written in the V century BC; the next year, in his *Storia della Sicilia e della Magna Grecia,* which was meant to be the first volume of a history of the Italian nation,[32] he describes Syracuse as the predecessor of Rome, as an enemy of Carthage.[33] A direct connection between Sicels and Italic people – with Rome being the *acme* and the unification of the Italian *ethnos* – is asserted by younger scholars, finding the *Kulturgeist* of Sicily in prehistoric times: the two main representatives of

[31] "[…] in molti […] intellettuali del nostro paese, lo svolgimento della politica interna ed estera del Regno d'Italia tra Adua e Versailles aveva determinato una netta presa di posizione in senso nazionalistico, in cui a Roma ed al suo mito spetta un ruolo di grande rilievo" Salmeri 1992b, p. 112. An interesting take on this historical conjuncture can be read in Canfora 1980.

[32] De Francesco 2013 and the review by Harari (2015).

[33] Salmeri 1992b describes the development of Pais' thought. Cagnetta 1994 focuses on his nationalistic views.

this trend, Biagio Pace and Giovanni Patroni, enthusiastically adhered to the National Fascist Party.[34]

At the same time, the role of German scholarship, and the impact of European cultural élites travelling to Sicily since the XVIII century for the Grand Tour, should not be underestimated.[35] It was strongly influenced by Winckelmann[36] and the travellers that he sent there to find the material and immaterial remnants of Greek culture.[37] Indeed, scholars such as Adolf Holm and Julius Beloch were especially interested in the classical past of the island and the analysis of ancient sources.[38] Julius Schubring was interested in topographical studies, and Otto Benndorf and

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[34] Nicoletti 2014-2015 with extensive bibliography. See also Settis 1989, 143-157 for an understanding of the dynamic relationship between regionalism and nationalism ("centre and periphery") in the work of Biagio Pace and Pirro Marconi.

[35] Falzone 1963; Ampolo 1985 on Magna Graecia; on Sicily Momigliano 1984a, pp. 125-130. For the role of German scholarship in the definition of Western Greek art see Settis 1989, and most recently Frisone 2018 with vast bibliography.

[36] He never went to Sicily, but he wrote about it based on the drawings and descriptions of others (Winckelmann 1759). An interesting perspective on the impact of Winckelmann's approach – as opposed to the rationalism of antiquarians and catalogue writers – on the study of Sicilian antiquities see Calderone 1992: he wishes for a renewed and modern return to Winckelmann's approach.

[37] Momigliano 1984b, pp. 143-145. According to him, German scholars had a role in the progressive identification of Sicilian people with their Greek past during the XIX century (Momigliano 1984b, p. 151).

[38] Mazzarino 1977.

Reinhard Kekulé were interested in Greek archaeology. Theodor Mommsen and Georg Kaibel studied inscriptions.[39] For them, Sicily was of interest because it was part of Greek history.

Following the same principle, the founding of Syracuse, as an important part of Greek history and a nodal point in its chronology, has been of interest to many scholars: from Scaliger[40] to Emmius,[41] from Bossuet[42] to Robertson,[43] from Van Compernolle[44] to Dunbabin.[45]

Ancient sources report three main traditions on the founding of Syracuse, assigning different dates to the event. Thucydides (VI, 3-4) traces a relative chronology of Sicilian *apoikiai*,[46] placing the foundation of Syracuse 5 years before Megara Hyblaea. But he also mentions that the destruction of Megara Hyblaea by Gelon happened 245 years after the foundation of Syracuse (Tychidides VI, 4). And we know (Herodotus VII, 156-7) that Megara was destroyed between the battle of Himera (480 BC) and the conquest of Syracuse

[39] Salmeri 1992b, pp. 97-98.
[40] Scaliger 1583.
[41] Emmius 1626.
[42] Bossuet 1681.
[43] Robertson 1778.
[44] Van Compernolle 1959.
[45] Dunbabin 1948, 435-471.
[46] A compendium of different dates attributed by different sources to the foundation of Sicilian colonies can be found in Miller 1970.

by Gelon (in 484 BC). From these dates one can count back and deduce that Syracuse was founded between 733 and 731 BC. Adding the foundation date of Camarina and of Akragas obtained from Pindar and his scholiasts,[47] the date 733 BC is the one preferred by scholars. Indeed, it roughly coincides with the Armenian version of Eusebius (734 BC).[48] This is the first and most widely accepted tradition.

The second tradition is linked to the Marmor Parium.[49] While the absolute date is lost, one can still read the indication that Syracuse was founded on the twenty-first year of the reign of Aeschylos, which would be around 757-756 BC. In their discussion on the chronology of the foundation of Syracuse, Vallet and Villard highlight that the 23-24 years separating the date in the Marmor Parium from the date of Thucydides are the same number of years separating the two dates given for the war of Troy, i.e. 1208 in the Marmor Parium and 1183 the traditional one: "Il ne s'agit pas, en réalité, de deux dates différentes mais de l'indication, au moyen de deux systems différents, d'une seule et même date".[50]

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[47] For Camarina: Schol. ad Pind. *Olymp.* V, 16 and 19; for Akragas: Pind. and Schol. ad Pind. *Olymp.* II, 166 ff.

[48] Vallet, Villard 1952, 292-299.

[49] *Marmor Parium,* I, 39 (consulted Jacoby 1980, orig. 1904).

[50] Vallet, Villard 1952, 300. On ancient systems of relative chronology (and specifically bringing the examples of Thucydides and the Marmor

Finally, the last tradition proposes a later date. Strabo (VI, 262 and 269-270) links the travel of Archias to the one of Myscellos, the founder of Kroton. Eusebius dates the foundation of Kroton to 708 BC. The same tradition can be found in a small fragment of Diodorus (VIII, fr. 17) and Stephanus of Byzantium (*s.v.* Συράκουσαι).[51]

The 733 BC date, however, is widely accepted and rarely questioned.

While historical sources on the founding of Syracuse have been under scrutiny at least since the XVI century, archaeological data on its foundation only became available with the discovery of the Fusco necropolis, in the mid-XIX century. Indeed, it was immediately believed to be the oldest necropolis of the town, chronologically close to the arrival of the first Greek colonists.[52]

The first findings were recorded in 1842 and, later, in 1868 when Sicily was already part of Unified Italy. In the last three decades of the XIX century and the first fifteen years of the XX century, programmed excavations – sponsored by the ministry and the regional committee for Antiquities – were conducted by Cavallari and later by Paolo Orsi. Both scholars have left extremely detailed accounts of their work,

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Parium) and how they relate to specific ancient calendrical and chronographic systems see Bickerman 1963, 59-80.

[51] *Ibidem*, 301-309.

[52] Cavallari 1883; Orsi 1894.

which is uncommon for that period. Most artefacts coming from these excavations are said to be kept at the Archaeological Museum Paolo Orsi in Siracusa, but some of them was lost (Cavallari excavation 1871). However, one should remember that, until the Nasi law of 1902,[53] pre-unitarian rules applied to archaeological excavations in what had formerly been the Reign of Two Sicilies. Therefore, the owner of the land where excavations took place had the right to keep part of the recovered artefacts (the 'spettanza'). Construction works for a railway line prompted a large excavation in 1915 where 94 tombs were dug up. This is the last campaign, to my knowledge, to have been systematically published, even though more recent excavations have been conducted by the Soprintendenza.[54]

The material remains found in Cavallari and Orsi's excavations prompted specialised studies, such as the one conducted by Patroni on the *fibulae* found in the Fusco necropolis, as well as several investigations of Corinthian and especially proto-Corinthian pottery: it precisely for the connection to the founding date of Syracuse that the site is

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[53] Even in the Nasi Law, Art. 16 states that the government can conduct excavations on private property (this is the case for nearly all the campaigns conducted in the Fusco necropolis, outside of the modern cemetery) but the owner of the land will receive ¼ of the artefacts found during the archaeological campaign.

[54] Basile 1993-1994 provides preliminary information; see also Zirone 2011.

considered crucial to anchoring the relative chronology of the typo-chronological sequence to the absolute chronology of Greek *apoikiai*.[55]

To this day, the site is often mentioned as a crucial element for understanding the early colonisation and its chronology, as well as the relationship between Greek colonists and local population, and the interchange of material cultures in the Mediterannean. But while other key sites, such as Cumae or Pithecussae, have been the subject of careful reconsideration,[56] the Fusco necropolis has not been analytically examined since the 1910s.

It is for these reasons that the site was selected as a case-study. Besides being a scarcely analysed reference site, it provides the opportunity to explore the connection between archaeology and local politics in XIX and XX centuries Sicily. Moreover, this case study allows us to investigate the methods and assumptions used to connect stylistic/typological sequences with historical dates, and with other artefacts' series.

[55] *Vide infra* 189-191.

[56] For Pithecussae cf. Nizzo 2007.

2. 1 The Fusco Necropolis. An under-published reference site

The first archaeological excavations in the Fusco necropolis took place in the 1860s. A local engineer and enthusiast antiquary, Luigi Mauceri, was the first person to publish this site in 1877. While his work was known and used by Paolo Orsi, it has remained unnoticed by most later commentators. Most of the archaeological excavations on this site were conducted in the last two decades of the XIX century and the first fifteen years of the XX century, thanks to the two main personalities of Sicilian archaeology at the time: Francesco Saverio Cavallari and Paolo Orsi. While it was the former who put the Fusco necropolis on the map, it was Paolo Orsi who enshrined the site as a reference point for Sicilian archaeology and the chronology of (proto)Corinthian pottery. His publications on the necropolis were extremely accurate and soon became the reference standard for scholars who wanted to work with that site. Over more than 20 years, nearly 800 tombs from the Fusco necropolis have been published by Paolo Orsi. More recent archaeological campaigns were conducted by the Soprintendenza, but the results remain largely unpublished.[57]

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[57] Basile 1993-1994 provides preliminary information; see also Zirone 2011.

The following paragraphs are devoted to those authors and works that describe the first excavations of the Fusco necropolis, and those that introduced the site to the archaeological discourse. Finally, one additional author will be taken into consideration in this paragraph: Giovanni Patroni. He neither conducted nor published any excavation campaigns at the site. Instead, he used first-hand knowledge and observations of both published and unpublished material stored at the Archaeological Museum of Syracuse to build a chronological sequence – in his words, a 'history of the Greek *fibula*' – based on the findings of the Fusco necropolis.

The assumptions and conclusions presented by these scholars on the Fusco necropolis, the artefacts found in it, and their chronological determinations will be analysed in relation to the authors' intellectual contexts, their ideas on the origin of races (e.g. monogenism and scientific racism), and their political conviction (from Sicilian independentism, to fascism).

2.1.1 Luigi Mauceri

The first published account of archaeological excavations in the Fusco Necropolis dates to the late XIX century, and was written by a local engineer, Luigi Mauceri.

In 1877 the *Annali dell'Instituto di Corrispondenza Archeologica* published a letter sent by Luigi Mauceri to Wolfgang Helbig, that described the remains found at the Fusco necropolis during what was at the time the most recent excavation. The article included tables (Fig. 14a - b), which showed some of the most emblematic artefacts.[58]

Figure 14 – Plate AB (a) and Plate CD (b) from Mauceri 1877.

[58] Mauceri 1877.

Wolfgang Helbig was at the time the vice secretary of the Instituto di Corrispondenza Archeologica (today Deutsches Archaeologische Institut) in Rome and the editor of its journal. The discovery reached the national and international academic community. A second edition of the letter, published as a short monograph in Palermo in 1878, was specifically meant to address the Sicilian academic *milieu*.[59] Luigi Mauceri was a civil engineer involved in the development of the early Sicilian railway lines. He was the brother of a famous art historian, Enrico Mauceri.[60] He had an eclectic mind and often wrote about archaeological discoveries[61] and valorisation.[62]

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[59] Mauceri 1878. This monograph was consulted – in a precarious state of conservation – at the Civic Library of Siracusa in April 2019. It is the only know copy of this book to be conserved and the library does not have a digitization project, nor a scanning service. Therefore, a photographical reproduction of this volume has been made by the author, with the agreement of the library, in order to ensure its preservation.

[60] The life and work of Enrico Mauceri was at the centre of a recent Conference (held in Palermo in 2009). The proceedings (La Barbera Bellia 2009) provide a thorough account of many of his interests, painting an interesting intellectual biography.

[61] Besides the Fusco necropolis, he wrote a 1880 volume on the discovery of tombs between Licata and Recalbuto, in 1896 he published a 'pelasgic' necropolis at Himera and in 1928 he published a survey of the ruins of the Eurialo Castle.

[62] As an engineer, he wrote about the urban planning – and what we would now call requalification of the urban area – for Siracusa (1910) and Messina (1909).

The first part of the letter consists of a short account of previously undocumented and unpublished excavations, which were usually the result of accidental findings during public construction work. According to Mauceri, the first tomb of the Fusco Necropolis was found in 1842 when some sarcophagi were uncovered during the cutting of the tuff bank for the construction of a road. Local intellectuals bought the artefacts: "vasi di terracotta e di rame – non trovandovi quei caratteri tanto diffusi dell'arte greca, lo credettero un sepolcro isolato, di poca entità e quindi non si curarono di farvi ulteriori studî".[63] It is worth noting that proto-Corinthian vases, and geometric pottery in general, had not yet been found in great quantity and were at the time just starting to be identified and studied.[64] Therefore

[63] The quote is taken from p. 3 of the 1878 book. Translation: intellectual who bought "terracotta and bronze vases did not find in them the typical characters of Greek art that are so common. Therefore, they believed it to be an isolated tomb of little relevance and did not study it further".

[64] The book written by Olivier Rayet in the 1880s and edited by Maxime Collignon for posthumous publication says : « En écrivant ce volume, nous avons eu surtout pour objet de retracter l'histoire de la technique et du style des vases grecs ; c'est le point de vue auquel les archéologues ont commencé à se placer depuis une vingtaine d'années seulemet » (Rayet, Collignon 1888, p. III). On geometric pottery only Conze (1870) and later Dumont (1888), have published extensive accounts. Helbig (1875) published a long letter, addressed to Conze, on geometric decoration in the *Annali dell'Istituto* two years before Mauceri's letter was published in the same venue.

local antiquarians did not recognise the grave goods as 'typical Greek artefacts'.

Mauceri recalls that in 1868 the Fusco terrace was excavated to quarry tuff for the construction of a railway line: soon the workers found more tombs. Gioacchino M. Arezzo, friend of Mauceri and director of the Regional Archaeological Museum, recovered the ancient artefacts from the tombs and purchased them at his expenses to enrich the collections of the Museum.[65]

In 1871, Saverio Cavallari – who was 'Direttore degli scavi' (director of the excavations) of the Sicilian Commission for Archaeology and Fine Arts (CABAS)[66] – came to Syracuse because thermal baths had been discovered in the neighbourhood of Acradina. During his visit, he saw the

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[65] Mauceri 1878, p. 3. As mentioned by Momigliano (1984a, 123-124) in his essay on the rediscovery of ancient Sicily, aristocracy always had a crucial role in the excavation and conservation of Sicilian antiquities. This role was recognized very early by the Bourbon monarchy. The Museum of the Seminary was founded in Siracusa in 1780 and it became the Civic Museum in 1808, while staying in the Bishop Palace. After the unification of Italy, it became the National Archaeological Museum of Siracusa in 1878. On the birth and the history of the Archeological Museum of Siracusa see Nicoletti 2017. On archaeology and conservation in Sicily under the Bourbon government, several papers were presented at the Conference 'I Borbone in Sicilia' held in Catania in 1998: see in particular Salmeri, D'Agata 1998, Iozzia 1998 and Spigo 1998.

[66] See Pelagatti 2001 on the history of CABAS and its transformation after the unification of Italy, with previous bibliography.

vases found in the Fusco Necropolis at the Archaeological Museum, and expressed the desire to conduct a systematic investigation there "per determinare l'estensione e la specialità dei vasi".[67] The excavation started in 1871, but it was soon interrupted by the land-owner's resistance and it could only be resumed in 1874. Mauceri's letter describes the findings of the two excavation campaigns. His account is particularly important because the artefacts found in the 1871 campaign, collected by Cavallari and stored at the museum, were lost three years later. Therefore, his description is the only thing we have left of them.

Mauceri provides a general description of the necropolis. The tombs are usually oriented east-west and consist mostly of tuff sarcophagi inserted into a trench which was cut into the bedrock. The stone of the sarcophagi is more compact than the tuff of the Fusco terrace, so the author argues that they might have been quarried in Acradina or Plemmirio. The covering slabs seem to be of a different material, which the author thinks might come from the Temenite mount, on the edge of the Fusco area. There are three main types of tombs: a) a sarcophagus made of one piece of stone is placed inside the trench and any remaining space is filled with earth and dirt; b) a second trench is cut beneath the stone case, connected to it through a hole in the lower part of the sarcophagus; c) the sarcophagus is directly cut into the rock,

[67] Mauceri 1878, p.4.

with a cavity to the east for the head or maybe for the main vase (the contexts were disturbed, therefore the author cannot be sure). All the types are normally covered by around 60cm of humus. Remains of small pillars and antefixes were found on surface levels, inducing the investigators to believe that they used to belong to funerary stele. At this stage, no walls had been found.

In this general description, Mauceri offers only two very limited chronological considerations, but relevant ones: a) cremation and inhumation are contemporary in the Fusco necropolis; b) vases were "di svariate forme, di un carattere speciale, e la cui tecnica insieme all'ornamentazione, accennava ad un'alta antichità".[68]

Let us start from the first chronological consideration. The contemporaneity of cremation and inhumation is deduced from one specific sepulchre: in one trench excavators found two stone cases, one being the cover of the other; the upper sarcophagus was closed with a large tuff slab, with two terracotta idols on the side; inside the first sarcophagus there were burned bones, two 'copper' vases and several small vases; the teeth of a male adult were visible in the larger vessel and several bird bones lied all around; a bucchero cup was found here as well. In the lower sarcophagus, excavators found the rests of a inhumated body, "che

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[68] *Ibidem.*

servivano a dare luminosa prova della contemporaneità dei due sistemi di seppellimento in uso in quell'antica necropoli".[69] This assertion went against the idea, that was often used in archaeology at the time, that different burial customs were linked to different ethnic groups which, like waves, occupied geographical areas in successive sequence.[70] This notion was pivotal in the construction of the so-called 'teoria pigoriniana', a theory on the peopling of ancient Italy, which took its name from Luigi Pigorini, the most influential Italian prehistorian of those days. In this regard, it is particularly interesting that Mauceri's letter was addressed to Helbig. Notoriously, Helbig was in constant contact with Pigorini and was the first to propose that the inhabitants of the *terramare* were the first 'Italic' people in the ethnographic sense. Several studies have now ascertained that what is now known as 'teoria pigoriniana' involved both Helbig and Pigorini equally: their correspondence shows that they discussed these ideas at length and each one published several works on the topic.[71] One of the first

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[69] Mauceri 1878, p.8.

[70] For a detailed account of the history and methods of funerary archaeology in Italy see Nizzo 2015 with extensive bibliography. See in particular 27-46.

[71] See Guidi 2011 for a publication of their correspondence. Among the several papers devoted to Helbig's role in the development of the 'teoria pigoriniana' and the personal relationship between Pigorini and Helbig – all thoroughly cited by Guidi 2011 – two publications are particularly relevant: Peroni 1992 and Pearce, Gabba 1995.

mentions of *terramaricoli* as 'the first Italic people' can be found in Helbig's letter to Conze – which would later issue as a monograph – on geometric decoration, published in the *Annali dell'Istituto* in 1875, two years before Mauceri's letter. In his essay, he argued that geometric decoration did not come from the Aegean, as previous scholarship had assumed, but was one of the traits of the indigenous people of Italy.[72] With that in mind, one should start to analyse Mauceri's second chronological statement on the Fusco necropolis.

His second chronological argument concerns the vessels found in the tombs.[73] Based on their 'form', their 'character' and their 'technique' he believes them to be very old. Mauceri gives a list of artefacts found in the excavations, with their measurements, a thorough description of their shape, and an assessment of their technique (mainly the kind of clay and the colour of the paint) and decorative patterns. Among the many vessels, only a small number deserve some attention, with respect to the Fusco Necropolis and its role in subsequent chronological disputes. Their relevance will be clearer in the closing lines of the letter, where the author

[72] Helbig 1875.

[73] He also lists some metal objects, as well as bone and glass artefacts, but the focus of his letter is clearly on ceramics (Mauceri 1878, pp.122-124).

poses some questions on the art of Greek colonies.[74] Among the many described artefacts, one should remember that Mauceri mentions one bucchero vase, describing it as elegant and heavy. But only one vase seems to have been decorated with a human figure: an '*alabastron*' (today we would call it a pyriform *aryballos*, n.d.r) depicting a naked man striking a rampant lion with a spear. The author describes the vase as decorated with black figures and reddish details. He also advances the hypothesis that the scene depicts one of the first known representations of Heracles fighting the Nemean Lion (Fig.14a).[75] Another vase, a 'patera of peculiar shape' with two handles (Fig. 14a) and geometric decoration, seems interesting as well: after describing its decorative patterns (a bird in the central space, lines and lozenges to the sides, triangles in the lowest part), he makes a comment suggesting that this vessel is a later imitation, not an actual geometric vase. "Io ritengo che questa patera non appartenga alla vera era dello stile geometrico, ma piuttosto sia una riproduzione di maniera già in disuso per le influenze dell'arte nuova orientalizzante".[76] A chronological determination is also expressed by the author regarding a 'small vase of spherical shape' with a very complex decorative scheme (which

[74] The questions are not included in the 1877 version of the letter: they can only be found in the 1878 monograph.
[75] Mauceri 1878, p. 11.
[76] Ibidem, p. 14.

unfortunately is not illustrated in the plates). He compares its decoration to an amphora from Caere, based on the likeness of animals and monsters decorating the two vases : "in questo vaso (i.e. the amphora), benché meno antico dei nostri, non è meraviglia trovare questa relazione, giacché si sa ch'esso appartiene alla seconda epoca dello stile corinzio, e perciò vi fa ancora capolino l'arte orientalizzante nella sua prima maniera, a cui generalmente appartengono i vasi fuscanici".[77] This last sentence allows us to understand Mauceri's position on the chronology of the Fusco necropolis: most vases found in the Fusco area belong to the first Corinthian style (while the amphora from Caere, which is more recent, belongs to the second Corinthian style) bearing the influence of the 'prima maniera' of Orientalising art.[78]

The abovementioned artefacts prompt some general reflections that the author only includes in the 1878 monograph. They concern two interconnected aspects: the development of a national Greek art (and how it relates to

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[77] Ibidem, p. 19.

[78] He refers to the system proposed by Conze in 1870. Corinthian vases were known as *necrocorinthia* since the Roman occupation of Greece as precious objects obtained from the looting of tombs: among others see Strabo, VIII, xi, 23; Cicero, *Paradoxes*, V. 3; Svetonius, *Augustus*, 7. Helbig (1877) was the first to bring attention to proto-Corinthian vases, i.e. older than Corinthian vases, from the same pages of the *Bullettino dell'Instituto* where Mauceri's letter was published.

the western colonies), and the need for linking the several chrono-typological sequences of Greek artefacts through synchronicities and cross-dating in order to build a shared chronological grid, which "può giovare in qualche maniera agli studî sintetici, sullo sviluppo dell'arte antica".[79]

First, the author draws comparisons between places presenting similar necropolises and/or similar artefacts "di stile rettilineo e orientalizzante".[80] Both in Sicily and on 'the Italian continent'. The first element to emerge is one of national identity. He writes "A Selinunte Cavallari messe in luce la necropoli di Galera e Bagliazzo, che sta alla necropoli di Manicalunga come il Fusco sta alle necropoli realmente greche di Siracusa" (Fig. 15).[81]

When mentioning the Manicaluga necropolis – as well as the 'really Greek tombs of Syracuse' – the author refers to the tombs where excavators found the so-called 'vasellame greco-siculo',[82] which according to Mauceri was the true national Greek art.

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[79] Mauceri 1878, 25.

[80] *Ibidem*, 29.

[81] *Ibidem*.

[82] For a clear summary of the necropolises of Syracuse with a thorough account of available bibliography see Musumeci 2006.

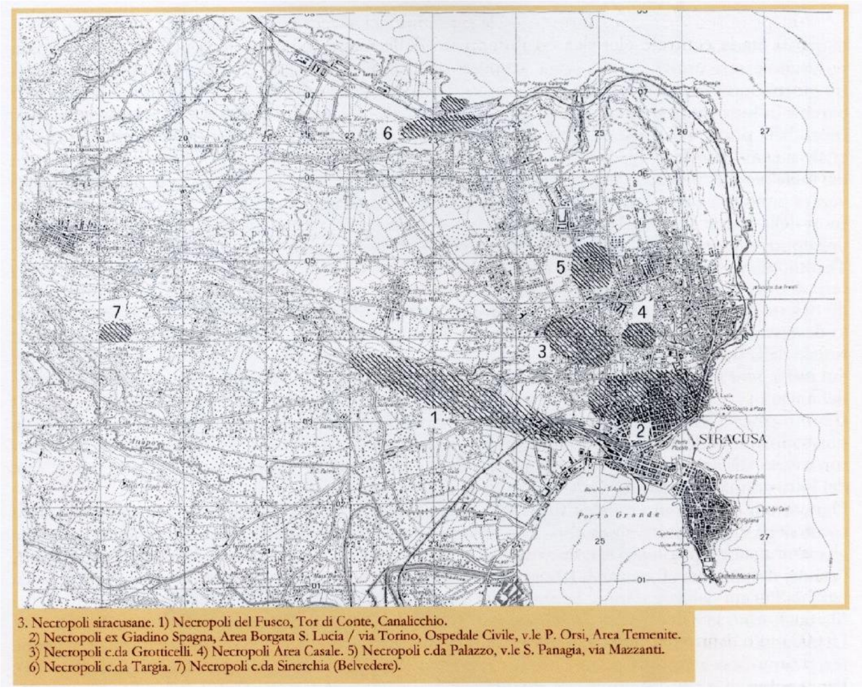

Figure 15 – The cemeteries of Syracuse (from Musumeci 2006, 6).

The affinity between Syracuse, Selinus and Megara is his main argument: he notices that monumental sculpture from Selinus presents stylistic similarities with figurative terracottas from Magara and Syracuse, arguing that they are the same age as the vases that he just described.[83] Let us look

[83] The first three metopes of Temple C were excavated by the Englishmen William Harris and Samuel Angell in 1823; Temple B was later investigated by Cavallari by mandate of Domenico Lo Faso, duke of Serradifalco. The main available work on the topic was Benndorf 1873. The path leading to the first systematic excavation in Megara Hyblaea is drawn in a recent study with extensive bibliography (Bérard

at his argument in more detail. In the museum in Syracuse there are some ‘small idols’ (votive terracottas, ndr) in Phoenician and ‘Aeginetic style’ (i.e. resembling the pediments of Aegina): small masks, sitting and standing figures of different kinds. The traits of one type present strong similarities to the images of Arthemis and Athena on the oldest coins of Syracuse. In the cavity of one of these statuettes, from the necropolis of Megara, Mauceri found fragments of vases that he deemed identical to the ones found in the Fusco necropolis and described in the first part of the letter. Therefore, this kind of votive terracottas and this kind of vases had to be contemporaneous. This reasoning mixes stylistic analogy (artefacts that look the same, in Megara and Syracuse, are part of the same group) and contextual association (two groups of artefacts were found together, therefore they are contemporaneous). Moreover, Mauceri mentions that Prof. Kekulé, during excavations in the necropolises of Megara, had found linear and Orientalising artefacts, similar to those recovered from the Fusco necropolis, in the same context as first attempts at ‘actual Greek vase-painting’. These vases were bought by the Archaeological Museum of Syracuse and they were stored there, but their distribution inside the tombs is not registered. Therefore, when Mauceri mentions the

2016). Though he does not mention it in the footnotes, this connection is taken from an article published by Cavallari a few years earlier (Cavallari 1873a).

'association' of different artefacts, he does not mean 'closed find'.[84] Other vases, similar to the ones found in the Fusco necropolis, were found in smaller numbers in Gela, Acre, Lentini, Imera and Girgenti.[85] From Himera we also have several beautiful 'greco-siculi' vases, found at that time.

After these comparisons, Mauceri can draw three main chronological conclusions: 1) "il gran numero di idoletti e di maschere scoperte a Megara sono contemporanee ai vasi di stile rettilineo e orientalizzante e, avendo relazione con le sculture monumentali di Selinunte e colle rappresentazioni delle più antiche monete siracusane, ci danno insieme alle stoviglie l'intero carattere artistico di un'epoca",[86] which means that his work is somewhat a search for the *Kunstgeist* of the era. 2) Before the destruction of Megara (traditionally placed, according to Thucydides, sometime before 480 BC) the new 'Greek national art of vase-painting' had started making its first steps. 3) Between the destruction of Megara and Himera (ca. 480-410 BC) red-figured painting develops in Sicily. Right here we have the three main elements that will be crucial to the understanding of the Fusco necropolis and its chronological relevance until now. The site finds itself at a crossroad between historical sources and ceramic sequences (established mostly on stylistic principles) and

[84] On 'closed find' (also called Worsaae's law) and its early use see Thrane 2008. Also, *vide infra* 252-280.
[85] Mauceri 1878, p. 34.
[86] Mauceri 1878, p. 33.

allows the synchronisation of different artefact types based on stylistic resemblance (e.g. votives and coins) or on their co-occurrence in an archaeological site (the information is seldom stratigraphic or comes from sealed contexts).

Then, he recalls all the contexts of the Italian peninsula where, to his knowledge, similar vases ‘rettilinei e orientalizzanti’ were excavated: he mentions Cumae, Capua, the necropolis of the Esquilino in Rome, then Albalonga, Perugia, Vulci and Chiusi. In general, Mauceri reminds the reader that Helbig, during a conference at the Instituto, had noticed that in Etruria these ‘linear and Orientalising vases’ are found together with bucchero vessels. Also at the Fusco necropolis, one bucchero vase was found in association with those vases ‘rettilinei e orientalizzanti’.[87] Mauceri recognises that the latter are mostly found in Corinth: they derive their name from this town, and most scholars assumed that it was also their place of manufacture. However, Mauceri is not convinced that the attribution of these vases to Corinth is correct. He mentions similar vases that had been found in Athens, beneath the bastion of Kimon. Finally, relying on Homer (Odyssey XIX, 226ss and XI, 609ss) and on a single fragment of vase found in Koyundschik, he suggests that Asia Minor is the production centre of this vascular style. Moreover, openly following Winckelmann, Mauceri identifies the influence of Egyptian art in the productions of

[87] Mauceri 1878, p. 35-36.

this era: indeed, Winckelmann wrote that the female heads on the tetradrachm of Syracuse had an Egyptian influence.[88] As a result – as for Mauceri one period has a peculiar 'artistic character'[89] which transcends the kind of artefacts, their use and practicalities – this Egyptian influence informs all artistic productions in said period.

At the end of his letter, Mauceri poses three questions which the new discoveries of the Fusco necropolis, according to him, can contribute to answer. 1) During the VII century, the vases 'di stile rettilineo e orientalizzante' – which in Greece follow the geometric ones – are commonly found in Etruria, in Lazio and in the Greek colonies. Are they imitations or imports? 2) Do the statuettes, coins and vases found in Acre, Syracuse, Megara Hyblaea and Gela form 'the artistic heritage of an epoch'? Or do they only represent a partial view of it because of commerce and co-existence of different races? 3) Did the new 'national Greek art' (i.e. black-figured and red-figured pottery) come from Greece to the western colonies or vice-versa?

In order to offer some thoughts on the first question, the author decides to divide the vases of the 'stile rettilineo e

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[88] Winckelmann 1784 [1764], 493.

[89] The concept of *Kunstgeist* – the spirit of the art of a people in a certain period – was introduced by Schlegel, whose Vienna lectures were translated to Italian in 1817. It was one of the main tenets of Romantic aesthetics.

orientalizzante' in seven groups, based on the type of clay used to produce them. This is for him the criterion for determining the production place of a pot: "in tal modo, trattandosi di opere contemporanee, avremo un solo criterio per distinguere le varie provenienze".[90] So, the style determines the chronology and the technology defines the provenance. The seventh group – the one of 'vasi rettilinei e orientalizzanti' *sensu stricto* – has thin walls; vessels of this group were done on the wheel, the clay was washed and decanted to obtain a white-yellowish colour and they sometimes presented a thin layer of fine clay on the outer surface; stripes and figures were drawn with confidence, with incised details and painting of *ochra ferri lutea* pigments; final vase colour variations depend on the position of the vessels in the kiln. The author argues that for their technological uniformity, these vases share the same provenance and "se mai vi si potrà scorgere qualche differenza […] parmi ciò debba attribuirsi a piccola varietà di tempo anziché di provenienza".[91] The vases of the seventh group are often found in association with local productions and, in Etruria, often with bucchero. Mauceri recognises that Helbig has been the first to see that bucchero, in Etruria, was a later production than geometric vases and that it was inspired by metallic ware.[92] However, in contrast to Helbig,

[90] Mauceri 1878, 39.
[91] *Ibidem*, 42.
[92] Commenting the findings of Capua: Helbig 1871, 1872, 1873, 1874.

the author claims that bucchero is an exclusively Etruscan production: it could not be manufactured all over Italy, otherwise different people should share the same ‘creative concept’ which appears unlikely, especially for contexts where metals were less common than Etruria. Consequently, Mauceri thinks that the bucchero vases found in Sicily are imports. Indeed, at the roots of the disagreement rests the very problem of whether one whole people – the Italic people – assumed different forms all over the peninsula and the islands, or whether the Sicilians have a special autonomous status, a by-product of the endless series of invasions, communications, racial mixing: a dichotomy that will traverse the history of the discipline.[93]

This consideration is also relevant to the second question, on the ‘artistic heritage of the epoch’. The sites he mentions in the comparisons’ section are among the earliest colonies to be founded and Mauceri provides a chronological grid to facilitate the reader[94]:

Megara Iblea (Olimpiad X, I year) = 736 BC

Siracusa 1 year after Megara (Olimpiad X, II year) = 735 BC

Leontini 7 years after Siracusa (Olimpiad XII, I year) = 728 BC

Gela 45 years after Siracusa (Olimpiad XXII, III year) = 690 BC

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93 Momigliano 1984a.

94 Mauceri 1878, 39-40.

Acre 70 years after Siracusa (Olimpiad XXVIII, IV year) = 665 BC

Imera (Olympiad XXXII, III year) = 650 BC

Selinus 100 years after Megara = 636 BC

At the time when the excavated section of the Fusco necropolis was active, the Orientalising style was widespread in Greece. Mauceri believes that, before acquiring the necessary technical skills to produce their own wares, the colonists bought them from Greece. Therefore, the complete 'artistic heritage' of this age is comprised of a mix of Greek and, less numerous, Etruscan imports, with some luxury vases from Egypt. At the end of this initial period, some artefacts of local production started to appear: the coins of Syracuse, the votive terracottas of Megara and the metopes of Selinus. The period is surprisingly long: it spans from the VII to the VI century BC, as it goes from the first *apoikiai* to the minting of the first coins of Syracuse (550 BC.) As no coins were found in the necropolis, Mauceri infers *ex absentia* that its use preceded the first coins' emission.[95]

While this consideration seems to gravitate towards the 'Sicily land of many peoples and conquerors' *topos*, the answer to the last one of the three questions shows a considerable amount of chauvinism and, interestingly, uses a chronological argument to argue for the primacy of Sicily in the production of what Mauceri calls 'the new Greek national art'. In fact, Megara was destroyed at the beginning

[95] *Ibidem*, 41.

of the V century by Gelon. As some *lekythoi* were found in the necropolis Megara, their production had to predate the beginning of the V century BC. But according to Dumat, *lekythoi* could only be found in motherland Greece at the end of the V century BC. Therefore, the type must have been developed in Sicily and transmitted to Greece afterwards. He concludes that, in Greece, the Orientalising style started to introduce the human figure and narrative representations, while, in Sicily, the colonists invented the 'national Greek art' and introduced new shapes such as the *lekane* and the *kelibe* (i.e. column krater), through their contacts with the Sicels, and especially with king Iblon in Megara. Once again, the peculiarity and national pride of Sicily resides in its multiculturalism. Mauceri also collects a number of literary sources to back up his 'tale of priority': from Diodoros (he says that Dedalus arrives to Sicily with a potter's wheel) to Athenaeos (stating that the *kottabos* was invented in Siracusa and that Agatocle used to mix golden and silver vases with beautifully decorated pottery). [96]

This is just one case of several where chronology answers to questions of priority and originality, in order to address the question of group identification and nationalism.[97] In this instance, it has to be seen in the context of complex dynamics between multiculturalism, localism and nationalism, which

[96] *Ibidem*, 42-43.

[97] Origin and priority were at the base of the first dating campaigns conducted through radiocarbon dating: *vide infra* 304-308 and 351-355.

has for centuries constituted the main subject of Sicilian historical scholarship.

Momigliano argues that Sicily did not take part in the national re-evaluation of the Greek past that was done in Greece.[98] This study starts to complicate this notion, showing that the 'national character' was indeed an object of interest in Sicilian archaeology, but more than identification there was an interest in the – geographical and chronological, but not yet social – interaction between different 'national groups' and their artistic expressions.[99]

2.1.2 Francesco Saverio Cavallari

In the first decades after the unification of Italy, Francesco Saverio Cavallari was the preeminent personality in Sicilian archaeology. He was the director of excavations for the CABAS (Commission of Archeology and Fine Arts of Sicily) and he superintended many archaeological campaigns, both for accidental findings resulting from public construction works and for systematic archaeological campaigns. He was

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[98] Momigliano 1984a and 1984b.

[99] This topic will be highlighted throughout the chapter: it remains a constant aspect of Sicilian archaeological scholarship, sometimes leaning towards local autonomy and sometimes towards fascist nationalism. This is also one of the main themes in the work of scholars such as E. Pais, B. Pace and L. Pareti, which will not be discussed in any detail here, but is crucial to the academic discourse on Sicilian antiquity.

an engineer, but he had been trained in archaeology by the Duke of Serradifalco, one of the central figures of XIX-century Sicilian archaeology.[100] He had also collaborated with the German art historian Heinrich Wilhelm Schulz, drawing the plates for the publication of the *Denkmäler der Kunst des Mittelalters in Unteritalien* (Dresden 1860): the job entailed travels to Rome and all around Southern Italy. In 1840, he began a long-lasting collaboration with German geologists who were studying the Etna volcano, first in Sicily and then moving to Göttingen for 5 years.[101] Cavallari's first archaeological topography of Syracuse was written in German in 1845, nearly 40 years before the publication of the Italian *Topografia Archeologica di Siracusa*. The Italian book, however, includes a great amount of additional data collected during several years of excavations and entire sites that had not been discovered in 1845, among them the Fusco necropolis. It is, to this day, the best topographical account

[100] A recent biographical profile of Domenico Lo Faso Pietrasanta Duke of Serradifalco has been written by Ettore Sessa (2018) with extensive bibliography; Momigliano 1984a and Salmeri 1992a, 72-73 contextualise him in his intellectual *milieu*. Fatta, Ruggieri Tricoli 1983 highlight the similiarities and the differences in the intellectual approaches of Serradifalco and his pupil Cavallari. It sould be noted that Cavallari drew most of the plates of Serradifalco's successful volume *Le Antichità della Sicilia* (1834-42).

[101] He was very connected to German culture which, according to Momigliano (1984a), became dominant in Sicilian archaeology in the XIX century. Indeed, the historical section of the *Topografia di Siracusa* was written by A. Holm.

of ancient Syracuse, and the base for any modern addendum. Returning to Sicily for the insurrection of 1848 and after the failed attempt at independence, he served several years as professor of Decorative Architecture and Topographical Drawing at the University of Palermo. He left in 1854 because of disagreements with the Bourbon government. After teaching at the Accademia di Brera in Milan and at the National Academy of Fine Arts San Carlos in Mexico, he only returned to Sicily in 1864 after the unification of Italy. Here he held various titles, being substantially in charge of all archaeological excavations until 1891.[102]

We know from Mauceri that Cavallari first excavated the Fusco necropolis in 1871. The excavation was interrupted soon after, due to the reticence of the landowner. In 1874, the excavation resumed, resulting in the discovery of the artefacts described by Mauceri and discussed in the previous paragraph.[103]

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[102] The short biography is derived from Cianciolo Cosentino 2012a. For an in-depth account of his life and work see Cianciolo Cosentino 2007. The essay of Cianciolo Cosentino 2012b frames his biography in the intellectual context of XIX-century Sicily through the correspondence between Amari and Cavallari.

[103] Mauceri 1878. Cavallari reported the news of the excavation with some descriptions in the *Bullettino della Commissione di Archeologia e Belle Arti della Sicilia* (CABAS).

In 1881, the construction of the railway from Licata to Syracuse led to the discovery of new tombs from the Fusco necropolis, near the town railway station. At the time Cavallari was writing with Holm the *Topografia Archeologica di Siracusa*, which is why he only provided a short note on the excavation in the *Notizie degli Scavi.* He states that the vases found in the necropolis were the ones commonly called 'Corinthian', without human figures, but with lions, tigers, sphinxes, and small roses on light-yellow clay. He states that they were very similar to the ones he found in Selinus, Gela, Camarina and Acre. In this brief essay, he argues for the first time that the Fusco necropolis was probably the oldest necropolis of Syracuse, a statement that will soon become widely accepted, with many consequences on its centrality for the construction of Mediterranean chronology. He even proposes the idea that the vases found in the tombs of the Fusco were brought by Archias and his companions from the lively *emporion* of Corinth, where Phoenicians, Ionians and Dorians, as well as people from Chalcis and Asia Minor would set a base for their exchanges. [104] His main argument is topographical in nature: it relies on the distance between the Fusco terrace and Ortigia, the island where the first Greek settlement was located, ca. 1 km. Besides human bones and Corinthian vases (mostly found in disturbed tombs), the excavations of 1881 revealed a wall

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[104] Cavallari 1881, p. 446.

and a painted tuff pavement, tentatively identified as the remains of a small temple. Moreover, the remains of a pressed earth pavement, moving in the direction of the Temple of Olympian Zeus, led him to advance the hypothesis of a *via sacra* connecting Ortigia to the temple. Another large wall was found, which was made of roughed out blocks, and in some areas formed a double row of blocks. The direction of the latter was transversal, towards the swamps of Epipoli. Cavallari supposes that the wall may have been made by the Athenians when sieging Syracuse during the Sicilian expedition.[105] The attribution of the walls to one of the many fortifications mentioned by the sources will be one of the main concerns of Cavallari in his study of the Fusco necropolis. While it will not impact the chronological framework of the site, it can be useful to shed light on one of the two main characteristics of Cavallari's work that are relevant to our argument: he feels the need to match the archaeological evidence with textual sources, following a tradition that, at the time, had been central to the debates in biblical archaeology.[106]

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[105] *Ibidem*, 449.

[106] In the Appendix to the *Topografia Archeologica di Siracusa,* Cavallari (1891) wrote: "[…] per noi dedicati a raccogliere quei dati topografici che servir dovevano a porre in evidenza i fatti in armonia con la storia" p. 8. A mordant but informative account of the relationship between (sacred) texts and archaeological methods in biblical archaeology can be found in Mazar 2005.

In 1883 the *Topografia Archeologica di Siracusa* was finally published. It soon became a work of reference for studying the ancient town. Francesco Saverio Cavallari worked with his son Cristoforo to complete the task; the fifth chapter, on 'the history of the topographical development of Syracuse', was entirely written by Adolf Holm, professor of Universal History at the University of Palermo.[107] Among the three possible dates for the foundation of Syracuse he chooses 734 BC, mostly based on his faith in Thucydides' reliability.[108] The historian locates the first settlement in the island of Ortigia. From a series of different textual sources and etymological interpretations, he also argues that the place was not empty when Archia and his companions arrived:

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[107] The historiographical work of Holm, and especially the three volumes of *Geschichte Siciliens im Alterthum* (1869, 1874, 1897), were extremely influential on Italian scholarship, especially after their translation in 1896 and 1901. The impact of the *Altertumswissenschaft* on Italian scholarship - with the arrival of Holm in Palermo, Beloch and Löwy in Rome - is the object of several studies. See at least Treves 1962, Settis 1989, and most recently Harari 2014 and Frisone 2018. The latter explores the impact of the "German protectorate" (using the words of Barbanera 2015) on Italian archaeology through the exemplary figures of Cavallari and Salinas. While her opinion of the former is somewhat reductive – she even suggests that Holms had the role of a 'scientific director' for the *Topografia di Siracusa* – in Salinas she sees the example of an 'Italian way' for the study of antiquity, influenced by German education but applied to the preservation and valorisation of the newly unified Italian state.

[108] In *Storia della Sicilia nell'antichità* (vol. I, 381-385), he discusses the problem in more depth, but his arguments are mostly intended to verify Thucydides' reliability.

the Sicels had already settled on Ortigia;[109] moreover, according to him, it was likely that either Ortigia or the mouth of the Ciane river served as a Phoenician emporium and that other Greeks – possibly Etholians – had already touched these shores. He mentions that archaeological traces of these pre-existing populations are to be found, among other places, at the edge of the Fusco terrace, in the *grotticella* tombs dug in the rock wall bordering the necropolis, near the 'portella del Fusco'. Holm builds a comparison between these tombs, the ones in Plemmirio and the ones of Pantalica, which were already well-known at the time.[110] Then he goes on discussing the evolution of the topography of Syracuse until the siege of Marcello.

The sixth chapter, which is a description of selected ancient monuments, was written by Francesco Saverio Cavallari himself. He warns the reader that the order in which the monuments are being covered does not follow the plates of the Atlas that complements the volume. Instead, they are ordered chronologically. Indeed, writing in chronological order and establishing causal relationships was typical of works that wanted to be called 'historical' instead of 'antiquarian'.[111]

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[109] The excavations in Piazza Duomo have confirmed the presence of religious buildings of the Siculi (see Voza 1999).
[110] Holm 1883, pp. 143-148.
[111] Momigliano 1984c, 5.

It was already known at the time that Syracuse had several necropolises, mostly included in the territory of the five districts of the town (in ancient times, the town was called the 'Pentapolis'). As Cavallari follows the assumed chronological sequence, the Fusco necropolis comes first. For Cavallari it is "indubitatamente la più antica di Siracusa"[112]. While he is not explicitly stating the reason for this attribution, he does mention that archaeologists had only found vases with animal figures and no human representation[113] - the exception being a large *kelibe* (i.e. column krater) depicting warriors, which was found in the area at the time:[114] according to Cavallari, this vase belonged to a transitional era when figures were introduced in pottery decorations. In the rest of the book he does not discuss the Fusco necropolis, and especially its exact chronology: he describes the form and the technique of the graves, giving thorough measurements and establishing comparisons with some other Sicilian sites.[115] However, he does not seem to attribute chronological relevance to the typology of the tombs.[116]

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[112] Cavallari 1883, p. 340.

[113] Mauceri, however, refers to an *alabastron* representing a hero fighting a lion, *vide supra* p. 123.

[114] Cavallari 1883, p. 340-341.

[115] *Ibidem*, pp. 341-345.

[116] It is interesting to notice the objects that bear chronological significance are mostly pottery. Our selection of chronological indicators

From 2 September to 4 October 1884 a new archaeological campaign was conducted in the Fusco necropolis, whose preliminary results were presented by Cavallari in the *Notizie degli scavi* of the following year.[117] Providing a general description of the necropolis, the author notes that "Nei siti più vicini all'isola di Ortigia si trovano vasi corinzii, con piture di animali; nei siti più lontani poi furono raccolti vasi di stile attico del V secolo a.C." [118] And further away, in the locality of Galera, Roman fictile objects were found. The presence of Attic vases had never been mentioned before. But, most importantly, one can find here an attempt at what is inaccurately called 'horizontal stratigraphy': the topographical position of the tombs, together with the types of artefacts, concur to illuminate the progression and expansion of the necropolis, so that the sequence is not only temporal, but it is given a spatial dimension as well. He then provides a thorough description of the main excavation highlights. They can be summarised as follows. The burials

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normally depends on a prejudice that certain things/characters/variables are more likely to change over time, mirroring to the 'spirit of the age'. This is the case for art, which in archaeology necessarily turns to pottery, as we do not have large Greek paintings. The fact that he considered pottery part of the history of art can be shown through a passage of his 1885 paper: "le indagini in questa zona del territorio siracusano, non forniscono soltanto la suppellettile funebre, ricca di vasi arcaici; ossia non producono materiale utile solo per la storia dell'arte […]" (Cavallari 1885, 50).

[117] Cavallari 1885.

[118] *Ibidem,* 49-50.

found in the first part of the necropolis, on the edge of the tuff terrace, had been disturbed by agricultural works. By contrast, intact graves were found in the central terrace area, which were documented in detail. The guardian kept an excavation diary and Cavallari made drawings, recording their form and orientation, the position of the skeletons and the grave goods. Unfortunately, these sketches remain unpublished.[119] The *loculi* were carved into the rock – which is located ca. 80cm beneath excavation walking level – and they are arranged east-west: the skeletons are oriented the same way. Larger tombs house a sarcophagus and have a recess for the cover, which is usually made of three tuff pieces. Two of these larger tombs contain a cylindric space carved in the rock on one of the short sides: it was probably a space for the ossuary and/or for large vessels. These two sepulchres had both been disturbed, therefore the author could not give more information. In one large tomb, under the untouched tuff cover, a large vase full of combusted human bones was found. Cavallari describes another, very large, tomb that was found in the Fusco necropolis: it was covered by four tuff pieces, one of which had three holes in it. Inside the recess in the bedrock was a large sarcophagus

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[119] The Soprintendenza Archeologica di Siracusa owns some of the archive of Cavallari. The remaining correspondence, drawings and notes are kept at the Cavallari fund in the Biblioteca centrale della Regione Siciliana in Palermo. The author was not able to consult these archival documents.

made of a single piece of stone, with a cover also made from a single stone slab of tuff and with four *acroteria* at the edges. To the west of the tomb there was a semi-circular hole carved into the rock: it was full of fragments from what had to be a huge vessel. Inside the sarcophagus was only the skeleton, with his head towards the east. No ancient artefacts were found here. On the outside, between the external walls of the sarcophagus and the cut in the bedrock, several ceramic fragments were recovered. This is not unprecedented at the Fusco necropolis. But this did not prompt Cavallari to offer any ritual explanation: he mostly kept description and interpretation separate in his writings.[120]

In the same area, the excavation led to the discovery of a line of large tombs, one of which immediately appeared relevant. It was a large grave covered by three pieces of non-local white tuff with three holes in the central slab. After removing the central stone of the cover, the investigators went down inside the tomb and, at first, it seemed empty: they only collected a *bombylon* of glass paste with lozenge decorations. On three sides the sepulchre was cut into the bedrock, but the fourth wall was made of a tuff slab. Proceeding with the exploration, they saw that at one edge

[120] E.g. Cavallari 1887 is an entirely interpretive work, relying on the excavations conducted in the Fusco necropolis. However, in the excavation reports (1881, 1885, 1886) he never mentions his ideas on the production of Orientalising pottery, albeit a minimal ritual interpretation is provided for the *alabastra* found in the dirt.

of the tomb there was a large number of vessels deposited in an orderly fashion: a large amphora placed upside down with the mouth covering the foot of a cup; the cup was upside down as well, covered in white incrustation that prevented any original decorations (e.g. figures) from being visible, and it sat on top of the cover of a large 'copper' krater/urn; the latter was full of combusted bones, and the remains of the funerary linen were still visible attached to the inner side of the urn; on the sides of the urn there were two very elegant painted cups, broken.[121] After cleaning, the cup that was under the amphora revealed a beautiful decoration (Fig. 16-17): in the *tondo* was a bird with the head of a woman, which he calls a Harpy,[122] with long curly hair and wide wings; on the outer yellowish surface was a black-figured scene representing a bearded man and winged creature in the 'bent knee' pose.[123]

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[121] Cavallari provides diameter and height for each one of the described vessels (Cavallari 1885, pp. 51-52).

[122] It is most likely a Siren. See Pepe, Rescigno, Senatore 2016 for a careful reconsideration of the figure of the Siren in Italic and Italiot productions.

[123] He did not recognise the pose as representing 'kneeling running': the concept of *Knielaufschema* was introduced by German scholars, first for coins and only later for images on other kinds of artefacts (for early uses of the concept see Kalkmann 1895). The history of iconographical interpretations in classical scholarship is something yet to be written but would surely be of great interest.

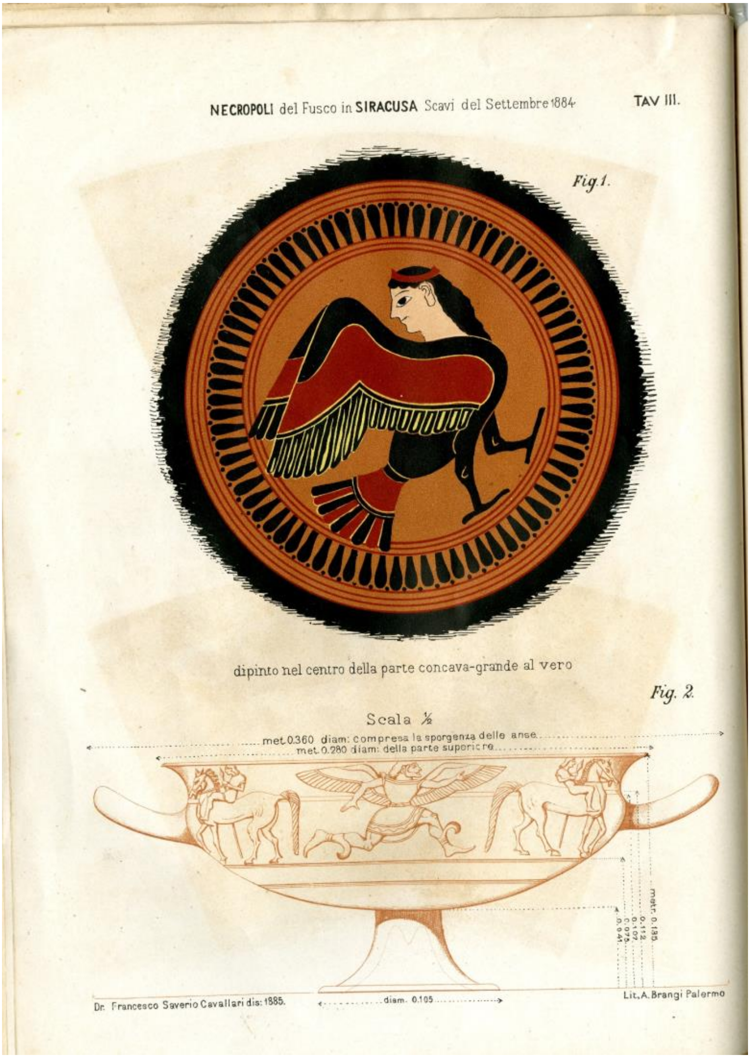

Figure 16 – Plate III from Cavallari 1887.

Figure 17 – Plate IV from Cavallari 1887.

The two broken cups to the side of the urn showed the same stylistic features: the first one displayed a bearded head in the *tondo,* and a tiger and a lion catching a pray on the outer surface; the second one had an eye in the *tondo* and Satyrs chasing a Maenad on the outside. To the east of this sepulchre, several other tombs were found to be arranged in a straight line. They were still closed and fragments of small cups and alabastra were found on top of the cover. On the inside, however, they were completely empty: not even skeletal remains were found. At the southern part of the necropolis several very small *loculi* were found, possibly for kids and infants: excavators recovered several fictile statuettes of animals, similar to those found in Megara Hyblaea; they also found cups decorated with stripes and animals. In a large sepulchre, to the side of these small ones, there was a beautifully preserved *bombylion* (h 14 cm, ø 7 cm) of oriental type, made of thin light-yellow clay with black figures and engraved details: it depicts two lions in heraldic position and a bird between them. The field is full of rosettes.[124] Inside another tomb, on the southern edge of the necropolis, there was a terracotta statuette of a seated

[124] Cavallari 1885, 53. The importance of the *bombylion* or *bombylios* (a shape in between the *aryballos* and the *alabastron*) in the construction of the proto-Corinthian and early Corinthian sequence will be highlighted in the next paragraphs (*vide infra* 191-203*).* This particular *bombylios* was used to illustrate the term in the *Enciclopedia Italiana Treccani* (Laurinisch 1930).

goddess with her hands on her knees and a high headgear: Cavallari calls it "un tipo molto arcaico"[125] without any further explanation. In the place called Pollicino, inside another grave, two *lekythoi* were found, with black-figured decoration on a yellowish field: the first one, intact, represents two fighting warriors – armed with spears, helmets and shields – and two cloaked men watching the fight. The second *lekythos,* whose mouth was found broken, presented the image of a naked youth and two cloaked men looking at him: on the right of the young man was a club. In another tomb, in the southern area of the necropolis, there was a large amphora (h 18cm, ø17 cm) decorated in rows with processions of animals, in the typical Corinthian Orientalizing style. In two other contiguous tombs, not far from the previous one, two beautiful cinerary urns painted in horizontal rows and with meanders had been deposited at the foot of the skeletons. During the excavation, in the earth mixed by the plough, the excavators found three fragmentary and one very well preserved *alabastra:* Cavallari advances the hypothesis that they were deposed on top of the tombs after they were sealed, constituting a final act of the funerary rite. In conclusion, judging by the ceramics found in this archaeological campaign, he continues to believe that this necropolis is one used by the first colonists who inhabited Ortigia. However, the new findings allowed

[125] Cavallari 1885, 53.

to attest that it had been used until the Roman age by the inhabitants of the island and probably by some of the inhabitants of Acradina and Neapolis.[126] Unlike in Selinus, the different necropolis areas did not correspond to chronological hiatus, but they served different areas of the 'Pentapolis'. The extensive area covered by the Fusco necropolis (ca. 1,5 km), and the presence of pre-historic tombs[127] carved into the wall that marks its limits made it remarkable. With this article – and these descriptions – the Fusco necropolis was entering the archaeological discourse as a crucial site where the passage between pre-Greek and Greek settlement was archaeologically visible.

The report of another archaeological investigation in the area appears in *Notizie degli Scavi* 1886. Cavallari writes a short paragraph to announce the discovery of a large paved *plateia* on a high platform in the Fusco necropolis, found on both sides of the wall of the modern cemetery. The author suggests that such a monumental platform inside the Fusco necropolis would lead one to think that it could be the base of the temple dedicated to Demeter and Persephone, which was built by Gelon in 480 BC, according to Diodorus Siculus (XI, 27).[128] In the short term, the chronology of the '*plateia*' would become a much more intensely discussed topic than

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126 *Ibidem*, 54.
127 Tombs which Cavallari already described in the *Topografia archeologica di Siracusa* (Cavallari 1883, 341-345).
128 Cavallari 1886, 139-140.

the chronology of the tombs,[129] despite having less of an impact in the long run.

As it is well known, the depiction of human figures on vases has long been considered a turning point in the development of classical art, denoting the distinctiveness and the supremacy of the Greek spirit. For many scholars, the Orientalising style, which had no human figures[130], came from the east – even though it was commonly called Corinthian, because of the large amount of specimens found in Corinth – and was a weaker predecessor of the 'true national Greek art'.[131] Francesco Saverio Cavallari shared

[129] The Appendix to the *Topografia Archeologica di Siracusa* (Cavallari 1891a) mainly focuses on the Fusco necropolis but, instead of reporting the wealth of tombs and grave goods found between 1883 and 1991, it discusses this wall and its connection to historical events narrated by textual sources.

[130] The vast bibliography on the introduction of human figures in Corinthian pottery is listed in Shanks 1995 and 199, 73-171: the author gives a 'contribution to a contextual and interpretive archaeology' (in his own words) connecting iconography to power dynamics and psychological narratives of the social and political context of the early *polis.* His approach has been widely debated and perhaps relies too much on (post)modern concepts for the interpretation of classical artefacts. However, it has the merit of raising an in-depth discussion on the societal and intellectual conditions under which Corinthian pottery was produced and commercialized.

[131] Ceramic studies are one of the most widely studied branches of the history of archaeology. Among others, one can mention Cook 1972, Van der Leeuw and Pritchard 1984, Rasmussen and Spivey 1991, and Orton, Hughes 2013. While dated, and maybe because of that, Ducati (1922, 6-

this opinion, though he included the 'Italic' people among the races that brought this revolution about:

> "La mancanza di figure umane nei vasi di stile orientale […] è un fatto notissimo; e se nei detti vasi esistono mostri con la testa di donna, come le sfingi, o figure virili con teste animali […] queste rappresentazioni sono riferibili a miti religiosi di un tipo orientale antichissimo ed invariabile, e non già ad episodi delle umane passioni, o a scene di civili costumanze, effigiate con quelle varietà dipendenti dalla vita e dai progressi di una razza libera, giovane ed intelligente come quella ellenica ed italica."[132]

It should be noted that for him vases of Orientalising style are connected to an Oriental type which is very old and 'unchanging'. Conversely, variety and progress were associated with the 'free, young and intelligent race' of Greek and Italic people. For this reason, after his excavations in the Fusco necropolis and Megara Hyblaea for the Museum[133] (where Orientalising vessels were by far the most common

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13), Luce (1918) and Walters (1905) are particularly useful to understand the attitude of their contemporaries.

[132] Cavallari 1887, 1-2.

[133] After a State-funded excavation campaign in 1872, Cavallari published a report in 1873 in the *Bollettino della Commissione di Antichità e Belle Arti di Sicilia.* The Ministry of Education financed a second short archaeological campaign in 1879, conducted by Cavallari.

kind of findings), he decided to write a short monograph on the much rarer vases with human figures as found in Megara Hyblaea and Syracuse. It was published in 1887 by the printer of *Giornale di Sicilia.* It was meant to show the victory of the Japhetic race over the Semitic one and the crucial role of Sicily in this transition.[134] According to Cavallari, because of the temperate climate, the East, Greece, the Mediterranean Islands – and Sicily in particular – as well as the Italian continent were predestined by Providence to give birth to the highest point of civilization.[135]

> "E qui nelle zone temperate in cui viviamo, con i più bei modelli creati da Dio, sempre a noi presenti, si sviluppò l'arte pura greca che vediamo apparire in Italia, e nell'VIII secolo a.C. in Sicilia […]. Ma quest'arte pura greca, importata in Italia ed in Sicilia dalle greche colonie, non subì alcuna trasformazione con la comunanza delle razze qui

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[134] Cavallari 1887, 4. From this passage, he appears to adhere to monogenetist ideas, but there are no other clues on the topic in his work. It should be noted that Freeman (1891) in his history of Sicily glorifies two moments of the island's history: the Greek past and the Norman rule, as they both halted the advancement of Semitic people (respectively, the Carthaginians and the Arabs), securing the 'Aryan race' would govern the island.

[135] Cavallari 1887, 8. The idea that ideal climatic conditions had something to do with the climax of Greek art was already in Winckelmann 1769. This idea had already been expressed by Abbé Dubos, Fontenelle and Montesquieu.

> da tempi anteriori stanziate […], razze che pur dovevano avere un'arte propria? Questo è il nostro tema. E siccome abbiamo in Siracusa e dentro i limiti della sua provincia opere d'arte che fanno supporre con valide ragioni che appartengano all'epoca del primo stanziamento delle greche colonie in Sicilia, siamo nel grado di poterle con cura esaminare, confrontandole con opere preesistenti, a notarne ogni mescolanza di stile".[136]

The *Leitmotif* of this work is that the introduction of human figures in Orientalising pottery happened in Sicily, the meeting point between the Greek colonists and the Italic people, who already had their art and civilisation.[137] Indeed, the Fusco necropolis presents tombs of indigenous people carved in the rock marking the limit of the tuff terrace. Megara Hyblaea is surrounded by prehistoric sites.[138] The

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[136] Cavallari 1887, 9.

[137] The belief that Sicily was inhabited by an ancient and advanced civilization, which in Sicilian scholarship had been popularised by Vincenzo Natale (1843), betrays the influence of the old *topos* of the 'ancient wisdom of Italic people'. The development of this concept from Giambattista Vico's *De antiquissima Italorum sapientia ex linguae Latinae originibus eruenda,* through the works of Micali, and its role in the formation of national identity have been discussed at length by Tagliamonte 2014 and Harari 2014.

[138] It should be noted that Sicilian prehistory had received in those years its first most relevant systematisation by Adrian (1878). Cavallari himself

vases described by Cavallari in the fourth and final section of his publication were found in these two necropolises (Figg. 16-20).

Figure 18 – Plate I from Cavallari 1887.

(1880) wrote a monograph on the remains of pre-Hellenic constructions in Sicily.

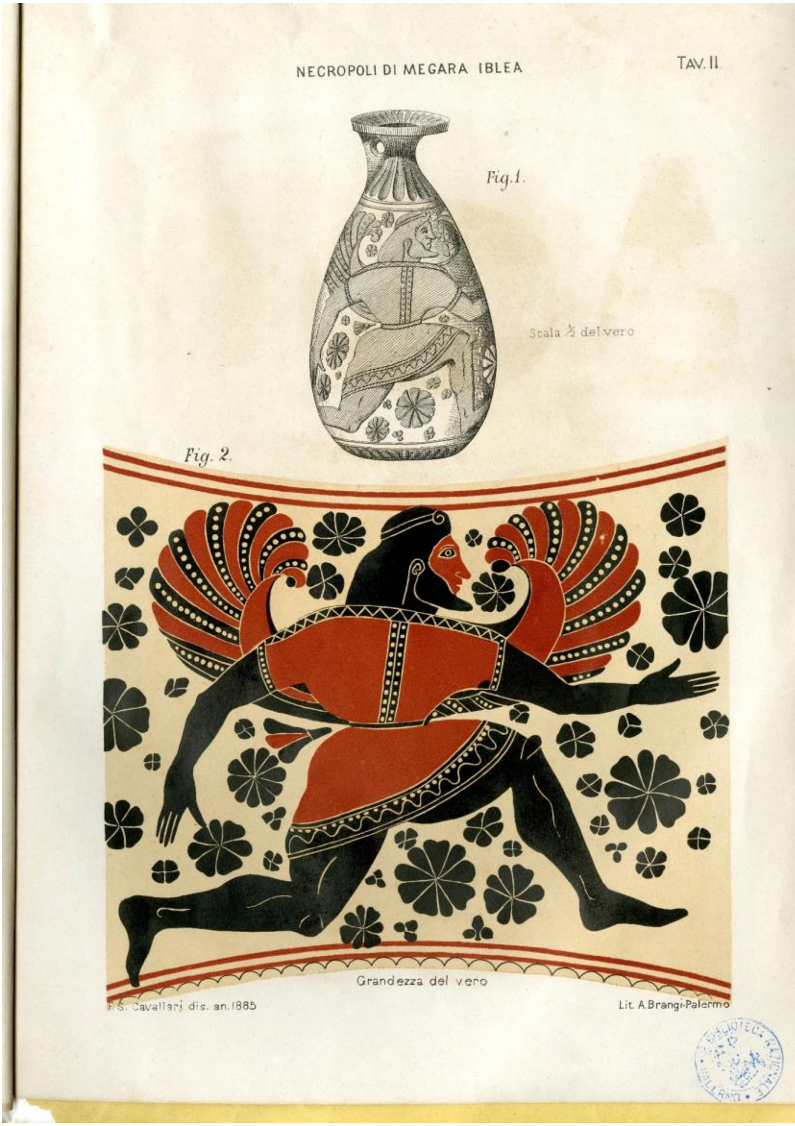

Figure 19 – Plate II from Cavallari 1887.

Figure 20 – Plate V from Cavallari 1887.

Those vessels, according to the author, represent the moment of transition between Orientalising art and Greek national art.[139] In particular, the Fusco necropolis presents a unique continuity, while in other places Orientalising burial places appeared to be separated from the ones called 'greco-sicule' (literally Greek-Sicel). It is for this continuity of occupation, from the pre-Hellenic to the Roman period, that Fusco is considered a site crucial to the construction of a chronological sequence of vase painting:

> "Se fossimo in qualche guisa agevolati, anche con lievi mezzi, da questa necropoli siracusana potremmo estrarre tanti altri vasi dall'epoca della prima colonia greca in poi […] da poter trarne una collezione cronologica tale, da segnare il progresso successivo della pittura vascolare dall'VIII secolo a.C. in poi".[140]

The complaint against the central government – lack of funds and investments in archaeological investigations – features strongly also in the last lines of the book. Particularly, Cavallari complains that the government chooses to invest in excavations in other regions over Sicily, where he locates the birthplace of 'great Greek art'.[141] One should read such

[139] Cavallari 1887, 16-21 (the entirety of the third section is devoted to this argument).
[140] Cavallari 1887, 14-15.
[141] *Ibidem* 42.

complaints, as well as Cavallari's theory, against the background of the regionalistic spirit mentioned at the beginning of this chapter: intellectuals, and historians in particular, had been strongly involved in Sicilian Risorgimento.[142] Francesco Saverio Cavallari took part in the Sicilian revolution of 1848 and his leaving Sicily of in 1854 was politically motivated. Even in his adherence to the unitarian agenda, he would have wanted Sicily to be recognised as having a special status.[143] These political ideals rested on historical regionalism, i.e. the concept of historical exceptionalism and the superiority of the Sicilian people. This preeminence was traditionally justified with the mixing of peoples who invaded Sicily, integrated and enriched its culture – a rhetoric that, from Florio to Freedman, was strongly asserted in many historical works on the island (Fig. 13).[144] However, the same regionalist agenda was sometimes expressed through the glorification of the Greek past or the celebration of the Sicels who – taking inspiration from Micali's *L'Italia avanti il dominio dei Romani* (1810) – were thought to be the first Italic people.[145] Cavallari brings together these three trends of Sicilian scholarship: for him the Sicels are not barbarians, they have their own civilisation and art and they are proto-Italic; when they meet with the Greeks

[142] *Vide supra* pp. 98-107.
[143] Cianciolo Cosentino 2012a.
[144] Momigliano 1984a.
[145] Micali 1810, cap. V.

in this land of encounters, Greek national art – the Japethian art *par excellence* – is born from from a combining of their different artistic features. He is, however, painfully aware of his own prejudice:

> "Parrebbe espressione di un vivace amor di patria il giudizio che ci facciamo a dare, se le prove di fatto oggimai nol confermassero: ma egli è certo che la trasformazione dell'arte orientale in ellenica si vede iniziarsi e sviluppare in Sicilia, e ne vediamo gli esempi incontrastabili [...] nelle recenti scoverte, e specialmente in quelle del Fusco".[146]

This passage shows an attempt at reconciling the idea of Sicilian preeminence (the word 'patria' refers to Sicily in the above citation) with the idea of an Italian Nation that was in the making.[147] In fact, after the Unification of Italy, Cavallari, like his friend Amari, abandoned the idea of an independent Sicily, and instead embraced the idea of an Italian nation.[148]

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[146] Cavallari 1887, 21.
[147] De Francesco 2013. Some fringes of Sicilian intellectuals were still linked to an independentist ideal and kept publishing historical and antiquarian works to further their agenda: the recent edition of Lionardo Vigo's *Protostasi sicula* by Giacomo Girardi (2017) provides a vivid picture of this intellectual milieu.
[148] Cianciolo Cosentino 2012a.

At the same time, the regional differences in fund allocation prompted his disappointment.

In the 1891 Appendix to the *Topografia Archeologica di Siracusa,* Cavallari summed up his previous work on the Fusco necropolis in one large plate (Fig. 21) where the succession of excavations and of pottery stiles mixes with topography in a remarkable chronological reconstruction.[149]

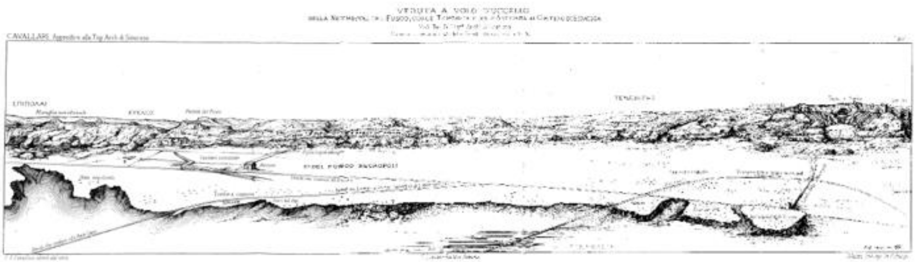

Figure 21 – *Veduta a volo d'uccello della necropoli del Fusco, colle Temenite e delle Antichità del cimitero di Siracusa,* from Cavallari 1891a.

2.1.3 Paolo Orsi

In May 1888, Paolo Orsi was inspector of the third level of the Royal excavations, museums and galleries by the Direzione Centrale delle Antichità, and was assigned to

[149] Cavallari 1891a. In the same year, Cavallari (1891b) also published a short essay on one red-figured vase from the Fusco necropolis. At the time, Paolo Orsi already took over most of his duties as Royal inspector of excavations (*vide infra* 162-179).

Syracuse under the supervision of Francesco Saverio Cavallari, then director of the National Museum.[150] Orsi was born in Rovereto in 1859, under the Austro-Hungarian Empire. He asked to become an Italian citizen in 1884, after having graduated in Padova in 1882. During his university years, he was strongly influenced by Luigi Pigorini, with whom he remained in touch throughout his career.[151] Before getting to Sicily, he had conducted stratigraphic excavations in Trentino and he had divided the local Neolithic in three sub-periods.[152] His nationalist ideals, and his undeniable intellectual weight, gained him a seat in the Senate in 1924, thanks to the recommendation of Ettore Tolomei, during the Fascist government (Fig. 22).[153]

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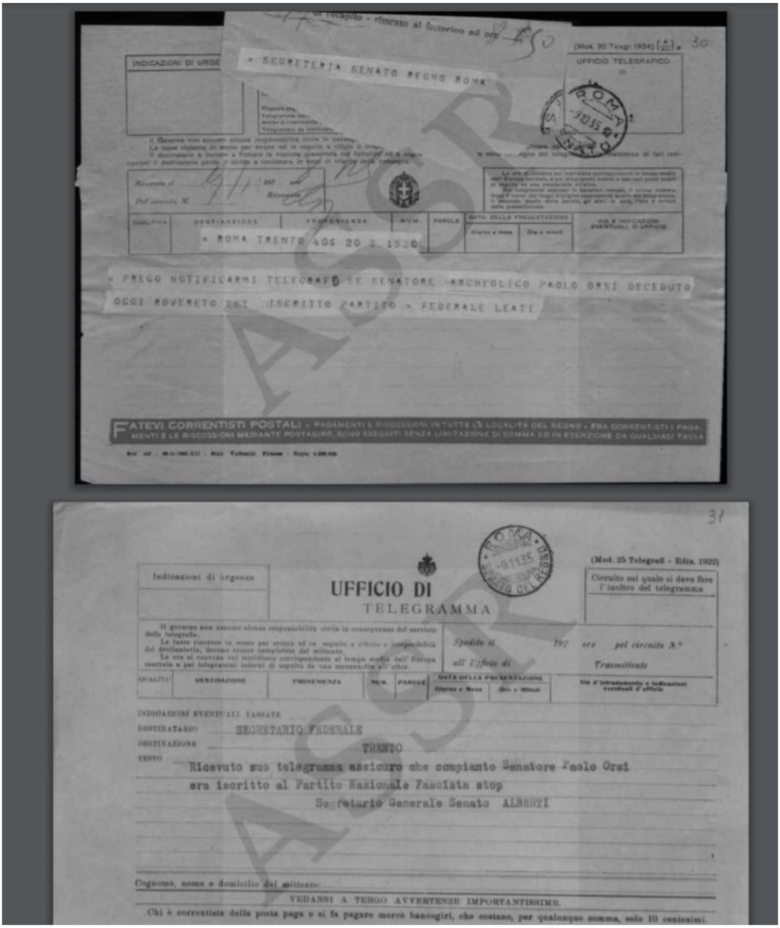
150 Paolo Orsi is one of the best studied personalities of Italian XIX-XX centuries archaeology: the 1991 proceedings of the Conference "Paolo Orsi e l'archeologia del '900" in Rovereto are to these days the starting point for researchers interested in his life, work and beliefs. For a short biography of Paolo Orsi see Calloud 2013. For his work in Sicily and Calabria see Arias 1976, pp. 15-29. Part of his 150 notebooks has been published by Lamagna and Monterosso 2018. Lambrugo 2013 provides a useful account of the every-day practice of his Sicilian excavations.

151 The Orsi – Pigorini letters are kept at the Fondo Pigorini at the University of Padua and their early contacts are fully accounted for in Cupitò, Facchin and Leonardi 2010 (with bibliography and published letters).

152 For further information on his excavations and work on the prehistory of Trentino see Ciurletti 1991.

153 Though he was part of the Fascist National Party (he had to be in order to be part of the Senate), Paolo Orsi strongly defended the 'anti-fascist



SEGRETERIA SENATO REGNO ROMA

ROMA TRENTO 406 20 8 1930

PREGO NOTIFICARMI TELEGRAFO SE SENATORE ARCHEOLOGO PAOLO ORSI DECEDUTO OGGI ROVERETO ERA ISCRITTO PARTITO = FEDERALE LEATI

FATEVI CORRENTISTI POSTALI

UFFICIO DI TELEGRAMMA

DESTINATARIO SEGRETARIO FEDERALE

DESTINAZIONE TRENTO

TESTO Ricevuto suo telegramma assicuro che compianto Senatore Paolo Orsi era iscritto al Partito Nazionale Fascista stop

Segretario Generale Senato ALBERTI

VEDANSI A TERGO AVVERTENZE IMPORTANTISSIME

Chi è correntista della posta paga e si fa pagare mercè bancogiri, che costano, per qualunque somma, solo 10 centesimi.

Figure 22 – Extract from the personal file of Paolo Orsi, from the archive of the Senato della Repubblica. The telegram confirms he was registered as a member of to the Partito Nazionale Fascista.

professor' Giuseppe Agnello when, in 1924, his work *Il carnevale politico nel Siracusano* was censored and he was removed from his role as a high school teacher, while his right to pension was revoked (Pergola 1988; Agnello 1962).

It is mostly through his work – as well as Bernabò Brea's – that Sicilian archaeology (and especially its prehistory) became a major topic of national and international interest.[154]

In his long and prolific career, Orsi wrote several publications on the Fusco necropolis, each reporting the results of an incidental or planned excavation.[155] In 1891, the same year he wrote the first article on the Fusco in the *Notizie degli scavi di antichità,* Paolo Orsi was also working on a monograph on the necropolis of Megara Hyblaea with Francesco Saverio Cavallari[156]. On many respects he was strongly influenced by his older colleague: when describing the findings made at Syracuse, he did not forget to place them with great *akrybia* in the plates of the *Atlante topografico.* He also took the work of Mauceri into consideration. Orsi often referenced Mauceri's previously published vases when discussing the one he himself had found.[157] However, even in this intial short publication, he already offers a distinctive point of view, showing a disposition to arguing for the pre-eminence of Attic pottery and Greek classical art. In fact, in describing the tombs excavated in the Fusco necropolis in October 1890 and August 1891 and their grave

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[154] Leighton 1986.

[155] A list of his over 300 publications was issued by G. Agnello (1935) soon after his death. More recently, A.M. Marchese and G. Marchese (2000) published a new bibliographic catalogue.

[156] Orsi and Cavallari 1892.

[157] Orsi 1891, 405 and 407.

goods, he uses Furtwängler's works as the main resource to class pottery, following his distinction of various 'shapes' and adopting his chronology.[158] Similarly, for the calyx krater of tomb XIV and the *pelike* of tomb XIII, Orsi compares the figures on the vases to the style and composition of, Praxiteles and Phidias, respectively, in order to establish their chronology.[159] Most importantly, to maintain that certain vases were local imitations he emphasises their "fattura scadente" and their "stile rigido e trascurato"[160]. Local production is, therefore, not an added value, but a degradation of the 'original' style: this stands in stark contrast to Cavallari's idea of the Sicilian birth of Greek national art.[161]

In 1894, Orsi wrote an extensive report on the excavation campaign conducted between 5 December 1892 and 12 January 1893 in the Fusco necropolis. 176 tombs were found, some of which devastated by agricultural activity or already previously excavated by Cavallari. The number of

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[158] In particular, he refers to Furtwängler 1885.
[159] Orsi 1891, 407-411.
[160] *Ibidem,* 405.
[161] *Vide supra,* 152-161. The problem of 'degradation' of style as a chronological indicator (i.e. the so-called 'provincial lag'), which hides an approach to classical art that is both biological and monocentric (but can also exist in a polycentric view) has been discussed in Settis 1989, highlighting how this prejudice persists in contemporary scholars, such as Antonio Giuliano or Brunilde Sismondo Ridgway (Settis 1989, 144-145).

untouched Greek tombs was 121, and their construction predated Dionysius I and his wall. After this archaeological campaign, he was assured that the southern bank of the terrace had been exhaustively investigated. He notes that the most interesting artefacts from the Fusco necropolis are undoubtedly the vases, as this site could be very informative on the proto-Corinthian style.[162] Deviating from Cavallari's theories, he looks at German scholarship on Greek pottery. He asserts that the beginning of the proto-Corinthian style is synchronous with the beginning of the Dypilon necropolis. The proto-Corinthian style fully develops in the VII century B.C. and around 600 BC it is substituted by the Corinthian style, while some late manifestations manage to linger until the fifth century BC.[163] After highlighting the centrality of vases among the findings of the Fusco necropolis, Orsi raises the issue of local imitations, which is especially challenging because the quality of the manufacture is not always a good indicator: he saw in Munich some rather mediocre vases from Corinth which, if found in Sicily, would have been labelled 'italo-corinzi'. Nonetheless, he believes that the vases found in the Fusco necropolis are mostly imitations, with some sporadic exceptions.[164] Most importantly, he finds

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[162] Orsi 1894, p.8.

[163] *Ibidem* footnote 3: he cites the works of Dümmler 1887, Wilisch 1892 and Masner 1892. The latter complains that the oldest tombs of the Fusco necropolis are not yet known (p. XI).

[164] Orsi 1894, pp. 8-9.

that the tombs excavated in this campaign are the oldest of Syracuse and the vases mostly date to the VIII-VI centuries BC. He argues that the sequence of vases reconstructed from the findings of the Fusco necropolis confirms the chronology proposed by Dümmler and Wilsch, and in addition it provides data on two crucial transitions: from Geometric to proto-Corinthian style; and from proto-Corinthian to Corinthian vases. The first is represented by Tomb 108 and the second by Tomb 29. He dates the first transition to the late VIII century BC and the second one to the end of the VII century BC, corroborated by findings from the excavations of Naukratis.[165]

> "Se i risultati materiali non sono stati pari alle speranze che si nutrivano per la più grande città dell'occidente ellenico, utilissimi furono invece quelli scientifici soprattutto per la cronologia vascolare"[166]

Tomb 29, although crucial to his chronological construction, "deve essere stata frugata anticamente"[167], therefore the vases attributed to this context come from a large accumulation of ceramic fragments found on top and in the immediate vicinity of the tomb. Some of the fragments identified were: several dozens of geometrically zoned

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[165] He mentions Smith's (1890) and Petrie's (1886) publications.
[166] Orsi 1894, 10.
[167] *Ibidem,* 15.

cups,[168] some *pyxides* with hempestic-geometric decorations, two *lekythoi* – one probably 'authentic' (not a local imitation) – and fragments of Rhodian vases with lotus flowers. *Bombylioi otriformi* or *aryballoi* of the Corynthian style were absent. Based on these findings, Orsi dates the tomb to the mid-VII century BC (Fig. 23).[169]

Figure 23 – Illustration of a selection of the artefacts from Tomb 29 (Paolo Orsi 1894: 15-16).

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[168] If the disturbed context was not convincing enough, the number of cups is another clue that goes against the likelihood of this agglomerate coming from one single burial.

[169] *Ibidem,* 15-16.

From the point of view of chronology building, tomb 65 is also interesting. It contained an amphora which Orsi identifies as having the style of Rhodes or Melos. Following Rhoden, Collignon and Rayet, Orsi states that the artistic development of this Rhodian fabrics peaked in the VII century BC, while examples dating to the beginning of the VI century are less numerous, and of lower quality. The low-quality manufacture of the amphora found in the Fusco tomb (Fig. 24) poses a critical question: is it a local imitation of the VII century or is it an imported insular production of the VI century?[170]

Figure 24 – Illustration of amphora from Tomb 65 (Paolo Orsi 1894: 21).

[170] *Ibidem*, 21-22.

As we shall see, this question is – implicitly or explicitly – at the centre of many subsequent discussions on the chronology of Greek vases.

Another grave, Tomb 108, appears crucial to the understanding of the early phases of proto-Corinthian pottery: the grave goods included two small *lekythoi* and a *kylix* of the proto-Corinthian style, a big fragmentary *olla* and one small cup. Moreover, a fragmented, primitive 'column vase' was found. Because Orsi cannot illustrate it, he reproduces a very similar vase, which was found in an unknown tomb at the Fusco necropolis and stored at the Archaeological Museum. He compares its shape to the Aristonothos krater, to geometric-style vases published by Conze[171], as well as to proto-Corinthian vases published by Wilsch.[172] Moreover, he publishes the drawings of a large proto-Corinthian flask, of a cylindrical *pyxis* and a spindle. The tomb also contained two thin rings (of bronze and silver) and one oblong amber bead, placed under the chin of the deceased. Orsi dates this tomb to the end of the VIII – beginning of the VII century BC.[173]

One last sepulchre from this publication needs attention: Tomb 129 contained six small '*fibulae a navicella'* with a long pin, one large '*fibula a navicella*' placed on the left shoulder

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171 Conze 1870, Pl. X.3.
172 Wilisch 1892, Pl. II.27.
173 Orsi 1894, 34-36.

of the deceased, four silver rings on his chest and one spindle, similar to the one found in Tomb 108. Describing this tomb, Orsi says that the history of the Greek *fibula* has still to be written and he excludes the possibility that Italic *fibulae* can be used to date the Greek ones. However, some rare similar brooches were found in Megara Hyblaea's VII century tombs, in Cuma and Suessola (720 – 520 BC). However, the presence of the spindle in Tomb 129 allows Orsi to propose a date closer to the VIII-VII centuries BC, despite the absence of ceramics. He takes comfort in the fact that this chronological determination agrees with the chronology of the bronze and gold *fibulae* of analogous type found at the Dipylon: for Orsi they belong to the same class, except for the squared appendix, which is lost in the Fusco specimens. The loss of the squared appendix is a typological evolution, which agrees with the fact that the Fusco necropolis and the necropolis of Megara Hyblaea are younger than the Dipylon.[174] The chronological arguments presented in this publication are very interesting for three main reasons: 1) he uses cross-dating inside the necropolis itself: the brooches are dated by the spindle, which is dated by the pottery; 2) he uses a typological argument that implies a genetic and linear evolution of types; 3) he mentions artefacts from Greece (and particularly the Dipylon) as direct predecessors to the Fusco *fibulae*, but he denies the

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[174] *Ibidem*, 40: footnote 2.

possibility of influences from neighbouring Italic people: the typological sequence built by Orsi is a manifestation of Greek ethnicity.[175]

It should be noted that, in his next publication on the Fusco necropolis, Paolo Orsi changes his mind on the dating of the tombs excavated in the 1892-1893 campaign. There, he states that no tomb of the VIII century was found during said campaign, and that only the following excavation, conducted in 1893, revealed the oldest burials of ancient Syracuse.[176]

Indeed, from June to December 1893, Paolo Orsi intensively excavated 5000 square meters in the area that he believed to contain the oldest Greek tombs of Syracuse.[177] He publishes a report of the excavation in the *Notizie degli Scavi* of the following year. Paying his usual attention to funerary rituals, Paolo Orsi highlights a correlation between the

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[175] His ethnic approach rests on a mild form of scientific racism, which becomes apparent when he discusses the 'barbaric' tombs found in the Fusco necropolis in 1893 (Orsi 1895, 11-12): he sent the skeletons to Giuseppe Sergi – who was a friend of both Lombroso and Galton – in order to establish their ethnic identity, as "il tipo antropologico [è] completamente diverso da quello della razza paesana, perché a Siracusa la massa della popolazione […] era rimasta greca; le dimensioni colossali, le poco armoniche linee delle teste enormi dicono tosto anche ad un profano non esser greca la razza cui spettano" (p.12).

[176] Orsi 1895, p. 11.

[177] This area had already been explored by Cavallari in an unsystematic way (Orsi 1895, 3).

choice of ritual and the antiquity of the necropolis: the older the necropolis the more common the use of inhumation.[178] However, the main value of this excavation rested on the impact it had on the history of vase-painting:

> "Ricca di inattesi risultati è stata la campagna del 1893 per la storia della pittura vascolare e per la relativa cronologia"[179]

The Fusco necropolis is site well suited to the study of the 'svolgimento dello stile corinzio primitivo' (i.e. the progression of the primitive Corinthian style) in its four phases:

A) Pure geometric proto-Corinthian group (last quarter of the VIII century BC)

Typical artefacts: nearly globular *lekythoi* (i.e. *aryballoi*) made of pure clay of a fair-yellow colour, with brown geometric friezes; rarely there are animal depictions.

> "Fino a prova in contrario reputo codesti i più antichi vasi greci della Sicilia, anello col Dipylon

[178] He recalls that in the necropolis of Megara Hyblaea (VI century BC) 25% of burials used incineration; in the Fusco necropolis (1892-1893 excavation: VII century BC) only 7% of burials used incineration (Orsi 1895, 5).
[179] *Ibidem*, p. 7.

> ed il geometrico, e però li assegno all'ultimo quarto del secolo VIII".[180]

Also, some *kylikes, skyphoi,* conical *lekythoi* with a high neck, and cylindrical *pyxides* produced in the same style were found, though they were associated with *lekythoi* of Group B, and were therefore believed to be slightly younger than Group A.

B) Geometric/zoomorphic proto-Corinthian group (first half of the VII century BC or slightly later)

Typical artefacts: *lekythoi* (i.e. *aryballoi*) approximating a heart shape with animals in the frieze, still subject to the geometric order of the decorative patterns. In this period some beautiful jugs appear, as well as dark *olpai* and dark conic *lekythoi.*

C) Developed zoomorphic proto-Corinthian group (mid VII century – early VI century BC)

Typical artefacts: tapered heart-shaped *lekythoi* (i.e. *aryballoi*). Zoomorphic decoration is prevalent, and first attempts at mythological representations in the form of demonic beings appear. The oriental influx starts to become apparent, while

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[180] *Ibidem.*

the geometric decoration may still sometimes be seen in *kylikes* and *skyphoi* “in forma corrotta e decadente”.[181]

D) Corinthian Orientalising group (ca.VI century BC)

Typical artefacts: *bombylioi* and *aryballoi* with animals and demonic beings.

The last group was rare in the 1893 excavation, which clearly investigated an area of tombs older than the VI century BC. This chronological classification system with four phases soon becomes widely accepted.[182]

Orsi’s attempt at a chrono-typological classification of *fibulae* was not met with the same success. The first group is the ‘*fibulae a gomito o trapezio*’ made of iron, ivory and amber. They are found in the oldest tombs and show oriental influences. The second group is the one of ‘*fibulae ad arco*’ with the body covered in bone decorations and Orsi considers them to be a transition group towards the third type. The ‘*fibulae* a piccola navicella’ are more recent and were found in great quantity in Megara Hyblaea. All these *fibulae*, according to the author, were of Greek manufacture

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181 *Ibidem,* p. 8. This poses again the same question (*vide supra* p. 170) of bad quality being either a sign of imitation or a sign of the degeneration of a style over time, according to a biological view of stylistic variation.
182 Neeft (1987, 18) at the beginning of his chronological assessment of proto-Corinthian Subgeometric *Aryballoi* provides a history of previous research and mentions Orsi 1895 paper as the founding moment for the chronology of proto-Corinthian pottery.

and – judging by their association with geometric vases in the Finocchito necropolis and with the oldest vases in the Fusco necropolis – were used between the end of the VIII and the beginning of the VII centuries BC.[183]

The chronological determinations proposed by Orsi in his publications on the Fusco necropolis are essential to the construction of two archaeological typological sequences – proto-Corinthian pottery and Greek *fibulae.* These works, especially the 1895 article, would continue to be referenced for a long time.[184] Moreover, they also impacted the imminent historical debate on the foundation dates of the Western colonies. Orsi supports Pais in the dispute with Beloch by stating that Syracuse was founded at the end of the VIII century not at the beginning of the VII. The VIII century date is also in agreement with the fact that the purely geometric Greek vases found in the prehistoric necropolis of Finocchito (dating from the mid VIII century BC) are not found in the Fusco necropolis. However, the presence of such vases in an indigenous necropolis gives archaeological weight to the hypothesis of commercial contacts between Greece and the western Mediterranean, before the establishment of colonies: "Così i risultati archeologici vengono a lumeggiare la situazione storica".[185]

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183 *Ibidem,* p. 9.
184 *Vide supra* footnote 178.
185 *Ibidem,* p. 11.

When establishing comparisons to assess the chronology of pottery types, Orsi shows a predilection for Greek sites and especially the Dipylon: his chronological conclusions build on the results of M. Holleaux's investigations in Boeotia, on the previous classification of proto-Corinthian vases made by Wilsch, and, most importantly, on the new findings of Brückner and Pernice at the Dipylon.[186] By contrast, for the *fibulae* there is no previous typology of Greek brooches to build on: instead, he chooses to draw comparisons to Nimrud and Thebes, and only mentions Italic specimens from Bologna and Corneto.[187]

With Paolo Orsi the times of Sicilian independentism in archaeology and history-writing were finished (or temporarily set in the background, to be more accurate).

More tombs were excavated by Paolo Orsi in the following years: in 1903 (Tomb 517 to 556), in 1905 (Tomb 557 to 559), in 1907 (Tomb 560 to 587) and in 1915 (Tomb 588 to 672).[188] However, his 1895 article and the plates from his 1894 publication became admittedly crucial to the chronology of

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[186] Holleaux 1894, Wilsch 1892, Brückner and Pernice 1893. *Contra* Kroker 1886.
[187] This is in accordance with his diffusionist ideas: see Salmeri 1992b, 110 and footnote 83 with bibliography. The Italic *fibulae* are mentioned in Orsi 1895, 9: footnote 1.
[188] Orsi 1903, 1905, 1907, 1915.

proto-Corinthian pottery, on which other chronological sequences were built for years to come.[189]

Paolo Orsi's publications were extremely thorrough, containing data about funerary rites, detailed descriptions of the grave goods and their position in the tomb, and information on the shape of sepulchres and their topographical position in the necropolis. This is possibly one of the reasons why the Fusco necropolis was not investigated further until recent years, when new archaeological campaigns were conducted by the Soprintendenza Archeologica di Siracusa and the University of Catania.[190] Unfortunately, the results of those recent excavations remain, as of writing, unpublished.

2.1.4 Giovanni Patroni

Giovanni Patroni was born in 1869 in Naples, shortly after the Unification. He strongly believed in the national identity of Italy and, during his long career, he frequently travelled throughout Italy (not only the peninsula, but also the

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[189] The crucial role of Orsi's excavations in Syracuse and in Megara Hyblaea for the chronology of proto-Corinthian pottery is recognised, for example, by Pace 1915, 442 footnote 2. See also Walters and Birch 1905, footnote 282.

[190] Basile 1993-1994 provides preliminary information; see also Zirone 2011.

islands), looking for the manifestations of 'the Italic spirit' in pre and proto-historical archaeology.[191]

After graduating in Classics at Federico II, he completed his specialisation at the Scuola Superiore di Archeologia in Rome, where he was a pupil of Emanuele Loewy, with whom he approached the study of Etruscology and palethnology. He spent the third year of the School in Greece and Asia Minor, where he got acquainted with the German philological tradition.

In 1895, after joining the administration of Fine Arts, he was sent to Syracuse to assist Paolo Orsi at the National Museum, aged 26. The following year he published a 'contribution to the history of Greek *fibulae*' in the Bullettino di Paletnologia Italiana.[192] The copy of the University Library in Pavia contains a handwritten dedication by the author, who sent it to a colleague from Naples on the 22nd of April 1896.[193] He only stayed in Syracuse for a little over one year and did not

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191 D'Adamo 2011 (with relevant bibliography). In the work of Patroni the interplay between classical archaeology and protohistoric research – with their different questions, intellectual traditions, academic methodologies – becomes evident, to the point that in the first pages of this essay he feels the need to clarify that his research can be useful both to classicists and prehistorians.

192 Vistoli 2014 provides a short biography with a comprehensive bibliography on Patroni's archaeological activities, intellectual life and political agendas.

193 On the career of Patroni at the University of Pavia cf. Barabanera 2009 and Harari 2017.

take part in any excavation at the Fusco necropolis. However, he used the published and unpublished material that was accessible to him at the Archaeological Museum to build a chronological system of the Greek *fibula*, which made this necropolis enter the discourse of Italian palethnology. Why would a history of Greek *fibula* be of interest to protohistorians? The answer is provided by the author in the very first paragraph of his work, which he tailored to palethnologists, from the venue of publication to the structure of the argument:

> "per più rispetti l'una ricerca (n.d.r. classica) completa l'altra (n.d.r. paletnologia sicula), in più di un punto s'illuminano a vicenda, quando vengan poste a riscontro. Uno di tali punti è lo sviluppo di un oggetto la cui storia desta tanto interesse nei paletnologi; per cui già nello strato archeologico delle terramare si può constatare l'influenza di una cultura che ebbe sede nel bacino orientale del Mediterraneo; intorno a cui ben poco era noto anche nel campo limitato degli studi ellenici, mentre la quantità straordinaria di esemplari raccolti nella necropoli greca del Fusco viene ora a chiarirci e ad aumentare notevolmente

> le nostre cognizioni. Il lettore già sa che vogliamo parlare della fibula".[194]

This quote contains several interesting points relevant to the analysis of Patroni's work. Firstly, for him, the development of Greek and indigenous *fibulae* cannot be separated. When (re)constructing the morphological and functional development of a type, the reasoning of Patroni is neither strictly evolutionistic, nor unilinear: he acknowledges foreign influence and that similar pre-existing conditions can lead to multiple, different and coexisting outcomes. As a result, if one had to represent his typo-chronological determinations, they would resemble a tree more than a line.[195] Secondly, the mention of oriental Mediterranean influences on the *terremare* is an early clue of his position on the prehistory of Italy: in his *Preistoria* he would break with the ideas of Pigorini, arguing that the Italic people did not come from the North, but that they instead were autochthonous and had a Mediterranean origin.[196]

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[194] Patroni 1896, 32.

[195] This is even more evident in his approach to the classification of red-figured pottery, cf. Patroni 1897.

[196] Patroni 1937, D'Adamo 2011 and Pearce 2015 for a comment. It was a manifestation of Mediterraneist and autoctonist racism, opposed to Pigorini's northern and migrationist theory: the opposition between several kinds of racism in Italian archaeology has been studied by De Francesco 2013, 181ff (see also the review of his book by Harari 2015). For the emergence of this catholic historicized racism in the African work

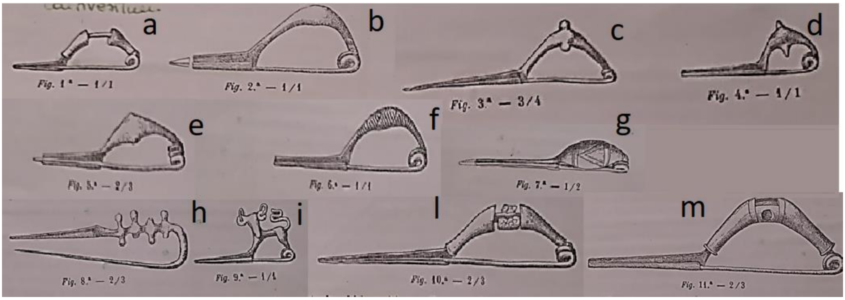

Figure 25 a-m: illustrations of *fibulae* from Patroni 1896.

Patroni distinguishes seven types of *fibulae* – six made of bronze and one of iron – from the Fusco necropolis (Fig. 25 a-m):

Bronze: 1) *arco semplice fibula* with the body covered in amber and bone Fig. 25a); 2) Ω-shaped *fibul*a; 3) *arco rigonfio fibula* (Fig. 25b); 4) small *navicella fibula* (Fig. 25c-g); 5) *serpeggiante fibula* (Fig. 25h); and 6) *cagnolino* and *cavalluccio fibula* (Fig. 25i).

Iron: 1) simple arch *fibula* with the body covered in amber and bone (Fig. 25l-m).[197]

From this classification it is evident that Patroni does not follow the schema proposed by Orsi in his 1895 publication. The latter had only identified five groups of *fibulae*: 1) *gomito*

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of Pallottino see Harari 2016. Especially for the early emergence of this Mediterraneist theory in the iconographical works of Patroni see Oddo *forthcoming*.

[197] Patroni 1896, 31-36.

fibula made of iron, ivory and amber; 2) bronze *arco semplice fibula* with the body covered in bone; 3) small *navicella fibula*; 4) Ω-shaped and *cavalluccio fibula*; 5) *serpeggiante fibula*.[198]

Patroni's reconstruction of the history of the fibula is very different to Orsi's, despite the illustrations in his publication being taken from Orsi's articles: Patroni even highlights mistakes in Orsi's drawings through autoptic observation (Fig. 25i).

Patroni provides an absolute chronology for his seven groups, based on associations with Corinthian pottery. For this, he relies on Orsi's chrono-typological system.[199] The first group is assigned to the end of the VIII – beginning of the VII centuries BC. [200] The second one – of which Orsi did not give a dating – is attributed to an early period, as the only specimen found in the Fusco necropolis was associated with a geometric proto-Corinthian *skyphos*.[201] The third group is dated to the mid VII century BC, based on a single specimen, from the artefact rich Tomb 428.[202] The *navicella* group includes more than 20 specimens, which have different

[198] Orsi 1895, p. 9.
[199] Orsi 1895, 6-8.
[200] Patroni 1896, p. 32.
[201] *Ibidem.* Tomb 205, where the *fibula* was found, is described in Orsi 1895, 27: it includes a proto-Corinthian geometric skyphos, a biconical spindle and, on the chest of the deceased, eight thin silver rings; on the shoulders two bronze pins with disk head.
[202] *Ibidem.* Tomb 428 is described in Orsi 1895, pp. 61-65.

dimensions and shapes of the arch in the body. Interestingly, while measurements are not interpreted as time-sensitive, the angle of the arch is considered chronologically diagnostic: the *arco a gomito* is considered a predecessor of the *arco semplice.* This sequence is inferred – as well as most archaeological determinations in this article – from the comparison between the Sicel necropolis of Finocchito and the Greek one of Fusco. In particular, he notes the presence of *gomito fibulae* and the absence of *arco semplice fibulae* in the Sicel necropolis of Finocchito, which according to Orsi's classification of Sicel periods ends around the mid-VIII century BC, and before the beginning of Greek colonisation. By contrast, *Navicella fibulae* with a simple arch – often decorated with linear patterns on the larger surface of the arch – were found in the oldest tombs of the Fusco necropolis, and their initial appearance is therefore dated to the late VIII century BC.[203] The fifth group was assigned to the VIII century BC and according to Patroni – after evolving from the *drago fibula* – it had a short fortune in Sicily and disappeared at the beginning of the VII century BC: indeed, at Finocchito several *drago fibula* and some *serpeggiante fibula* had been found, while the latter was only registered twice at Fusco, originating in very ancient tombs.[204] The sixth group, consisting of four bronze *fibulae* (two shaped like a small

[203] Patroni 1896, 32-34.
[204] Patroni 1896, 34-35.

horse and two like a lion),[205] is assigned to the first half of the VII century BC.[206] Patroni disagrees with Orsi's typological sequence in regards to this group, as well as the seventh group (i.e. iron *fibulae* with the body covered in amber and bone). According to the Roveretan archaeologist, the iron *fibulae* are the oldest and the ones with small animals on the arch are the most recent. However, Patroni points out that three of the four animal *fibulae* found in the Fusco necropolis and Megara Hyblaea have been discovered in tombs that also contained iron *fibulae*.[207] According to Patroni, the *arco semplice fibula* produces three different embellished types, which are therefore contemporaneous, and cannot be described as a sequence: the *navicella* type with linear decorations; the type with the body covered in amber and bone, with material decorations; and the zoomorphic type with plastic decorations. The first two types account for nearly 80% of the *fibulae* in the Fusco necropolis, and appear to have been manufactured for a longer period. They were in circulation from the beginning of Greek colonisation in the late VIII century to the final years of the VII century BC[208], after which - according to Patroni - *fibulae* stopped

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[205] Patroni 1896, 35 notices that the drawing published by Orsi (1895, 69, fig. 73) is misleading and the *fibula* represents a young lion – not a dog as Orsi stated.
[206] Patroni 1896, 37.
[207] *Ibidem*. He refers to Tomb 421 and 441 of Fusco and Tomb 501 at Megara.
[208] *Ibidem*, 37-38.

being used in Sicily.[209] In his chronological system, different classes and types of artefacts have different time-spans according to their use and diffusion. Moreover, Patroni considers both geography and local preferences as factors that may have affected the length of the time interval during which those types and classes were in circulation. This becomes evident in his belief that Sicily followed a different path in the adoption of *fibulae* than the rest of Italy.[210] In order to establish these differences in timespan and geography, he needs to adopt a chronological reference framework: he chooses ceramic associations, following the subdivision of proto-Corinthian style proposed by Orsi just one year before.[211]

Indeed, provenance is one of the main concerns of Patroni. On this topic, he strongly opposes Orsi's idea that the first *fibulae* were produced by Semitic peoples in Asia, who subsequently brought them to Greece and the Western Mediterranean. For Patroni, *fibulae* originated in Greece (more specifically Mycenae) and diffused from there eastward and westward. In his eyes, Orsi's theory was the product of cultural bias:

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[209] *Ibidem,* 44-46: he dismantles the iconographic arguments for the use of the *fibula* during the VI and V century BC.
[210] *Ibidem,* 35.
[211] He does not seem to consider that Orsi's ceramic typology does not account for different durations, nor for local preferences.

> "Mi pare che faccia qui capolino ciò che S. Reinach ha chiamato *miraggio orientale* (italics in the original), e che poi si risolve sempre a profitto esclusivo del semitismo"[212].

Here one can see his strong antisemitic viewpoint.[213] Furthermore, in the following paragraph he argues for the local production of iron *fibulae*. In Patroni's writings special attention is often devoted to local productions, which are considered manifestations of the 'Italic spirit'.[214]

In the final pages of the paper, Patroni aims to build a history of the *fibula,* based on their appearance in Sicel necropolises, and following Orsi's periodisation of Sicilian prehistory:

1) First Sicel period: no *fibulae.*
2) Second Sicel period: Mycenean *fibulae* (*arco di violino* and *drago*) are found in Pantalica and Cozzo del Pantano. Continental Italy – and in

[212] *Ibidem* 38. On Paolo Orsi's adherence to diffusionism see Salmeri 1992b, 110 and footnote 83 with bibliography.

[213] Patroni was to become an enthusiast supporter of the fascist regime in the 1920s. See D'Adamo 2011 on the implications of nationalist and fascist ideals on his archaeological and historical interpretations.

[214] Patroni 1897 uses the same approach to argue that Apulian vases were not produced in Taras but were a local product of indigenous people of Ruvo di Puglia. See also Barbanera 2009, D'Adamo 2011, and Pearce 2015.

particular the *terremare* – acquire these imported goods later than Sicily. His Mediterraneist theory begins to emerge.[215]

3) Third Sicel period: passage from the bronze age to the iron age. The *drago fibula* slowly transforms into the *serpeggiante* type. The *arco semplice* and its decorated variants (*navicella, zoomorphic,* bone & amber iron *fibulae,* but also the Ω-shaped type) start to appear.

Already in this paper, Patroni explicitly rejects the idea of 'light coming from the east' as well as the Indo-European migrationist theories of Brizio.[216] With this paper, at age 27, Patroni starts to establish his position as the main proponent of the Mediterraneist hypothesis. His ideas on the topic will become clearer the following year with the publication of his first – and soon widely disseminated – monograph on the red-figure vases of Southern Italy.[217]

2. 2 The Fusco Necropolis in chronology building

Since its discovery, the Fusco necropolis has been a nodal site in a long-standing chronological debate: a threefold

[215] Pearce 2014 on the Mediterraneist positions of Patroni, opposed to Pigorini and connected to his nationalistic and racist ideas, as well as the idealistic context where he was raised.

[216] Patroni 1896, p. 48-49.

[217] Patroni 1897. On his chauvinistic use of iconography see Oddo (*forthcoming*).

problem that crosses the barriers between archaeology and history as well as the ones between classical and protohistoric studies.[218] Indeed, 1) the foundation-dates of Greek colonies in Sicily and southern Italy are connected to 2) the chronology of proto-Corinthian pottery and 3) the first contacts between the Etruscans and the Greeks.[219] While the latter, which entails issues of origins and of priority (e.g. on the birth of urban settlements), only indirectly relates to the Fusco necropolis,[220] the former are directly impacted by the interpretation of the Fusco necropolis.

Other sites and colonies – e.g. Pithecusae and Cumae – share this centrality in the history of Greek colonisation and, therefore, in the construction of typological sequences used to date Greek and pre-Roman sites all over the Mediterranean. These sites have usually been continuously studied from their discovery to the present day, and for some the history of such studies has also been written.[221] This

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[218] It was already clear in Patroni 1896. While the interdisciplinarity of the problem is already recognised by Dunbabin (1948, 435) it is not until 2005 that scholars from different disciplines gathered to discuss it (Bartoloni, Delpino 2005).
[219] Dunbabin 1948 *loc. cit.*
[220] Though it should be remembered that bucchero findings prompted a reflection on the provenance of such vase-fabric: *vide supra* 131-132.
[221] Nizzo 2007 on Pithecusae; for Cumae see Valenza and Rescigno 2010 and Nizzo 2008 among others.

is not the case for the Fusco necropolis.[222] Fusco is as much essential to chronology building as it is understudied. This allows us to unveil a certain number of assumptions and generalisations used for the construction of the proto-Corinthian sequence and for connecting it to foundation dates.

Indeed, assumptions and generalisations are always part of archaeological inductions,[223] but in this case, they are more visible, as they are required to fill larger voids than usual. Thus, the cultural context that constitutes the condition of possibility of such assumptions and generalisations becomes clearer.

Here we will address some recurring issues regarding the interpretation of the available data. In doing so, the author of this dissertation elucidates the logic of the arguments that

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222 After Orsi 1915, Arias (1936 and 1941) discussed at length the provenance of large geometric ossuaries, Villard and Vallet (1952) and Neeft (1987) re-examined Corinthian aryballoi from the Fusco excavations. More recently Pelagatti (1984) analysed 14 kraters from the Fusco necropolis in comparison with fragments found in the Foro Siracusano, in the Giardino Spagna and in Viale P.Orsi, as well as in the new excavations in Ortigia, defining in detail this very specific class of artefacts. However, a global reconsideration of the necropolis is yet to be done. See Neft 2012, 487-488, on the unpublished or partially published major excavations conducted in Syracuse in the last sixty years.

223 Among the many works on the subject see at least Clarke 1968 and 1972 and Gardin's response to him (1970 and 2000).

produced different – sometimes contrasting – interpretation of the Fusco necropolis. This pages also offer an analysis of the various attempts at anchoring the relative chronology of proto-Corinthian pottery to absolute dates. This analysis prompts a reflection on our current relationship with ancient written sources and on the preference given to historical dating over other available methods.

2.2.1 Vases and chronology: building the proto-Corinthian typology.

Throughout this chapter, the importance of the Fusco necropolis – especially the publications of Paolo Orsi – to constructing the proto-Corinthian pottery chronology has repeatedly been highlighted. It is worth noting that the relevance of this sequence resides in the fact that the chronology of other vase-fabrics and artefact classes is anchored to it.[224]

Because of this, several later researchers published papers and books that discussed the chronology of proto-

[224] Among others, notoriously, the Daedalic style (Ducat 1957, 165-166 and 1962, 165-166); the Rhodian vase-fabric is also dependent on the chronology of Corinthian vases (Ducat 1962, 166, footnote 4, and Schiering 1957; see Bossolino 2019 for a recent reassessment of the Geometric necropolises of Kamiros).

Corinthian pottery and its connection to dating the founding of Sicilian colonies. Most of them employ the Fusco necropolis as a key site for reconstructing their chronological sequences.[225]

Giving a detailed description of these essays would go beyond the scope of this work. However, a brief summary will help in better understanding the arguments brought forward.[226] For a schematic overview of the different hypotheses, the reader is referred to the summary table provided by Ducat (Fig. 26).

[225] Neeft 1987, 363-365.

[226] The goal of this dissertation is not, strictly speaking, to assess the exactitude of such chronological constructions: therefore, the following list of publications should not be regarded as a comprehensive literature review.

Figure 26 - Ducat, J. 1962, Plate 1.

TABLEAU I : CHRONOLOGIES SUIVANT LES PRINCIPES DE PAYNE

	Johansen *V S* 1923 p. 185	Payne *P K V* 1933 p. 20 *N C* 1931 *passim*	Matz *Gnomon* 1937 p. 407	*Buschor *Gr. Vasen* 1940 p. 25-35 p. 63-73	Kübler *A A* 1943 col. 417-441	Kübler *Kerameikos* VI 1959	Åkerström *Geom. Stil in Italien* 1943 p. 33 & 165	Weinberg *Corinth* VII, I 1943 p. 33-83	Hopper *A B S A* 1949 p. 180	R M Cook *Greek Painted pottery* 1960 p. 44-60	*Byvanck *Mnemosyne* XIII 1947 p. 249-250
P C ancien	800-725	750-700				740/35-715/10	débute vers 700-675	725-700		725-700	725 ?-680 ?
P C moyen I 1° style F N	725-690	700-675	690-670/65	690/80-660/50		715/710-690/85		700-675		700-675	680 ?-650 ?
P C moyen II 2° style F N	690-650	675-650	665-640	660/50-640		690/85-665/50		675-650		675-650	
P C récent		650-640	640-625	640-625	650-625	665/60-640/35			650-630	650-640	autour de 600
Transitional		640-625	625-610	625-vers 610	625-610	640-35-620/15			630-620/15	640-625	
C ancien		625-600	610-590	vers 610-vers 590	610-580	620/15-595/90			620/15-590	625-600	
C moyen		600-580	590-570	vers 590-575	580-550					600-575	
C récent I		580-550	570-550	575-550	après 550					575-550	

In 1923, K. Friis Johansen wrote an important book on this class of vases, which he believed to be produced in Sicyon: he took as his type fossil the *aryballos* - particularly its evolving shape (from the globular to the piriform type). He noted that the globular *aryballoi* were found in the Fusco necropolis but could not be found in Gela. He therefore anchored their period of circulation to the two canonical founding years of Syracuse in 733 BC (when the early proto-Corinthian style was already in use) and of Gela in 688 BC (when the style had already been abandoned).[227] Humphry Payne, whose work on Corinthian pottery was encouraged and proofread by J.D. Beazley,[228] wrote a first book on the topic in 1931. He would refer the reader to Johansen's work for the chronology of the proto-Corinthian style. But he added a *terminus ante quem* for the end of the late proto-Corinthian, i.e. the foundation-date of Selinus (628 BC) where piriform *aryballoi* were hardly ever found.[229] Two years later he was also doubting the upper limit of Johansen's construction, proposing a younger date for it.[230] In 1941, Weinberg proposed ca. 725 BC as the upper limit of proto-Corinthian pottery, basing his argument mainly on the evolution of decorative styles and anchoring this chronological determination to the 'earliest cemeteries' of

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[227] Johansen 1923, 181-185.
[228] Payne 1931, xi.
[229] *Ibidem*, 23.
[230] Payne 1933, 20.

Cumae and Syracuse.[231] The resulting system underwent additions and redefinitions by Johansen, Payne and Weinberg. Finally, it was corroborated by Dunbabin[232] and became the accepted chronology for the late VIII and VII century BC. This mainstream chronological framework was challenged by a 'low chronology school', starting with Byvanck's[233] and Åkerström[234]. Their view gained traction in the archaeological community through the works of Villard and Vallet[235]. The accuracy of the traditional dates for the founding of Greek colonies was being effectively questioned by Van Compernolle.[236] Among the more recent essays on the topic, one should mention Courbin's computerised classification of proto-Corinthian *skyphoi.* It is worth of notice both for the early use of computer statistics and for the choice of a different form than *aryballoi.*[237] Finally, among the many re-evaluations of proto-Corinthian pottery, which

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231 Weinberg 1941, 35-37.

232 Dunbabin 1948, 435-470 and especially 452-460.

233 His article was issued in 1937 in German but the 1947 revised English version was consulted.

234 Åkerström 1943.

235 The authors discussed the chronology of Corinthian pottery in several venues: see at least Villard 1948 and Vallet and Villard 1952.

236 Van Compernolle 1959, especially 409-436. In general, scholars such as Feeney (2007, 43-67) have noticed that ancient historians establish synchronicities between crucial events of their history and anchor them to certain dates that appear particularly crowed with historical events (e.g. 480 BC).

237 Courbin 1983.

are mostly the result of new excavations, Neeft keeps using Syracuse (and therefore the Fusco necropolis)[238] as a key site for the construction of his chronological framework.[239] He follows the mainstream system built, adding further details and refining it (Fig. 27).

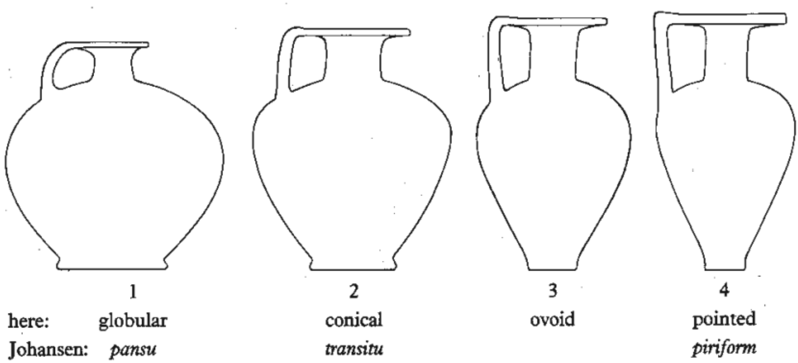

Figure 27 – Neeft 2012, Fig. 1.

Instead of repeating the arguments of each scholar, we will analyse some key points of the discussion: recurrent points of contention and methodological fallacies repeatedly reported by the authors against their opponents.

First, it should be noted that the controversies that have arisen in regard to proto-Corinthian pottery predominantly

[238] Other proto-Corinthian vessels and fragments have been found in Ortigia (Athenaion) and in the Foro Siracusano, but the evidence from the Fusco necropolis is more abundant and orderly, therefore it is often the main corpus of evidence used in general works.
[239] Neeft 1987 and 2012.

rest on its absolute chronology. The relative order of types and phases provided by Johansen and Payne has been redefined and improved, but never radically challenged.[240]

In fact, one crucial point of this controversy is the nature of the groupings – types, styles, groups, etc. – that need to be put in a sequence and assigned a date. By contrast, the fundamental time variables appear to have never been disputed. The shared characteristics that determine whether an artefact belongs to one group or another are not always explicit.[241] It should be noted that, even when implicit, the variables that are deemed to be time-sensitive (and therefore a chronological indicator) are always shape and decoration,[242] which are sometimes supplemented by size.[243] Disagreements arise on the specific aspects of shape and decoration that should be taken into consideration for chronological purpose – the neck? the maximum diameter?

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[240] Villard, Vallet (1952) argue for the foundation of Megara Hyblaea to have happened before the foundation of Syracuse. However, their argument is based on the accepted sequence of development of Corinthian style (Ducat 1962, 167).

[241] This is one of Neeft's (1987, 19) main criticisms towards his predecessors. He is rather explicit in his grouping criteria and, by necessity, so is Courbin (1983).

[242] The one scholar who disagreed – this avenue of inquiry could have been groundbreaking, but he did not pursue it further and sought refuge in decoration – was Coldstream (1968) stating that shape was not a reliable chronological indicator for proto-Corinthian pottery.

[243] Neeft 1987, 299-301.

the diameter/height ratio? etc.[244] It should be noted that such groupings and their time-sensitive characteristics are usually treated in a nomothetic way,[245] which does not allow for outliers or probabilistic assessments. One exception is Neeft, who writes:

> "the sequence of a list [of Proto-Corinthian *aryballoi*, ndr.] offers a sketch of the probabilities to be found at any given time"[246]

However, no one, not even Neeft, use the principles of probability when linking Protocorinthian typology with other typological sequences: error bars and bell curves are nowhere to be found.

This, however, does not seem to worry scholars as much as the four main methodological fallacies that each author seems to find in his predecessors' work: 1) the use of one-

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244 Courbin 1983 provides interesting tables on the matter.
245 *sensu* Clarke 1968.
246 Neeft 1987, 19.

specimen arguments;[247] 2) circular arguments;[248] 3) *ex absentia* and *ex silentio* arguments;[249] 4) fallacy of Worsaae's law.[250]

When looking at these assumptions and approximations from a cultural perspective, one can find some of the main theoretical and cultural issues of our century lurking in the shadows of reasoning. A quote from one of the most

[247] E.g. Neeft (1987, 363-365) argues that Fusco *aryballoi* from Tombs 312, 223 and 326 are typologically later than the *aryballos* found in Taras (Inv. 52718) and this provides a *terminus post quem* for the Fusco graves: 706 BC, i.e. the foundation-date of Taras. Besides the fact that a single *aryballos* is adopted as a chronological anchor, one should notice that the *aryballoi* of the Fusco have been conflated with their tomb and the *aryballos* of Taras with the foundation-date of the colony.

[248] Among numerous others, see Ducat's (1962) criticism on the argument of Villard and Vallet (1952), and Dunbabin (1948, 460-470) criticisms towards lower chronology proponents.

[249] E.g. Johansen (1923) and Payne (1931) establish the lower boundaries of globular and pyriform *aryballoi* based on their absence in Gela and Selinus, respectively.

[250] Authors envisage the possibility that artefacts deposed in the same grave are not necessarily synchronous, because there could be heirlooms (this is one of the main concerns of Neeft 1987). Therefore, the date provided for a tomb by an object contained in the tomb is only a limit date, a *terminus post quem*. However, when the need arises to produce an absolute date – i.e. they need to be combined with other data from typology or historiography – they are converted into finite intervals of time, and synchronicities are implicitly assumed (see Neeft 1987 himself on Pithecussae T. 325). This practice, in combination with the use of single-specimen arguments, gives their opponents the opportunity to pose the question 'what if this one object was an heirloom?'.

acclaimed historical chronology manuals of the XIX century should help to unveil them:

> "[…] un vaso viene datato secondo lo stile, cioè introdotto nell'evoluzione stilistica. L'evoluzione stessa deve poi essere collocata nel tempo con un riferimento alla cronologia antica. La datazione dei vasi greci arcaici è basata per esempio sui ritrovamenti avvenuti in Italia. La successione ammessa per questi dipende a sua volta dalla cronologia relativa delle colonie greche secondo Tucidide (VI 3): Gela fu fondata 45 anni dopo Siracusa, ecc. La cronologia relativa di Tucidide si trasforma in datazione assoluta con l'ausilio delle notizie di Eusebio […]"[251]

Let us comment the three main statements that emerge form this quote.

First, a vase is dated for its style. Except for Åkerström,[252] the authors concerned with the dating of proto-Corinthian pottery do not usually fathom the possibility that the chronological boundaries of a type or phase may be different

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[251] Bickerman 1963, 2. It should be noted that Bickerman purposely separates historical and archaeological chronology, concerning himself with the mechanisms and methods regulating the first one, being aware that it forms the base for a large part of the second one.
[252] Åkerström 1943. Strongly criticised by Dunbabin (1948, 466-470) for this reason.

in different geographical or social contexts. This is possibly due to the practicality of needing proto-Corinthian pottery to be a sequence of reference for the chronology of the VIII – VII centuries BC. However, it may also be linked to a somewhat absolute idea of style in the study of classical art, derived from German and Neapolitan idealism.[253]

Second, the sequence of vases is dated by the sequence of *apoikiai*. For this to be possible, the chronology of the vases – mostly *aryballoi* – is conflated with the chronology of the tombs and, most importantly, with the chronology of the colony founding dates.[254] Indeed, for the founding of the colony to be archaeologically distinguishable, one has to assume that artefacts are, to some extent, a manifestation of ethnic traits: the chronologist needs to identify actual Greek tombs – to be dated after the foundation of colonies – and indigenous tombs – that can be dated before them.[255]

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253 Barbanera 2015 provides several hints on the great influence of Crocian idealism on Italian classical archaeology, from Paolo Orsi to Ranuccio Bianchi Bandinelli.

254 In turn, the foundation dates of the *apoikiai* are calculated through a series of synchronisms and approximations needed to bridge what Bickerman calls 'chronology' (i.e. ancient calendrical systems) and 'chronography' (i.e. ancient systems for recording the succession of historical events in time, such as kings' lists) and, then, reducing them to the modern chronographic system (see Bickerman 1968, 64 ff.)

255 The modern gaze operating ethnic divisions in the study of Greek colonization is the subject of numerous studies (see Nijober 2011a and 2011b, Hall 2016, Esposito and Pollini 2016 and Zuchtriegel 2017 for an

Third, the sequence of *apoikiai* and their absolute dating is based on Thucydides and Eusebius. However, the accuracy of Thucydides' dating system has itself been a topic of unresolved debate.[256] While an analysis of this debate would exceed the scope of this dissertation, it is worth noting that a subtle comparison with the modern concept of historiography is implicit in the opposite arguments of both Dunbabin (arguing for annalistic history, similar to our notion of chronologic history) and Van Compernolle (arguing for oral genealogic tradition, closer to the notion of history found in 'primitive' people).[257] In this *querelle*, as in many other aspects of classical scholarship, our interpretations are influenced by the relationship – of continuity or discontinuity – we see between the classical past and our own civilization.

Finally, one last remark seems worthy of notice. The debate on the chronology of proto-Corinthian pottery does not

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extensive bibliography). However, to my knowledge, this is the first time a connection is established between this approach and chronology building.

[256] For a critical overview of the debate see Greco and Lombardo 2012; Nizzo 2016 (with a special attention to the chronological aspects of the debate) and in general all the essays in that volume (Nizzo, Donnellan, Burgers 2016).

[257] Dunbabin 1948, 447-452; Van Compernolle 1959, 409-436 (but much more explicitly the summary of his arguments in Ducat 1962).

make use of any of the so-called 'hard methods'.[258] Neither radiocarbon nor dendrochronology have entered the discourse, if not through cross-dating with other typological series that were affected by those methods. One might argue that the main resistance towards radiocarbon is its (perceived) lack of precision.[259] Dendrochronology requires a very specific kind of remains. This strive for precision – even at the evident expense of accuracy – appears to be driven by a (sub)conscious subordination of archaeological evidence and methods to the textual sources: the latter often shape the questions we ask of archaeological evidence in Magna Graecia and Sicily.

2. 2 Conclusion

The Fusco necropolis has proven to be an effective example of the role that cultural factors play in the definition of chronological problems and disputes. First, it has been shown that the very questions asked of archaeological evidence – even more than the answers – are influenced by political agendas and ideologies. Second, it was highlighted that the selection of variables believed to be time indicators

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[258] This is sensibly different if we consider the broader discussion on the chronology of Italian Iron Age: see Nijober's numerous articles (at least 2016, 2013, 2008) and Bartoloni, Delpino 2005.

[259] See for example the conclusions of D'Agostino (2005) in the Rome conference (Bartoloni, Delpino 2005) or the critics to Nijober 2013.

can be dependent on disciplinary traditions: for classical archaeology – where artistic theory is consistently a big component of scholars' education – this time sensitive variable is often style, intended as a combination of shape and ornament. Third, the choice of dating methods and their perceived validity is dependent on the intellectual context.

The next section will therefore focus on dating methods and the historical and intellectual conditions under which they were invented and adopted.

Chapter 3

Relative dating, the Pharaohs, and the fossil man

> « Dans cette étude des êtres qui ne sont plus, leurs traces superposées, sorte d'échelle des jours écoulés, seront nos tablettes historiques, tablettes authentiques, car la poussière des âges ne s'improvise point et la couleur des siècles est inimitable. »
>
> Boucher de Perthes 1847, 17

Histories of archaeology – in particular, the ones written by prehistorians – usually devote one or more chapters to the definition of the three-age system and to the development of methods such as stratigraphy, typology and seriation as defining moments in the history of the discipline[260]. They tend to highlight the role of geology in the acquisition of a

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[260] Stienburg 1994, 46-49; Gran-Aymerich 1998; Trigger 1989, 73-86; Trigger 1996, 129-138; Guidi 1988; Daniel 1963, 38-60; Daniel 1976. Schnapp (1996, 298-303) while understanding these developments in a their wider historical context (his book investigates how ancient objects have been used for the construction of history from classical antiquity to modern days) he devotes a paragraph to the 'Invention of Archaeology' where he discusses the three-age system. He also describes the construction of 'types' (be them taxonomic or stylistic tools) as the crucial element that emancipates archaeology from antiquarianism (*Ibidem*, 321-324).

scientific archaeological methodology[261] and find in early 19th century Denmark the 'birthplace' of the discipline[262]. By contrast, histories of classical archaeology often see the roots of the discipline in antiquarianism, emphasizing the importance of the taxonomic approach and of the evidentiary value attributed to material remains for the development of archaeology as an empirical science[263]. However, in several instances, it has been shown that the two worlds of natural sciences and of antiquarianism were not as separate as one might think and that intellectual cross-fertilisation contributed to the development and acquisition

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261 This was the account already given by De Mortillet 1883, 1-7.

262 De Mortillet 1883 (pp.5-7) acknowledges the role of previous scholars – from Eckard to Goguet and Lucretius – and of other nations in the definition of some key concepts of archaeology. However, he turns to Thomsen and Worsaae as the 'father founders' of the discipline. The attention devoted to such iconic personalities has not decreased: to this day, most histories of archaeology devote several paragraphs to their achievements: cf. Trigger 2006, 121-138; Guidi 1988; Daniel 1963, 38-60; Daniel 1981.

263 Lynch and Lynch 1968; Pucci (1993) does not intend to write a history of archaeology, but he does highlight the relevance of antiquarianism in the development of an empirical approach to human sciences; Barbanera 2015 in his 'History of Classical Archaeology in Italy' does not mention Cuvier or Darwin and he does not discuss the debate on the 'deep history' of humankind, though he does comprehensively discuss at length the dialectic interaction between historians of classical art and empirical archaeologists (particularly Fiorelli and Paolo Orsi) at the time; Himmelmann 1981; as mentioned above (footnote 1) Schnapp (1996) pursues a wider narrative where antiquarianism intermingles with several disciplines and personalities.

of archaeological paradigms, as well as methods, such as the three-age system and evolutionary typology[264].

The methods that are usually said to have 'given birth' to the discipline are mostly tools for establishing chronology, notably relative chronology. Therefore, the invention and development of such methods has been studied extensively, with several papers and monographs devoted to the

[264] Eskildsen 2012 provides a very detailed account of the intellectual context in which Thomsen developed the three-age system, focusing on the development of display strategies used in the Royal Museum of Nordic Antiquities in Copenhagen and on the commentaries of visitors. Through the analysis of personal correspondence and of his library, the intellectual background of Thomsen is investigated, placing him at the crossroad of antiquarianism, ethnography and comparative anatomy. Oscar Montelius, while being a member of the Society of Antiquaries in London, considered himself an evolutionist, to the point that he came to question human agency in history: his intellectual background is widely discussed in Baudou 2012 (the English review by Goldhahn 2012 has been consulted) and in Gråslund 1987. The background of Petrie in mathematics and the consequent application of statistical principles to archaeology have been the subject of several studies: Kendall 1963 and, most recently, Gertzen and Grötschel 2012. Moreover, Debbie Challis' (2013) monograph has recently established the influence of Galton's writings on the arrangement of objects in the UCL Museum of Egyptian Antiquities, when the director was Flinders Petrie (cf. also Sheppard 2010). Among the numerous studies on the proximity of natural sciences and humanities and on the relevance of their interplay to the definition of disciplines and methods along the XVIII and XIX centuries, it is worth mentioning Rossi 1979 and Ginzburg 2019 for their congruity with the following discussion and the abundance of bibliographical references.

intellectual biographies of their inventors[265] and to the adoption of said methods in different national contexts[266].

This chapter aims at presenting a specific point of view, focussing on the intellectual *milieu* where these methodologies were initially developed and applied. The first paragraph deals with the debate on 'uniformitarianism' and 'catastrophism', investigating its impact on archaeological practice and on dating methods in particular. The second section delves into the history of the three-age system through the works of the scholars who introduced it into the archaeological discourse: Thomsen and Worsaae. The mechanisms underlying the comparative method used by Thomsen and its proximity to both anatomy and ethnography are here of special interest. Finally, the last paragraph is devoted to Oscar Montelius and Flinders Petrie, two prominent scholars who faced the same problem,

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[265] The amount of literature on Christian Jürgensen Thomsen is impressive: besides Eskildsen 2012, it is worth mentioning the chapter devoted to him in Gråslund 1987, 17-30, and the biography written by Lund Hansen 1988, as well as the collective volume edited by Wiséhn 1988. Jensen 1992 and Jakobsen 2004 discuss his activities and methodology as Museum director, while Jolles 1999 frames his approach in the larger picture of German Romantic Chronology. Less abundant is the literature on Worsaae's life and work and it is mostly centred on his very blatant nationalistic ideology: Gråslund 1987 devotes some pages to him and his methodology, Briggs 2005 and Dìaz-Andreu 2014, 24-49, are mostly concerned with his nationalistic claims.

[266] Among others, Nash 2000, O'Brien and Lyman 2002, Lyman and O'Brien 2006, Gråslund 1987, and Rowley-Conwy 2007.

namely ordering artefacts into sequences, but proposed two different solutions. While highlighting the complex intellectual filiations of scholars and their stance towards the most heated scientific debates of the XIX century, the aim of this chapter is to offer some thoughts on the methodological implications of such theoretical and ideological positions, showing how they had an impact on the variables and models deemed valid for measuring time through objects.

It has already been noted that most of the methods discussed in this chapter are meant for establishing relative chronology. Indeed, after the demolition of biblical chronology, and until the first decades of the twentieth century, the efforts of archaeologists were mostly directed towards *relative chronology*. The purpose of dating methods was, in this period, the construction of a grid of events and eras, and the identification of relationships of anteriority/posteriority between them[267]. A famous statement of Flinders Petrie can be considered an

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[267] On the relevance of relative dating and of establishing diachronic and synchronic relationship between distant contexts in the second half of the XIX century Cf. Blinman 2000 (specifically on the beginning of ceramic dating in North America); Stein 2000 (specifically on the adoption of systematic stratigraphy in North America); O'Brien and Lyman 2002, 23-59 (specifically on the adoption of typology in Americanist archaeology); O'Brien and Lyman 2006, 144-163 (specifically on seriation strategies in the Mississippi Valley); Stig Sørensen and Reba y-Salisbury 2008 (specifically on the construction of the Urnenfelderzeit in XIX century Germany).

emblematic example of such attitude towards archaeological time:

> "[T]he main value of dates is to show the sequence of events; and it would matter very little if the time from Augustus to Constantine had occupied six centuries instead of three, or if Alexander had lived only two centuries before Augustus. The order of events and the relation of one country to another is the main essential in history." [268]

3.1 Beyond the Bible

Ironically, a chapter devoted to relative dating methods begins with a discussion concerning absolute chronology. Indeed, to ensure clarity in the following arguments, it is first necessary to introduce some key aspects of the debate on the antiquity of man, and the consequent rejection of Biblical chronology.

This dissertation will not provide a detailed account of the so-called 'discovery of the deep past', which is manifold and the result of a long process that involved a variety of disciplines (philosophy, astronomy, geology, natural

[268] Petrie 1899:295

sciences and, in the end, archaeology) and lasted over a century[269]. During this time, between the second half of the XVIII century and the end of the XIX century, the idea of the great antiquity of the Earth and of its inhabitants, particularly of humankind, was conceived of, fiercely debated and then largely accepted[270]. While this is not the

[269] Several volumes and articles have been written on this topic: see at least Toulmin 1965, Albritton 1980, Rudwick 2005 and 2014: these authors focus on the intellectual context and the different disciplines and philosophical positions involved in the debate; Rossi 1979 provides a similar account of the premises of such debate, from Hooke to Vico; Dalrymple 1991, Richter 1986 and Lewis 2000 focus on geochronology, and Burchfield 1975 devotes special attention to the role of Lord Kelvin in the definition and affirmation of the antiquity of the Earth; Grayson 1983, Van Riper 1993 and Sackett 2000 are mostly concerned with the archaeological evidence for the antiquity of man and the surrounding debates.

[270] Many scholars in the 1870s and 1880s speak of the antiquity of man as an established fact (cf. Munn and Beach 1872 on *Scientific American*, Haynes 1880 on *Popular Science Monthly*) and even prominent Catholic scholars affirm it: «Dateci dunque, dicevo, un vero propriamente dimostrato, come quello, per esempio, che il mondo non s'è fatto in sei giorni, ma in milioni di anni e di secoli, e, per quanto possa sembrare contrario alla fede, lo ammetteremo senza esitazione, senza rimorsi, anche non intendendo come si concili colla fede; per questa ragione semplicissima, e certissima a priori, che, ciò che si credeva o si crede di contrario al vero dimostrato, non si credeva né si crede per fede appoggiata alla Rivelazione, ma per falsa interpretazione della Rivelazione stessa. È questa per noi dottrina cattolica.» (Stoppani 1884, 74). However, in the early years of the XX century further books continued to be published to provide additional evidence for the

place for reporting in any detail the large amount of studies conducted on this crucial junction of modern intellectual history, some aspects of the debate should nonetheless be mentioned. Indeed, the controversy over the age of man was the context where many relative dating methods were conceived of, tested and applied. This controversy may even be considered a condition of possibility for their development: the soundness required for an argument to contradict the Bible forced scholars to develop robust methodologies, which would give empirical observations the strength to disprove the sacred text. In this sense, biblical chronology should be understood to be something more than just a limitation to scientific endeavours: it became a compelling reason for many disciplines to refine their methods and to rely on empirical evidence.

In 1727 John Conduitt published a manuscript, written by Isaac Newton, on the reconciliation of ancient chronologies with biblical chronology. In the last paragraph of the introduction he writes:

> "I have drawn up the following Chronological Table, so as to make Chronology suit with the Course of Nature, with Astronomy, with Sacred

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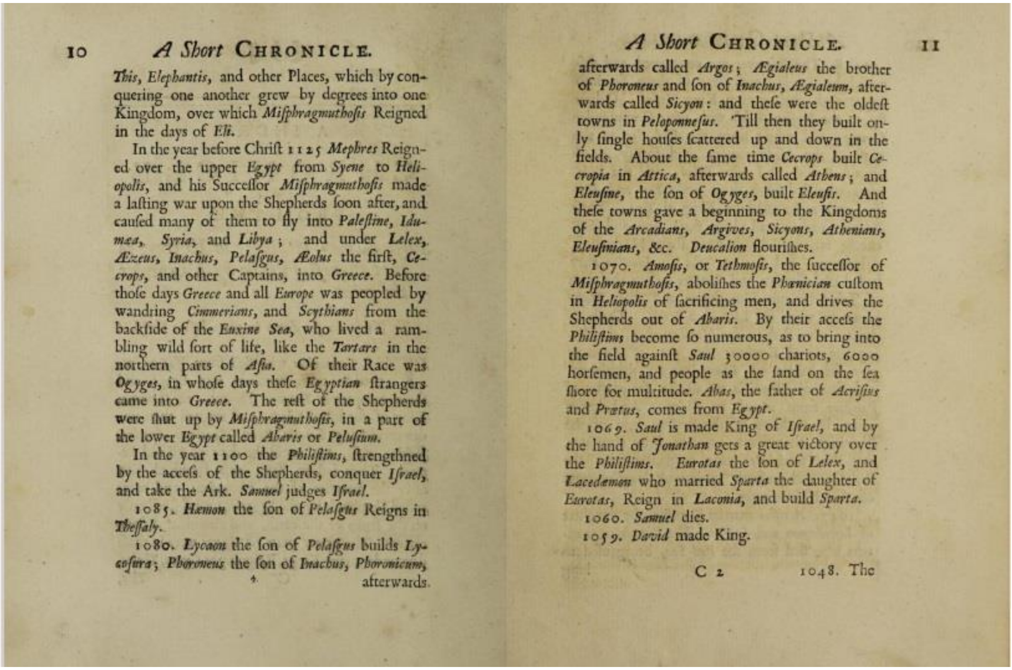
antiquity of man (e.g. MacCurdy 1910, Wright 1912, Keith 1915): this means that there was still resistance against this concept.

History, with Herodotus the Father of History, and with itself"[271].

The main sources of chronological information, at the beginning of the XVIII century, were indeed astronomical observations, the Bible and classical sources, which were not always in agreement.



10 A Short CHRONICLE.

This, Elephantis, and other Places, which by conquering one another grew by degrees into one Kingdom, over which Miſphragmuthoſis Reigned in the days of Eli.

In the year before Chriſt 1125 Mephres Reigned over the upper Egypt from Syene to Heliopolis, and his Succeſſor Miſphragmuthoſis made a laſting war upon the Shepherds ſoon after, and cauſed many of them to fly into Paleſtine, Idumæa, Syria, and Libya; and under Lelex, Æzeus, Inachus, Pelaſgus, Æolus the firſt, Cecrops, and other Captains, into Greece. Before thoſe days Greece and all Europe was peopled by wandring Cimmerians, and Scythians from the backſide of the Euxine Sea, who lived a rambling wild ſort of life, like the Tartars in the northern parts of Aſia. Of their Race was Ogyges, in whoſe days theſe Egyptian ſtrangers came into Greece. The reſt of the Shepherds were ſhut up by Miſphragmuthoſis, in a part of the lower Egypt called Abaris or Peluſium.

In the year 1100 the Philiſtims, ſtrengthned by the acceſs of the Shepherds, conquer Iſrael, and take the Ark. Samuel judges Iſrael.

1085. Hæmon the ſon of Pelaſgus Reigns in Theſſaly.

1080. Lycaon the ſon of Pelaſgus builds Lycoſura; Phoroneus the ſon of Inachus, Phoronicum, afterwards

A Short CHRONICLE. 11

afterwards called Argos; Ægialeus the brother of Phoroneus and ſon of Inachus, Ægialeum, afterwards called Sicyon: and theſe were the oldeſt towns in Peloponneſus. 'Till then they built only ſingle houſes ſcattered up and down in the fields. About the ſame time Cecrops built Cecropia in Attica, afterwards called Athens; and Eleuſine, the ſon of Ogyges, built Eleuſis. And theſe towns gave a beginning to the Kingdoms of the Arcadians, Argives, Sicyons, Athenians, Eleuſinians, &c. Deucalion flouriſhes.

1070. Amoſis, or Tethmoſis, the ſucceſſor of Miſphragmuthoſis, aboliſhes the Phœnician cuſtom in Heliopolis of ſacrificing men, and drives the Shepherds out of Abaris. By their acceſs the Philiſtims become ſo numerous, as to bring into the field againſt Saul 30000 chariots, 6000 horſemen, and people as the ſand on the ſea ſhore for multitude. Abas, the father of Acriſius and Prœtus, comes from Egypt.

1069. Saul is made King of Iſrael, and by the hand of Jonathan gets a great victory over the Philiſtims. Eurotas the ſon of Lelex, and Lacedæmon who married Sparta the daughter of Eurotas, Reign in Laconia, and build Sparta.

1060. Samuel dies.

1059. David made King.

C 2 1048. The

Figure 28 - Isaac Newton, The Chronology of Ancient Kingdoms Amended, 1727, 10-11.

[271] Newton† 1728, 8.

The Chronological Table mentioned in the above citation looked more like a list (Fig. 28), starting from 1125 B.C. with the reign of the Egyptian pharaoh Mephres. This was, according to the author, 'the first memory of things in Europe'.[272] While providing an account compatible with traditional chronologies, Newton did not include the day of creation in his list, nor did he admit the possibility of constructing chronology beyond (written) memory[273]. Heathen times were the object of philosophical or theological enquiry, not of empirical sciences[274]. This

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[272] *Ibidem*, 9.

[273] "Some have made the Kings of Germany as old as the Flood : and yet before the use of letters, the names and actions of men could scarce be remembered above eighty or an hundred years after their deaths: and therefore I admit no Chronology of things done in Europe, above eighty years before Cadmus brought letters into Europe; none, of things done in Germany, before the rise of the Roman Empire." Newton† 1728, 7. Cf. Bedford 1728 (*Animadversions*, iii): his main criticism against Newton's account is the distance from biblical sources and their canonical interpretation: Bedford proudly states that he never departs more than 5 years from Ussher's chronology.

[274] Cf. Zedelmeier 2003 on the debate between philosophers and historians on the early history of humanity and related disciplinary boundaries, involving the legitimacy of universal histories as an academic genre. For a detailed account of the theological debate on the pre-Adamites, and especially its diffusion during the Enlightenment, see Livingston 2008. Schnapp 2008 provides a distinctive point of view on the pre-Adamites issue, focusing on the role of material remains in the debate. At the beginning of the XIX century, one of the main battles of Thomsen was to emancipate 'heathen times' from philosophy and

approach was to change remarkably over the next decades[275]. Let us consider the words of Lewis H. Morgan, in the Preface to his 1877 book *Ancient Society*:

> "The great antiquity of mankind upon the earth has been conclusively established. […] Since the probable length of their career ['their' = 'of the branches of the human family', ndr.] is connected to geological periods, a limited measure of time is excluded. […] Whatever doubts may attend any

universal history, making it a subject of empirical knowledge (cf. Eskildsen 2012).

[275] Scientists soon came to be concerned with the age of the earth and with the early history of mankind. A book written by Benoit de Maillet was printed in 1748 (10 years after the author's death) arguing for the great antiquity of the Earth, based on a theory of gradual lowering of sea levels. The Earl of Buffon tried to calculate the time needed for the Earth to cool off from its (supposed) original incandescent state through experiments on iron spheres (Mattinson 2015, 321), but he declared his theory on the development of planets to be just philosophical speculation (Buffon 1749). In 1785 James Hutton overtly denied traditional chronology on the basis of geological observations (see Hutton 1788) and opened the gates to recognising the great antiquity of the Earth. Hutton's theory was further refined and popularized by Lyell (1830), reaching an initial consensus in a subset of academic circles (on the debate between uniformitarianism and catastrophism, *vide infra* 221-244*)*. The actual age of the Earth was the subject of longer debates, where several disciplines were used in an attempt to estimate it (for an appraisal of the various attempts until the adoption of isotopic dating cf. Richter 1986 and Lewis 2000): Kelvin's theories have been particularly relevant to the intellectual context of his time (Burchfield 1975).

> estimate of a period, the actual duration of which is unknown, the existence of mankind extends backward immeasurably […]. It is both a natural and proper desire to learn, if possible, how all these ages upon ages of past time have been expended by mankind."[276]

In Morgan's work, geology and archaeology are the empirical sciences that legitimately investigate the times before history and establish comparisons with ethnographic observations[277]. Methods of absolute chronology might have been considered more suited for geology than (pre)history, as a result of their lack of precision. By contrast, relative chronology – in particular, the investigation of the three progressive stages of savagery, barbarism and civilisation – was indeed the main purpose of his investigation[278]. It

[276] Morgan 1877, v-vi.

[277] In the *Preface* to *Ancient Society,* Morgan (1877, i-viii) attributes to geology the fundamental role of establishing the antiquity of man and studying the succession of human races; ethnology, archaeology and – for the historical periods – philology are the disciplines used to investigate the path of human societies from barbarism to civilisation.

[278] "It can now be asserted upon convincing evidence that savagery preceded barbarism in all the tribes of mankind and barbarism is known to have preceded civilization. The history of the human race is one in source, one in experience and one in progress" Morgan 1877, vi. Hundreds of academic articles have been written on the idea of social progress in history and on its application to archaeological accounts (cf. Dunnell 1980; Kohl 1998, focusing on nationalistic aspects; some authors who keep advocating cultural evolutionism provide useful bibliography

appears evident that, during the 150 years dividing Newton's book from Morgan's monograph, a major intellectual revolution had barraged the beliefs of western societies. In 1650 Bishop Ussher, a revered theologian and the Primate of Ireland, had calculated the day of creation to be 23 October 4004 B.C. This date was added in a footnote to the 1701 edition of the English Bible and became widely accepted[279]. However, since the mid-XVIII century, natural

on their XIX century predecessors: e.g. Shennan 2002, Riede 2006, Riede, Apel and Darmark 2012) and on the comparison between modern savages and ancient people (among others cf. Launay 2010, Pettit and White 2010, Richard 2012, Goodrum 2014 with extensive bibliography) and how this refers to nationalistic ideologies and colonialism (see at least De Francesco 2013, Dietler 2010, Diaz-Andreu 2007, Gosden 2004, Lyons and Papadopoulos 2002, and Schnapp 1988, with extensive bibliography). Many of these exegetic efforts concern specifically Morgan's work and his intellectual biography (among others cf. Fortes 2017; Moses 2009; Trautman and Kabelac 1994 on Morgan's library). Providing a complete literature review on these issues would go beyond the scope of the present argument. However, the publications mentioned above provide an extensive bibliography in several languages and across various disciplines.

[279] Cf. Ussher 1650. The intellectual context of Ussher's work, and the premises on which his chronological reconstruction is built are investigated in Barr 1985.

scientists[280], geologists[281], and archaeologists[282] increasingly used experiments and material evidence to support the deep

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[280] Cf. Benoit de Maillet † 1748; George-Louis Leclerc, earl of Buffon in his *Introduction à l'histoire des minéraux* (*II Supplément à l'Histoire Naturelle*, 1775) calculated the age of the Earth, using Leibniz's idea (*Protogaea*) that the Earth was originally a sphere in a molten state. He devoted an entire chapter to the description of the *Experiment:* he registered the time needed for molten spheres of different diameter and composition to cool. Departing from the Liebnitian premises, and using Fourier's 1822 study on heat, William Thomson – better known as Lord Kelvin – estimated the age of the Earth to be between 20 and 400 Ma (Kelvin 1862). While this was a large underestimate of the antiquity of our planet, the authority of Kelvin and his prominent academic position were crucial in establishing the antiquity of the Earth as a scientific fact (cf. Burchfield 1975 and Albritton 1980).

[281] The reality of the 'Deluge' account had already been questioned several times: among others, it had been discussed by Leonardo da Vinci (Opere, Vol.II, Book XVI, Chapter VI) and Voltaire (s. v. *Inondation,* in *Dictionnaire Philosophique*). But a scientific argument for the antiquity of the earth was only provided by James Hutton who, in 1785, presented the very first account of *uniformitarianism* to the Royal Society of Edinburgh (his papers on the matter were published in 1788): he asserted the fundamental principle that, from geological phenomena observable in the present, one could infer general laws applicable to the past. Therefore, observing ongoing episodes of sedimentation, one could calculate backwards the deep past of geological layers. Charles Lyell (*Principles of geology,* 3 volumes, 1830-1833) applied this principle to observations in Europe and the Mediterranean, especially the volcanoes. Georges Cuvier (1830), while refuting uniformitarianism, would argue for a series of deluges before the biblical Deluge, using these catastrophic events to explain the extinction of species that were found in the fossil record but no longer exist. The debate between catastrophism and uniformitarianism is the subject of the next paragraph.

history of man and the antiquity of Earth. In fact, after the publication of *The Origin of Species* (1859), many prehistorians thought that the purpose of their research was to provide evidence in support of evolutionism, by both searching for the most ancient possible traces of human existence and investigating the stages of progress that divided earlier men from modern civilized society[283]. To accomplish this, the establishment of a relative chronology of archaeological sites (and regions) became one of the main goals of the discipline[284]. Biblical chronology, which had

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[282] In 1797, at the Society of Antiquaries in London, a letter from John Frere was publicly read: he reported to have found, in a site near Hoxne, worked stone tools in connection with large bones of extinct animals, deposed beneath a geological layer that appeared to be a sea floor; for this reason he suggested that the tools belonged to a very remote period "beyond that of the present world" (Frere 1800). Frere's claim was, however, largely forgotten and it was not until Boucher de Perthes announced (1840s) to have found flint artefacts in the old terrace deposits of the river Somme and demonstrated it through the 'mechanized objectivity' of photographs (Schlanger 2010, 347-351), that the idea of the antiquity of mankind started gaining momentum. The debate was then widely studied and discussed, both in academic venues and in specialized newspapers: in 1872, *The Scientific American* published a crucial article on the Antiquity of Man, popularizing the results obtained by Lyell and Boucher de Perthes.

[283] It has been argued that in the second half of the XIX century archaeologists followed more Spencer's idea of evolutionism than Darwin's theory of descent with modification (Dunnel 1989). A bibliographical essay on Evolutionary Archaeology can be found in Trigger 1996, 565-567.

[284] Trigger 1989, 100-108; Daniel 1963, 60-82;

been for a long time the backbone of western societies' self-perception, crumbled.

The debate on the Antiquity of Man, however, hides another, more fundamental conflict, one between continuity and discontinuity. Living beings and, among them, humans were found to be older than previously accepted. And, most importantly, they appeared to have changed to some degree over time. Explaining this change was the main subject of a very complex debate that took place in conjunction with and being contingent to the discussion on the Antiquity of Man: this *querelle* saw the supporters of *catastrophism* and the proponents of *uniformitarianism* oppose each other in ways that had lasting impact on archaeological methodologies[285]. This debate has been widely discussed in intellectual history and the history of science[286] and the following paragraphs do

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[285] *vide infra* 221-244.

[286] To mention a selection of relevant readings: Cannon 1960 provides an important inquiry of the topic, through the analysis of one letter by John Herschel; Rudwick 2008 provides the most recent translation and comment to Cuvier's works and a comprehensive account of previous scholarship; Outram 1986 focuses on the intellectual background of Cuvier's theories on the laws of nature; Foucault 1970 [1966], 125-165, examines Cuvier's taxonomic efforts in their historical context; Ginzburg 2019 points out the relationship between Cuvier's 'static morphology' and antiquarianism; in Hoykaas' (1963, 1-32) book on the uniformity of nature, the first chapter is devoted to the 'catstrophism vs uniformitarianism' debate; Camardi 1999 provides an insightful discussion on the place of 'uniformitarianism' in the history of scientific thought; finally, Baker 1998 uses this debate to build a history of the

not have the ambition to expand what has already been studied in detail. Here one provides a different perspective, which tries to illuminate how these two intellectual currents – and the adherence of archaeologists to the one or the other – has affected the methodologies deemed valid to extract chronological information from ancient artefacts and their contexts.

3.1.1 Catastrophism and uniformitarianism

> « Je pense donc, avec MM. Deluc et Dolomieu, que s'il y a quelque chose de constaté en géologie, c'est que la surface de notre globe a été victime d'une grande et subite révolution, dont la date ne peut remonter beaucoup au-delà de cinq ou six mille ans que cette révolution a enfoncé et fait disparaître les pays qu'habitaient auparavant les hommes et les espèces des animaux aujourd'hui les plus connus qu'elle a, au contraire, mis à sec le fond de la dernière mer, eut en a formé les pays aujourd'hui habités que c'est depuis cette révolution que le petit nombre des individus

kinds of inferences that were deemed legitimate in scientific discourse over time. The debate over the validity of the Uniformity Principle is to this day a major concern in the philosophy of science, following the article by Gould 1965.

> épargnés par elle se sont répandus et propagés sur les terrains nouvellement mis à sec, et par conséquent que c'est depuis cette époque seulement que nos sociétés ont repris une marche progressive, qu'elles ont formé des établissements, élevé des monuments, recueilli des faits naturels et combiné des systèmes scientifiques. Mais ces pays aujourd'hui habités, et que la dernière révolution a mis à sec, avaient déjà été habités auparavant, sinon par des hommes, du moins par des animaux terrestres; par conséquent une révolution précédente, au moins, les avait mis sous les eaux et si l'on peut en juger par les différents ordres d'animaux dont on y trouve des dépouilles, ils avaient peut-être subi jusqu'à deux ou trois irruptions de la mer"[287]

This passage, taken from Georges Cuvier's *Discours sur les révolutions de la surface du globe et sur les changements qu'elles ont produits dans le règne animal,* is an emblematic expression of catastrophism. According to this theory, our current civilization is four or five thousand years old, an upper chronological limit which salvaged the veracity of biblical chronology. However, Cuvier admitted that the world is much older: the Biblical Flood (*deluge*) is the last one of a

[287] Cuvier 1830, 290-291.

series of catastrophes which led to the extinction of most of the species that had inhabited the planet[288]. A series of successive creations, therefore, has rebuilt new and better versions of the world, whose sequence can be verified in the fossil record: the earth bares traces of these revolutions (Fig. 29). On the presence of human communities in previous worlds Cuvier seems to be more hesitant[289]: he remains

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[288] However, the possibility that some human communities survived some of the catastrophes in a remote and secluded part of the emerged land would be instrumental to his adaptation of monogenism to racist theories : « La plus dégradée des races humaines celle des nègres, dont les formes s'approchent le plus de la brute, et dont l'intelligence ne s'est élevée nulle part au point d'arriver à un gouvernement régulier, ni à la moindre apparence de connaissances suivies, n'a conservé nulle part d'annales ni de traditions anciennes. Elle ne peut donc nous instruire sur ce que nous cherchons, quoique tous ses caractères nous montrent clairement qu'elle a échappé à la grande catastrophe sur un autre point que les races caucasique et altaïque, dont elle était peut-être séparée depuis longtemps quand cette catastrophe arriva. » *Ibidem,* 138. On Cuvier's racism and how it relates to his anatomical works cf. Kistner 1999; he dissected the body of Sara Baartman, known as 'the Hottentot woman' and displayed it in the Muséum national d'histoire naturelle (cf. Qureshi 2004).

[289] The paragraph titled 'Il n'y a point d'os humains fossiles' (*Ibidem,* 88-93) has often been understood as a denial of human antiquity. In it, the author examines alleged findings of fossil human bones, highlighting the fallacies of geological – and sometimes even anatomical – attributions. However, in the same book, he stated: «Ce sont là les principaux animaux dont on ait recueilli les restes dans cet amas de terres, de sables et de limons, dans ce diluvium qui recouvre partout nos grandes plaines, qui remplit nos cavernes, et qui obstrue les fentes de plusieurs de nos rochers ils formaient incontestablement la population des continents à

focused on the idea of successive revolutions, denying transmutation (Lamarck) as it would go against the main principle on which his comparative anatomy was based: the correlation of parts[290]. While the correlation of parts will be discussed further in this paragraph, it is worth noting here that, to substantiate the idea of catastrophism, he had to show that the history of civilisations – or at least of those preserved in our collective memory – did not extend into the past beyond the last catastrophe, four to five thousand years ago[291]. He devoted a large section of his work to this

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l'époque de la grande catastrophe qui a détruit leurs races, et qui a préparé le sol sur lequel subsistent les animaux d'aujourd'hui. Quelque ressemblance qu'offrent certaines de ces espèces avec celles de nos jours, on ne peut disconvenir que l'ensemble de cette population n'eût un caractère très différent, et que la plupart des races qui la composaient ne soient anéanties. […] Il n'y a non plus aucun homme tous les os de notre espèce que l'on a recueillis avec ceux dont nous venons. De parler s'y trouvaient accidentellement, et leur nombre est d'ailleurs infiniment petit, ce qui ne serait sûrement pas si les hommes eussent fait alors des établissements sur les pays qu'habitaient ces animaux. Où était donc alors le genre humain ? Ce dernier et ce plus parfait ouvrage du Créateur existait-il quelque part ? […] C'est ce que l'étude des fossiles ne nous dit pas, et dans ce discours nous ne devons pas remonter à d'autres sources. » *Ibidem* 215.

[290] *Vide infra* 221-244.

[291] « En effet, bien qu'au premier coup d'œil les traditions de quelques anciens peuples, qui reculaient leur origine de tant de milliers de siècles, semblent contredire fortement cette nouveauté du monde actuel, lorsqu'on examine de plus près ces traditions, on n'est pas longtemps à s'apercevoir qu'elles n'ont rien d'historique on est bientôt convaincu, au

argument, analysing historical accounts from Greek and Roman times, while also discussing Indian and Chinese traditions.

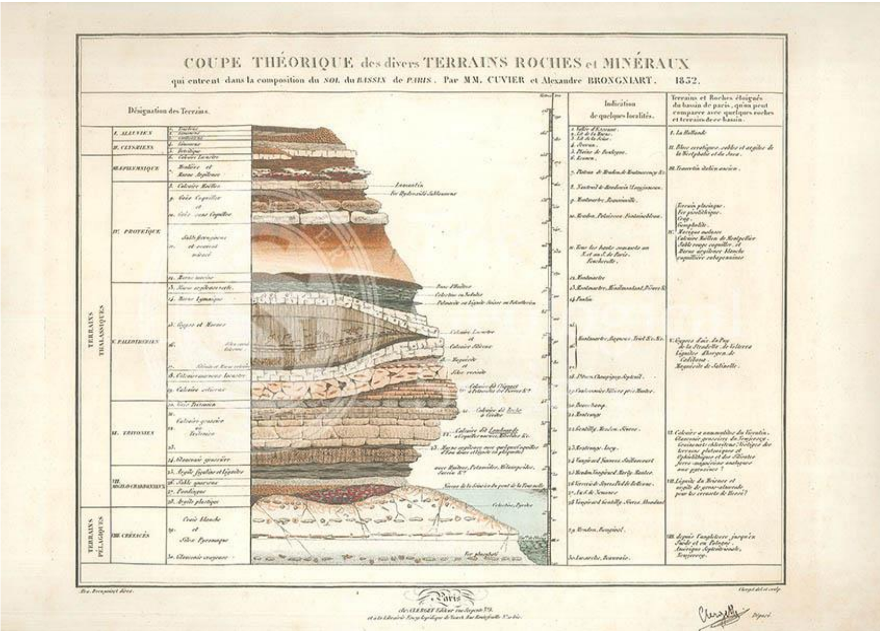

Figure 29 - Print, 1832: *Coupe theorique des divers terrains roches et mineraux qui entrent dans le composition du sol du Bassin de Paris* (tr. Eng. *Theoretical section of the sediments, rocks and minerals composing the soil in the Paris basin)*, engraved by Clerget for Georges Cuvier and Alexandre Brongniart. Image from the archives of the Geological Society.

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contraire, que la véritable histoire; et tout ce qu'elle nous a conservé de documents positifs sur les premiers établissements des nations, confirme ce que les monuments naturels avaient annoncé. La chronologie d'aucun de nos peuples d'Occident ne remonte, par un fil continu, à plus de trois mille ans » Cuvier 1825, 81.

He argued that positive historical evidence of civilisation only date back to around three thousand years and that all other accounts are mythological in nature[292]. The construction of this argument seems particularly useful to shed some light on his approach to chronology building. Two quotes from the same work will show this more clearly:

> "Voilà donc un ensemble de faits, une suite d'époques antérieures au temps présent, dont la succession peut se vérifier sans incertitude, quoique la durée de leurs intervalles ne puisse se définir avec précision; ce sont autant de points qui servent de règle et de direction à cette antique chronologie"[293]

> « C'est que chaque peuplade de Grèce qui avait conservé des traditions isolées les commençait par son déluge particulier, parce que chacune d'elles avait conservé quelque souvenir du déluge universel qui était commun à tous les peuples; et lorsque dans la suite on voulut assujettir ces diverses traditions à une chronologie commune,

[292] *Ibidem,* 115-151 : the title of this paragraph, *L'antiquité excessive attribuée à certains peuples n'a rien d'historique,* is a clear indication of the position defended in those pages.
[293] Cuvier 1830, 26.

> on crut voir des événements différents, parce que des dates toutes incertaines, peut-être toutes fausses, mais regardées chacune dans son pays comme authentique, ne se rapportaient pas entre elles."[294]

In both quotes emphasis was added to highlight a specific methodological preference expressed by Cuvier when building his theory. Indeed, the passages above show a careful consideration of the difference between precision and accuracy. The first one, in this case, indicates the ability to define exact chronological boundaries for an event (e.g. flood) or an interval of time (e.g. the period between two subsequent revolutions) on the base of available evidence. The other one relates to the reliability of evidence, the *certitude* of obtained information. Examining Greek traditions, Cuvier shows the fallacies of annalistic chronology: it provides very precise one-year dates, while losing sight of accuracy. Indeed, chronicles of different areas of ancient Greece would all account for one massive inundation, but each would provide a slightly different date for it. Thus, ancient historians postulated the existence of several different floods, with several dates, possibly all of them inaccurate (*peut-être toutes fausses*). On the contrary, stratigraphic observation is a factual evidence (*un ensemble*

[294] *Ibidem* 114.

de faits) and the relative dating of geological layers grants accuracy, even if does not grant precision. And the former appears to be more valuable for Cuvier, maybe for the first time in the history of historiography.
At least two more aspects of Cuvier's work are relevant to the development of archaeological dating methods. Significantly, they are related to his work on comparative anatomy. In a recent article, Carlo Ginzburg, recalling a statement by Cassirer, defines Cuvier's method of classification as a 'static morphology' – thus contrasting it to Goethe's dynamic morphology – as the taxa in Cuvier's system are invariable and do not allow for divergence[295]. A famous quote from the *Recherches sur les ossements fossiles* states that he could reconstruct the skeleton of an animal from a single bone, or even from a footprint in earth:

> "aujourd'hui, quelqu'un qui voit seulement la piste d'un pied fourchu peut en conclure que l'animal qui a laissé cette empreinte ruminait, et cette conclusion est tout aussi certaine qu'aucune autre en physique et en morale. Cette seule piste donne donc à celui qui l'observe, et la forme des dents, et la forme des mâchoires, et la forme des vertèbres, et la forme des les os des jambes, des cuisses, des épaules et du bassin de l'animal qui

[295] Ginzburg 2019. On Cuvier's types see also Eigen 1997.

vient de passer : c'est une marque plus sûre que toutes celles de Zadig » [296]

From the footprint of an animal one can infer its food habits and some formal aspects of its bones: the author considered it to be a solid argument, 'as certain as any other inference in physics or moral matters'[297]. The soundness attributed to this process rests on two factors, which are in turn causally related: static morphology and the concept of correlation of form[298]. Because all parts of an organism are related in a unique and invariant way, each part determines all the others, following a functional organicity of anatomic views:

"An organized being is a unique whole, an ensemble of parts which act on each other reciprocally to produce a common effect. None of its parts can be essentially altered without all the

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[296] Cuvier 1825, 51.

[297] *Ibidem.*

[298] The *loi de corrélation des formes* or *loi de corrélation organique* is known in English as the 'law of the correlation of parts': Limoges 1970 compares Cuvier's static idea of organic correlation with Darwin's dynamic theory, which also employs a principle of organic correlation. Outram 1986 discusses this topic in the larger framework of Cuvier's ideas on natural laws. Foucault (1966, 137-176) believes that Cuvier's taxonomic views and his confidence in the possibility of extrapolating general laws from anatomical observations are the *conditions de possibilité* for the development of Darwin's evolutionary theory and the formation of the *épistèmè moderne.*

others being affected by it. Therefore, there are only a certain number of possible combinations among the modifications which can be experienced by the principle organs, and beneath them there are only a certain number of possible subordinate combinations. Consequently, if one had exact knowledge of all these combinations of organs, one would therefore also have a true representation of the whole system of organized beings: all their relationships, all their properties, could be reduced to general propositions, the inner nature of each one of them could be clearly demonstrated: in other words, natural history could become an exact science."[299]

The relatedness of organs, of their form and their function, is also the reason why transmutation – and later evolution – was deemed impossible by Cuvier and his followers: if one part of an organism was to change, the rest would have collapsed, or it had to change accordingly and affecting all connected features simultaneously (as it happened with revolutions)[300]. At the same time, the strictly monothetic nature of the anatomical taxa defined by Cuvier is exactly the reason why one bone would allow the reconstruction of the entire skeleton: specific variables can be selected to be

[299] Cuvier 1819, 31 (eng. transl. from Outram 1986).

[300] Cf. Limonges 1970 mentioned above.

representative of their respective groups[301]. Cuvier admits that in his work the use of comparative anatomy for the interpretation of natural laws was inspired by antiquarian studies, where medals and coins – especially the ones that don't bear any inscriptions – were used as a source of chronological and historical information[302]. It should be

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301 cf. Eigen 1997.

302 "Antiquaire d'une espèce nouvelle, il me fallut apprendre à la fois à restaurer ces monuments des révolutions passées et à en déchiffrer le sens j'eus receuillir et à rapprocher dans leur ordre primitif les fragments dont ils se composent, à reconstruire les êtres antiques auxquels ces fragments appartenaient, à les reproduire avec leurs proportions et leurs caractères ; à les comparer enfin à ceux qui vivent aujourd'hui à la surface du globe ; art presque inconnu, et qui supposait une science à peine effleurée auparavant, celle des lois qui président aux co-existences des formes des diverses parties dans les êtres organisés." Cuvier 1825, 1. The osmotic dissemination of knowledge, skills and authority from antiquarians to paleontologists – and later vice versa – has been discussed in Schlanger 2010: the author highlights the role of John Evans, a famous numismatist, who was renowned for his work on 'uninscribed' British coins and who was one of the first authors to produce a classification of prehistoric stone artefacts. Ginzburg (2019) focuses specifically on Cuvier's attitude towards antiquarianism – the geologist defines himself 'a new species of antiquarian' (*antiquaire d'une espèce nouvelle*) – and his possible knowledge of Woodward's works, from which he may have taken the notion of an existing connection between antiquarianism and geology. The intellectual biography of Luigi Pigorini, who is considered the father of Italian prehistoric studies, is strongly influenced by Pellegrino Strobel (professor of natural history in Padua) and by Gaetano Chierici (antiquarian and director of the Cabinet of Antiquities in Reggio Emilia): on antiquarianism and science in Pigorini's work cf. Nizzo 2014.

noted, however, that many antiquarian morphologies were dynamic in nature, as they were meant to describe how objects' manufacture – and style – changed with time and society[303]. Hence antiquities, especially coins, were represented in large charts showing how successive reproductions of a model progressively deviated from the original: the visual representation of this process could sometimes resemble *ante litteram* phylogenetic trees (e.g. Fig.30).

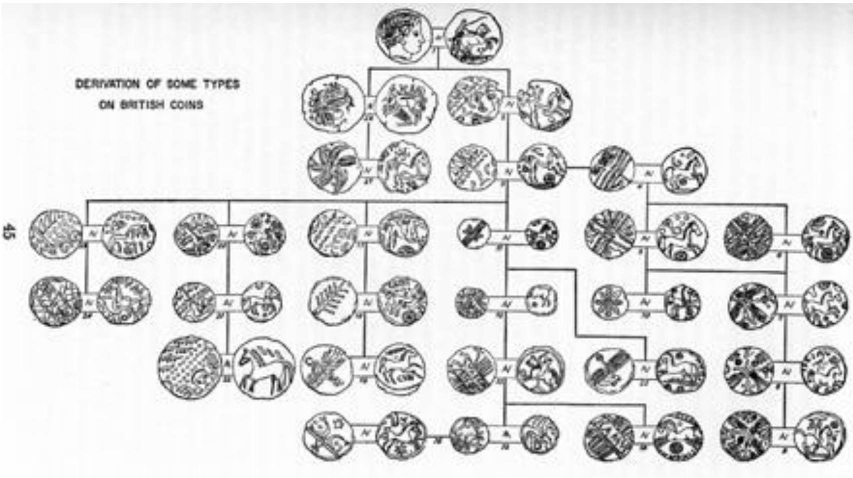

Figure 30 - John Evans, 1850, On the Date of British Coins, Numismatic Chronicle XII: Plate I.

Despite this, static morphology was largely present in the study of antiquity, both before and after Cuvier[304]. One might argue that 'static morphology' was a crucial

303 Schnapp 1996, 179-204.
304 Schnapp 1996, 316-317.

component of the theories of successive migrations that characterized European prehistory for a long time: in an organicistic view of ancient societies, change in artefacts' form and in ritual customs would indicate a change in population – not a gradual and progressive modification of existing habits[305].

After Cuvier's death in 1832, his pupils further developed his ideas, and Alcide D'Orbigny came to argue that based on geological evidence the world had undergone 28 revolutions[306]. Indeed, it was towards the middle of the XIX century that the dispute between *catastrophism* and *uniformitarianism* started to gain momentum. The last volume of Lyell's book on the *Principles of Geology* wasn't even published until 1833, when Cuvier was already dead[307].

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[305] The role of migration theories in culture-historical archaeology (cf. Adams, van Germen and Levy 1978), the intellectual and ideologic roots of this approach (Härke 1998; Clark 1994), and the importance of modern migratory movements for the interpretation of ancient ones (cf. Burmeister 2000 for a processual approach; *contra* Clark 1994) have been the subject of several studies. The articles suggested here can provide the reader with a diverse and extensive bibliography.

[306] « Une première création s'est montrée avec l'étage silurien. Après l'anéantissement de celle-ci, par une cause géologique quelconque, après un laps de temps considérable, une seconde création a eu lieu dans l'étage devonien; et successivement vingt-sept fois des créations distinctes sont venues repeupler toute la terre de ses plantes et de ses animaux, à la suite de chaque perturbation géologique qui avait tout détruit dans la nature vivante » D'Orbigny 1849-1852, II.1, 251.

[307] On the role of D'Orbigny in the dispute between catastrophism and uniformitarianism cf. Gaudant 1984.

Among all the different positions which found their expression in this debate, the key disagreement between the two main schools of thought centred around the causes that govern past geological processes: *catastrophists* argued for a world whose mechanisms and inner laws would change at any given catastrophe; *uniformitarianism* contended that the same laws operating in the present were to be used to interpret past geological processes:

> "We hear of sudden and violent revolutions of the globe, of the instantaneous elevation of mountain chains, of paroxysms of volcanic energy, declining according to some, and according to others increasing in violence, from the earliest to the latest ages. We are also told of general catastrophes and a succession of deluges, of the alternation of periods of repose and disorder, of the refrigeration of the globe, of the sudden annihilation of whole races of animals and plants, and other hypotheses, in which we see the ancient spirit of speculation revived, and a desire manifested to cut, rather than patiently to untie, the Gordian knot.
>
> In our attempt to unravel these difficult questions, we shall adopt a different course, restricting ourselves to the known or possible operations of existing causes; feeling assured that

> we have not yet exhausted the resources which the study of the present course of nature may provide, and therefore that we are not authorized, in the infancy of our science, to recur to extraordinary agents."[308]

For Charles Lyell, one of the main proponents of uniformitarianism, the empirical approach to earth sciences is the *conditio sine qua non* for establishing the foundation of a 'positive' approach. He sees himself as the 'philosophe' of geology, as the uniformitarian approach is for him the embodiment of progress in knowledge and social development, as it was described by Enlightenment scholars[309].

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[308] Lyell 1830-1833, vol. III, 7.

[309] "We have seen that, during the progress of geology, there have been great fluctuations of opinion respecting the nature of the causes to which all former changes of the earth's surface are referrible. The first observers conceived that the monuments which the geologist endeavours to decipher, relate to a period when the physical constitution of the earth differed entirely from the present, and that, even after the creation of living beings, there have been causes in action distinct in kind or degree from those now forming part of the economy of nature. These views have been gradually modified, and some of them entirely abandoned in proportion as observations have been multiplied, and the signs of former mutations more skillfully interpreted. Many appearances, which for a long time were regarded as indicating mysterious and extraordinary agency, are finally recognized as the necessary result of the laws now governing the material world; and the discovery of this unlooked for conformity has induced some geologists to infer that there has never

Arguing for continuity between past and present, uniformitarian views also allowed for evolutionary processes to be at the heart of changes in shape and function spotted in fossil ancestors of living species[310]. By contrast,

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been any interruption to the same uniform order of physical events. The same assemblage of general causes, they conceive, may have been sufficient to produce, by their various combinations, the endless diversity of effects, of which the shell of the earth has preserved the memorials, and, consistently with these principles, the recurrence of analogous changes is expected by them in time to come. Whether we coincide or not in this doctrine, we must admit that the gradual progress of opinion concerning the succession of phenomena in remote eras, resembles in a singular manner that which accompanies the growing intelligence of every people, in regard to the economy of nature in modern times. In an early stage of advancement, when a great number of natural appearances are unintelligible, an eclipse, an earthquake, a flood, or the approach of a comet, with many other occurrences afterwards found to belong to the regular course of events, are regarded as prodigies. The same delusion prevails as to moral phenomena, and many of these are ascribed to the intervention of demons, ghosts, witches, and other immaterial and supernatural agents. By degrees, many of the enigmas of the moral and physical world are explained, end, instead of being due to extrinsic and irregular causes, they are found to depend on fixed and invariable laws. The philosopher at last becomes convinced of the undeviating uniformity of secondary causes, and, guided by his faith in this principle, he determines the probability of accounts transmitted to him of former occurrences, and often rejects the fabulous tales of former ages, on the ground of their being irreconcilable with the experience of more enlightened ages." Lyell 1830-1833, vol.I, 72-73.

310 The Antiquity of Man and Evolution was one of the two 'conversions' of Lyell cf. Cohen 1998. In 1862 he also devoted an entire volume to this problem, backing Darwin's theory and Boucher de Perthes' theories on

catastrophism was based on the principle of the 'correlation of parts', wherein the mutation of a single aspect of an organism or of an ecosystem was not conceivable[311].
As for the dating methods, in the XIX century all geologists relied on three basic rules, which were established by earlier

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fossil men, which raised many critical comments, cf. Bynum 1984. However, in 1832 he had written: "Let us now consider what conclusions are deducible from the important facts above enumerated. Must we infer that man and these extinct quadrupeds were contemporaneous inhabitants of the south of France at some former epoch? We should unquestionably have arrived at this conclusion if the bones had been found in an undisturbed stratified deposit of subaqueous origin, especially if it contained shells in regular layers like that of North-Cliff in Yorkshire, described by Mr. Vernon, from which we learn that the mammoth coexisted with thirteen species of our living British land and fresh-water Testacea. But we must hesitate before we draw analogous inferences from evidences so equivocal as that afforded by the mud, stalagmites and breccias of caves, where the signs of successive deposition are wanting. No one will maintain that man, the hyæna, and the bear, were at once joint tenants of these caverns; and if it be necessary to assume that the mud and pebbles were washed into their present position by floods, the same inundations might possibly have caught up the bones lying in more ancient deposits, and thus have mingled the whole together in the same mass. More than ordinary caution is required in reasoning on the occurrence of human remains and works of art in alluvial deposits, since the chances of error are much greater than when we have the fossil bones of the inferior animals only under consideration. For the floor of caves has usually been disturbed by the aboriginal inhabitants of each country, who have used such retreats for dwelling places, or for concealment, or sepulture." Lyell 1830-1833 Vol. II, 225-226.

[311] *Vide infra* 221-244.

scholars – such as Arduino, Steno and William Smith[312]. These rules are the law of superposition, the mineral structure of strata and the organic remains contained in them. This is clearly stated by Lyell in his book:

> "We explained in the last chapter the principles on which the relative ages of different formations may be ascertained, and we found the character to be chiefly derivable from superposition, mineral structure, and organic remains. It is by combining the evidence deducible from all these sources, that we determine the chronological succession of distinct formations,"[313]

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312 In the first volume of the *Principles of Geology*, several chapters are devoted to a history of geology and the progress in its recent development: several pages were devoted to the Italian school - to Steno in particular (pp. 33-44) - and the work of William Smith was discussed on pp.101-102. The history of the discipline was very important to Lyell, as he also uses it to back up his actualistic theory: "We shall adhere to this plan (only considering existing causes, ndr), not only on the grounds explained in the first volume, but because, as we have above stated, history informs us that this method had always put geologists on the road that leads to truth,—suggesting views which, although imperfect at first, have been found capable of improvement, until at last adopted by universal consent." Lyell 1830-1833, Vol. III, 7. One example of the relevance of local stratigraphers to the development of geological and archaeological methods cf. Schnapp 1996, 198-204.

313 Lyell, 1830-1833, vol. III, 46.

For Lyell, however, the law of superposition – and of stratigraphic relationships in general (cf. Fig. 29) – is the only independent tool for verifying the relationship between species, and particularly between living species (including man) and extinct ones. As stratigraphy is an empirical and 'positive' method of relative chronology, he argues for the complete dismissal of 'mysterious and extraordinary agency'. Mysterious causes cannot be part of the scientific understanding of the order of species:

> "[…] in the present deficiency of historical records, we have traced up the subject to that point where geological monuments alone are capable of leading us on to the discovery of ulterior truths. To these, therefore, we must now appeal, carefully examining the strata of recent formation wherein the remains of living species, both animal and vegetable, are known to occur. We must study these strata in strict reference to their chronological order as deduced from their superposition, and other relations. From these sources we may learn which of the species, now our contemporaries, have survived the greatest revolutions of the earth's surface; which of them have co-existed with the greatest number of animals and plants now extinct, and which have

> made their appearance only when the animate world had nearly attained its present condition. From such data we may be enabled to infer whether species have been called into existence in succession or all at one period; whether singly, or whether by groups simultaneously; whether the antiquity of man be as high as that of any of the inferior beings which now share the planet with him, or whether the human species is one of the most recent of the whole."[314]

It should be noted that this statement implies the possibility of different species having different lifespans: the multi-temporality of the record is, indeed, one of the reasons why one can build a table of co-occurrence and extract chronological information from it. At the end of this chapter it will be clear that the same principle applies to Petrie's combinatorial system, which is applied to artefacts instead of fossils[315].

As for the other two instruments of geological chronology, Lyell is aware that the mineralogical composition of strata might at times be a misleading indicator for its relative age, though it is useful in combination with other data[316]. As for

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[314] Lyell 1830-1833, Vol. II, 183.

[315] On the use of statistics in Lyell's work cf. McCready and Schwertman 2001; on Petrie's methodological statements *vide infra* 280-285.

[316] Lyell 1830-1833, Vol. III, 344-347.

organic materials found in the matrix, while their reciprocal position is to be established based on stratigraphy, once enough evidence is collected and analysed, they may provide a useful indicator for the contemporaneity of unrelated contexts. Indeed, Lyell is aware of the possibility of synchronising distant layers through the analysis of fossils contained in them – at least inside the same *zoological province*[317]. This same principle will later be used for cross-dating in archaeology[318]. However, unlike his predecessors, Lyell did not condone the use of single index fossils as indicators for one period: he did specify that, to establish a connection between distant strata, a careful consideration of the specific combination of different fossils in the stratum is required [319].

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317 'Zoological provinces' are intended as areas where – due to certain environmental barriers and climatic homogeneity – one would expect a certain zoological and botanical uniformity at any given time (Lyell 1830-1833, vol. III, 48-72).

318 Gabriel De Mortillet (1883, 1-7) explicitly acknowledges the filiation of his highly influential prehistoric taxonomy from geological methods.

319 "Fortunately, the extent of the same zoological provinces, especially those of marine animals, is very great, so that we are entitled to expect, from analogy, that the identity of fossil species, throughout large areas, will often enable us to connect together a great variety of detached and dissimilar formations. […] Such identity of fossils, we may remark, not only enables us to refer to the same era, distinct rocks widely separated from each other in the horizontal plane, but also others which may be considerably distant in the vertical series. Thus, for example, we may find alternating beds of clay, sand, and lava, two thousand feet in thickness, the whole of which may be proved to belong to the same

This idea that one fossil is not enough to define a layer – as any variable is not enough to define a group – comes from a dynamic idea of natural morphology, where multiple natural causes exist, and it is their different combination that produces observable results. In Lyell's understanding, while taxonomy is a useful and necessary heuristic tool, monothetic taxa could only be 'true' if they rested on a single natural cause. By contrast, the laws of nature are numerous, and they interact in different ways:

> "In recent times, we may attribute our rapid progress chiefly to the careful determination of the order of succession in mineral masses, by means of their different organic contents, and their regular superposition. But the old diluvialists were induced by their system to confound all the groups of strata together instead of discriminating, to refer all appearances to one cause and to one brief period, not to a variety of

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epoch, by the specific identity of the fossil shells dispersed throughout the whole series. […], we must form our conclusions from a great number of species, since a single species may be enabled to survive vicissitudes in the earth's surface, whereby thousands of others are exterminated. When a change of climate takes place, some may migrate and become denizens of other latitudes, and so abound there, as to characterize strata of a subsequent era." Lyell 1830-1833, Vol. III, 41-45.

causes acting throughout a long succession of epochs."[320]

The notion of multiple causes can also be found in Montelius' typological work, where he applied the principle to human industries instead of biological organisms, in order to pose the problem of human agency[321]. On the other hand, many archaeologists and anthropologists embraced the idea that current – ethnographic – observations could be used to understand the past, and in some cases to infer general rules about human societies and the material traces they leave behind[322].

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320 Lyell 1830-1833, vol. I, 30.

321 *Vide infra* 271-280.

322 This tendency is represented by very well-known works of Lubbock (1865) and Morgan (1877): the former was also a naturalist and published more on entomology than he did on archaeology. These were also the years, in which the great collections of anthropological museums were formed, displaying artefacts collected during ethnographic explorations next to archaeological remains: for the Pitt Rivers Museum see at least Pitt Rivers 1891, Blackwood 1970, Chapman 1985, Hicks 2013; for the Musée d'Éthnographie du Trocadero cf. Dias 1991, Carminati 2011, DeGroff 2012; for the Museo Nazionale Preistorico Etnografico in Rome see at least Nobili 1990, La Rocca and Mangani 2014; for the Danish Ethnographic Museum and the role of Christian Jurgensen Thomsen in its organization cf. Jensen 1992 and Eskildsen 2012.

3.1.2 Archaeological excavations and the fossil man: Boucher de Perthes

The debate over the antiquity of man was the condition under which archaeologists adopted geological methods and adapted them to suit their research questions and extant body of knowledge. For example, the law of superposition was adopted to establish the diachronic relationship between adjacent deposits, while the concept of 'index fossil' was used to determine the synchronic relationship between distant deposits[323].

Among the many archaeologists who soon started adopting stratigraphy[324], J. Boucher de Perthes provided one of the first methodological claims on the value of artefacts' spatial position. He tirelessly studied Paleolithic remains in the

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[323] Groenen (1994, 97-105 and 129-138) provides an interesting account of the first applications of vertical stratigraphy and 'index fossils' in French archaeology, tracing the direct link between prehistory and geology, while somewhat underrepresenting the role of antiquarians (cf. *infra* 255-269).

[324] Groenen 1994, 129-138, provides a brief account of the first applications of stratigraphy to XIX century archaeology; O'Brian and Lyman 2002 chronicle the late adoption of stratigraphic methods in Americanist Archaeology; however, Thomas Jefferson – second president of the United States – adopted a form of stratigraphic principle already in 1782 (cf. Lehmann-Hartleben 1943); Harris 2014, 7-13, points out that early archaeologists acquired geological stratigraphy but failed to adapt the Law of Superposition to the conditions specific to archaeological excavations.

Somme valley[325]. In 1847, in his *Antiquités celtiques et antédiluviennes*, he stated:

> « Que tous ces objets confondus soient jetés pêle-mêle sur le sol, ils n'indiquent plus rien, et, comme ils n'ont de prix que par ce qu'ils enseignent, dès ce moment ils tombent dans les non-valeurs et les futilités. C'est donc leur position comparative qu'on doit étudier ; c'est la superposition des couches sur lesquelles ils reposent ; c'est enfin la cendre des morts qu'il faut analyser. »[326]

According to this statement, one of the most important information provided by artefacts is their *comparative position* and the *superimposition of layers* on which they lay: these data were used to establish the relative order of different settlement phases, as well as contemporaneity for items

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[325] The philosophical inclinations of Boucher de Perthes gave lesser weight to his claims on the Antiquity of Man, until the issue was publicly discussed – with the support of an abundant photographical documentation – at the newly established Société d'Anthropologie de Paris on 3 November 1859 (the discussion is published in the Bulletin de la Société Anthropologique de Paris 1960). An intellectual biography of Boucher de Perthes has been published by Cohen and Hublin in 2017; his role in the development of Palaeolithic archaeology has been recently discussed by Gowlett 2009 (with previous bibliography).

[326] Boucher de Perthes 1847, 163-164.

found in the same layer. This quote of Boucher de Perthes has been selected among several others because of the interesting intellectual position of its author. Indeed, when he first started arguing for the antiquity of man (1847), he adapted his claim to fit Cuvier's theory of successive revolutions: the very reason why he could argue for the antiquity of man was that he had found ancient artefacts in the *diluvium inférieur* which, according to Cuvier, was much older than the Biblical Flood[327]. Somewhat more than a decade later, when he wrote *De l'homme antédiluvien et de ses oeuvres* (1860), he had embraced Lyell's uniformitarian beliefs, stating that current causes generated past deposits and criticizing the indiscriminate use of supernatural and catastrophic events to explain change in the geo-archaeological record[328]. This change of opinion was in accordance with a philosophical reflection, on the continuity of the chain of beings and the unity of the human species, which had been part of his philosophical speculation since the 1830s[329]. The debate between uniformitarianism vs

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327 Pautrat 1989, 132-136, provides a very detailed account of the intellectual path of Boucher de Perthes, from Cuvierism to Uniformitarianism, while elaborating his own Natural Philosophy.

328 Pautrat 1989, 139-143.

329 Boucher de Perthes 1838-41; Aufrère 2007 and Pautrat 1989, 143-148, briefly analyse the relevance of Boucher de Perthes' reflections on metaphysics and spontaneous generation for his archaeological work. But his numerous volumes on these topics received little attention to this day.

catastrophism does not seem to have had a factual impact on his application of the principles of geological stratigraphy. But the relevance of Cuvierism in his initial and formative years, had an impact on his treatment of artefacts for chronological purposes.

In 1859 Boucher de Perthes' excavation in Acheul had two prominent visitors: John Evans, a renown English antiquary, and Joseph Prestwich, a geologist following Lyell's school. They photographed the trench where a *hache* was found and were of great help in supporting the evidentiary value given by the Society of Antiquaries and the Royal Society to such discoveries[330].

John Evans was a well-known numismatist, specialising in difficult or uninscribed medals and coins from England[331]. In his taxonomic and chronological efforts, he used *dynamic morphology* to classify different coins[332]. Following a principle that could be considered an *ante litteram* evolutionary typology, he aimed to establish the relative chronology of uninscribed coins: he thought that the repeated imitation of original models produced mistakes, thus generating change

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330 Cf. Schlanger 2010, in particular footnote 15. See also Tucker 2005 on the use of photography as an eyewitness in Victorian science.

331 Later in his career, he also authored one of the most influential publications on stone implements until Lubbock's work: the collective volume edited by Mac Gregor (2008) contains several crucial articles on his intellectual background and antiquarian methodology.

332 'Dynamic morphology' and 'static morphology' *sensu* Ginzburg 2019.

in the formal aspects of the coins in a progressive and branching fashion[333]. He published charts explaining his model, which looked like a phylogenetic tree of coins' iconography (Fig. 30).

Even though Boucher de Perthes got in contact with Evans and even after he adopted uniformitarian views, he kept adopting the *static morphology* acquired from the study of Cuvier's analytical anatomy. Examining the three volumes of his *Antiquités Celtiques et Antédiluviennes* – issued in 1847, in 1857 and in 1864 respectively – one can find that even in the last volume the author tries to divide at least some artefacts (the most advanced in terms of technological complexity) into 'fixed types' justifying their formal homogeneity through social norms:

> « Cette régularité indique qu'elles n'ont pas été faites au hasard, mais d'après un type reçu et des mesures données, haches qu'on pourrait nommer de calibre, car elles devaient servir à un même usage, à un même manche ou à un même instrument de projection » [334]

> « Beaucoup de ces haches de la couche brune ou noire ont entr'elles une certaine uniformité : on ne peut admettre que ce soit l'effet du hasard ; elle ne

[333] Cf. Evans 1850.

[334] Boucher de Perthes 1864, 420.

> s'obtenait que par un travail plus long, plus attentif. C'était donc un type donné, que la loi, l'usage, la religion prescrivaient, et dont il n'était pas permis de s'écarter »[335]

In those same years, Gabriel De Mortillet developed one of the most influential systems of artefact chronology for the European Palaeolithic (Fig. 31). After examining Lartet's paleontological subdivisions based on faunal remains, he decided to use industry for chronological divisions instead, thus following the antiquarian/archaeological tradition:

> « Laissant de côté la méthode paléonthologique, j'ai fait appel à la méthode archeologique. En effet, en archéologie n'est-ce pas toujours par les produits industriels qu'on détermine les époques ? L'époque étrusque, l'époque grecque, l'époque romaine, l'époque mérovingienne, le moyen âge, la renaissance, ne sont-ils pas bien caractérisés et sans contestation par leur produits divers ? »[336]

While doing so, he would however keep using the principles of geological taxonomy for issues of nomenclature and visual presentation of the archaeological periodization,

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335 Boucher de Perthes 1864, 211.
336 De Mortillet 1873, 435.

which he had established through artefacts[337]. Indeed, it seems apparent that antiquarianism, geology and paleontology contributed to the formation of archaeological methods in many more instances than it is usually acknowledged. In particular, the study of objects from archaeological deposits (or collections) was pursued through different methods which were developed during the XIX century: these methodologies, both the ones derived from natural sciences and the ones from antiquarianism, would implicitly contain a specific idea of morphology, either static or dynamic. And this would have a much greater impact on their work than the discipline they were inspired by. In this hybrid intellectual context – where discussions centered around transmutation and evolution (both biological and sociological) as well as around uniformitarianism vs catastrophism, and methods were drawn from geology, antiquarianism and ethnography – in this hybrid intellectual context took place the development of one of the main paradigms of western archaeology, the Three Age System.

[337] De Mortillet 1869, 7-8.

TEMPS.			AGES.	PÉRIODES.	ÉPOQUES.
Actuels.		Historiques.	du Fer.	Mérovingienne.	Wabennienne, Franque, Burgonde.
				Romaine.	Champdolienne, Décadence romaine.
					Lugdunienne, Beau-temps romain.
		Protohistoriques.		Galatienne, Etrusque.	Marnienne, Gauloise, 3^{e} Lacustre.
					Halstattienne, des Tumulus, 1^{re} du Fer.
			du Bronze.	Bohémienne.	Larnaudienne, 2^{e} Lacustre en majeure partie.
					Morgienne, 2^{e} Lacustre partie.
		Préhistoriques.	de la Pierre.	Néolithique, Pierre polie.	Robenhausienne, 1^{re} Lacustre, des Dolmens.
Géologiques.	Quaternaires.			Paléolithique, Pierre taillée.	Magdalénienne, des Cavernes en majeure partie, du Renne presque totalité.
					Solutréenne du Renne partie, du Mammouth partie.
					Moustérienne, du Grand Ours des cavernes.
					Chelléenne, Acheuléenne, du Mammouth partie, de l'Elephas antiquus.
	Tertiaires.			Eolithique.	Cournyenne.
					Thenaisienne.

Figure 31 - Gabriel de Mortillet, *Le Préhistorique. Antiquité de l'homme,* 1885 – Tabl. 1.

3.2 History of 'Heathen Times'

In 1836 Christian Jürgensen Thomsen published *Ledetraad til Nordisk Oldkundskab,* summarising his findings on Danish antiquities and giving the first account of what will be later known as the three-age system[338]. However, his ideas did not spread through prehistoric archaeology until eight years later, when the book was translated and included in a collective volume intended for an English-speaking audience, published by the Royal Society of Northern Antiquaries of Copenhagen. A chapter on written sources was added by N. M. Petersen, and two Fellows of the Society, Finn Magnusen and Charles C. Rafn, elaborated on some sections[339]. Most of the volume was devoted to the description of the 'objects of the heathen period' and to constructing the internal periodisation of 'heathen times'[340]. The following year, *The Primeval Antiquities of Denmark* was

[338] Thomsen 1936.

[339] *Guide to Northern Archaeology* 1948, xvi. Gräslund 1987, 1-4, discusses how the methods developed by Danish archaeologists reached foreign scholars and the delay in the publication of translations; the reception of the three age system in the UK is the topic of a monograph by Rowley-Conwy 2007; Morse 1999 explores the connection between craniology and the adoption of such chronological system. German scholars were the first to issue a translation of Thomsen's guide (already in 1837) but the book soon found an influential opponent in Ludwig Lindenschmit (cf. Street-Jensen 1985).

[340] *Ibidem,* 35-71.

issued: it was an English adaptation of Worsaae's *Danmarks Oldtid oplyst ved Oldsager og Gravhøie*, written in 1843 at the request of king Christian VIII[341]. In the English version, the three-age system was applied to British archaeology in an attempt to identify non-Roman antiquities and establish a temporal sequence for them. W. J. Thoms had selected relevant data from the writings of British antiquaries, arranging them in accordance with the proposed chronological framework[342]. These publications introduced the three-age system to the archaeological discourse, forcing many scholars to rearrange their findings. Most importantly, it instituted the idea that technology may be a driving force in the history of humanity. Objects acquired, in this sense, an unprecedented relevance in the study of history.

Dating the past through ancient artefacts had a long history in the antiquarian tradition, especially in the absence of a reliable historical chronology[343]. However, in the three-age

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341 Worsaae 1843, *Forord*.

342 Worsaae and Thoms 1849, iv-v.

343 The literature on antiquarianism is vast: Pomian 1992, Pucci 1993, Schnapp 2002, Levine 2003, Toscano 2009, Vine 2010, Peltz and Myrone 2018, all provide general accounts with extensive bibliography on more specific topics. Momigliano 1984 is particularly relevant to this topic, as he devotes most of his attention to the importance of chronology building to the legitimization of the antiquarian method, focusing on the role of ancient artefacts as evidence for arguing and then resolving the resurgence of 'historical Pyrrhonism' in the modern age: in particular, see Grell 2003 on Père Hardouin. The work of John Evans with British coins has already been mentioned, *vide supra* fig. 29. Some other iconic

system artefacts not only acquired an autonomous status, but also became the tangible manifestation of a teleological path of human history. Human time was divided into two main eras, 'heathen times' and 'Christian times'[344]. Tellingly, Thomsen did not provide any indication of absolute chronology when describing the three ages into which

figures should be mentioned. Francesco Bianchini (1662-1729), an astronomer from Verona, was interested in dating historical events through celestial motions. He was one of the first scholars to advocate the use of empirical evidence as a standalone tool of historical investigation, especially for chronological aspects (See the Preface of his unfinished *Istoria Universale*, pp. 1-4). He retained this principle also when he was entrusted with the re-arrangement of Clemente XI's Museo Ecclesiatico, in which he chose to preserve the artefacts that could provide information about ancient habits, as well as those which had some chronological relevance, such as the inscriptions with names of *consules* (see Hülsen 1890 for the 1706 and 1707 catalogues of the museum). Another event seems relevant: in 1722 Levesque de Pouilly wrote the article *Dissertation sur l'incertitude de l'histoire des quatre premieres siècles de Rome*, leading to a debate on the Archaic period in Roman history. According to him, it couldn't be the subject of historical analysis since the ancient sources do not treat it extensively. Among the many scholars who spoke on the matter, there was Nicolas Fréret (*Réflexion sur l'étude des anciens livres et sur le degré de certitude de leurs preuves*, 1724) and Abbé Anselme (*Des monuments qui ont supplé au deffaut de l'histoire et servi de mémoires aux premiers historiens,* 1724) ; they claimed that, in the absence of written sources, the history of Archaic Rome could and should be investigated by means of material evidence, which was perfectly suited to fill the gaps of historiography. Many more examples could be provided, but this would exceed the scope of this dissertation.

[344] Cf. the indexes of Worsaae 1849 and the *Guide to Northern Archaeology* 1848.

'heathen antiquities' were divided[345]. By contrast, in the following paragraph on the Christian period, he included absolute dates already in the first few lines[346]. The adoption of the three-age system did therefore not require a fix position in the debate on the antiquity of man, despite the debate's immense impact on intellectuals of every discipline, as well as on the public, between the 1820s and the 1870s[347]. Worsaae – while referring to uniformitarianism when accounting for geological changes – explicitly stated that there was no definite answer to the question of human antiquity.[348] This factor should be kept in mind when evaluating the public success of the three-age system. Moreover, this neutrality should be taken into account when considering the relationship of these Danish scholars with natural sciences, which were, with antiquarianism, their source of inspiration and methodology.

3.2.1 From coins to the Three-Age System

The absence of monumental classical remains in Scandinavia has often been considered one of the major incentives to

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[345] *Guide to Northern Archaeology,* 1948, 63-71.
[346] *Ibidem,* 71.
[347] *Vide supra,* 210-221.
[348] Worsaae 1849, 10. *Contra* Heizer 1962, 261, who thinks that the contradiction of biblical chronology was one of the necessary conditions for the establishment of the three-age system.

establishing an innovative school of archaeology there[349]. J. J. A. Worsaae, whose main publication has been mentioned above, was the first professor of archaeology at the University of Copenhagen[350]. He explicitly declared that the study of Danish national antiquity had been much facilitated by the fact that the region had never been part of the Roman Empire and did not preserve bulky monumental evidence of the Latin era, which would have inevitably attracted scholarly attentions, while making the distinction between Roman and pre-Roman antiquities more difficult[351]. Indeed, when arranging the Central Museum of Danish Antiquities in 1816, Christian Jurgensen Thomsen had to catalogue and display a huge quantity of artefacts for which no historical record was available.

In his *Guide*, when describing the 'periods of heathen antiquities', Thomsen explained the way he established the relative chronology of artefacts. It seems apparent that the combined use of stylistic analysis (likely taken from the antiquarian tradition and his experience as a numismatist) and stratigraphic observation (derived from geology) contributed to his chronological scheme:

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349 Among others, see Trigger 1989, 77.

350 Gräslund 1987, 15.

351 "It will evidently be serviceable to British antiquaries to look to the national antiquities of countries that were never conquered by the Romans, and whose national remains are therefore unmixed. In that respect the primeval antiquities of Denmark are peculiarly important." Worsaae 1849, iii-iv.

> "Towards determining the exact age of antiquities, or at least the period to which they belong, there is still another guide which hitherto has been but little followed with respect to the antiquities of the north, viz. an investigation of the forms of the objects and of the ornaments with which they were decorated, with a view that by a careful comparison and by accurately noting what sorts are generally found together, we may ascertain the order in which the successive changes took place, and thus determine the periods to which a mere inspection of the ornaments will authorize us to assign the object."[352] (emphasis was added for the sake of the following argument)

Through form and ornaments he defined groups of artefacts, which were to be considered analogues and could probably be understood to have the same function and age. Indeed, Thomsen had learned through his private numismatic collection that, in the absence of an inscription with the date and provenance of a coin, its formal aspects could be diagnostic[353]. His collection of coins and medals was so

[352] *Guide to Northern Archaeology* 1848, 69.

[353] Thomsen's vast coin collection was sold at auctions after his death. For the occasion, several catalogues were issued, which reworked

important to him, that he devoted a large part of his testament to the definition of the criteria for the publication of its catalogue[354], explicitly requiring the chronological order to be respected (Fig. 32).

Another crucial part of the method, however, is noting which types of artefacts 'are generally found together' and establishing relationships of contemporaneity among them[355].

Jens Jacob Asmussen Worsaae worked as an intern with Thomsen at the National Archaeological Museum of Denmark and he embraced the method of his mentor[356]: after having tested it through first-hand excavation experience, he formulated it explicitly and popularized it[357].

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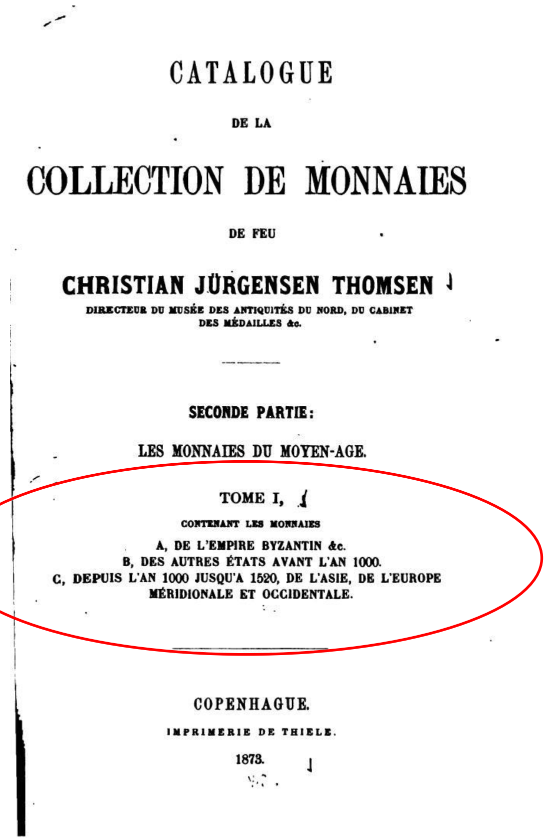
material from the inventories that he had kept during his life: *Catalogue de la collection de monnaies de feu* in 8 volumes (1866, 1867, 1869, 1871, 1873, 1874, 1876), each corresponding to an historical period.

354 Thomsen †, Erslev, Krohn, Brock, Laessøe 1873, Preface.

355 Most commentators emphasize the role of Worsaae in the attention given to context (e.g. Heizer 1962, 259; Trigger 1996, 131). Indeed, Thomsen did not conduct excavations to confirm his theory. But he was aware of the importance of context, and of registering which kinds of artefacts 'were usually found together' in establishing chronological relations (cf. *Guide to Northern Archaeology* 1948, 69).

356 Worsaae's life and career has been the subject of studies, even though most commentators focus on his nationalistic stances, e.g. Briggs 2005, Rowley-Conwy 2004 with the English translation of many relevant documents.

357 "It was not my plan to write a book merely for the archaeologist, but more particularly for the general reader. I endeavoured to prove the use and importance of archaeological researches, by showing how the


CATALOGUE

DE LA

COLLECTION DE MONNAIES

DE FEU

CHRISTIAN JÜRGENSEN THOMSEN

DIRECTEUR DU MUSÉE DES ANTIQUITÉS DU NORD, DU CABINET
DES MÉDAILLES &c.

SECONDE PARTIE:

LES MONNAIES DU MOYEN-AGE.

TOME I,

CONTENANT LES MONNAIES

A, DE L'EMPIRE BYZANTIN &c.
B, DES AUTRES ÉTATS AVANT L'AN 1000.
C, DEPUIS L'AN 1000 JUSQU'A 1520, DE L'ASIE, DE L'EUROPE
MÉRIDIONALE ET OCCIDENTALE.

COPENHAGUE.

IMPRIMERIE DE THIELE.

1873.

Figure 32 - Frontespiece of Thomsen †, Erslev, Krohn, Brock, Laessøe 1873 – Emphasis on the chronological division of sections.

When a group of artefacts was found in the same grave or repository, he assumed that they had been buried at the same time, so he established a contemporaneity criterion for

early history of our country can be read through the monuments, and I wanted in that way to excite a more general interest for the preservation of our national remains" Worsaae 1849, iv.

what he called *closed finds,* and which became widely known as Worsaae's Law[358]. Then, he divided and classified the artefacts based on their formal characteristics, identifying distinct *types*. From the connection between *types* and *closed finds,* he built a general chronological grid for Danish prehistory[359]. The passage that is usually quoted as the first explication of *closed find* is the following:

> "To establish as reliable and complete a picture as possible of the earliest settlement and most ancient circumstances of our native land, it is not sufficient to be concerned only with antiquities which have been removed from the earth. It is indispensable also to study and compare the sites where the antiquities are most commonly found, because otherwise many of the most important problems will not be solved at all or will be resolved in a very unsatisfactory manner. Thus, in the preceding pages we should scarcely have been able to refer the antiquities mentioned to three successive ages if experience had not taught us that antiquities which belong to different ages are also regularly found separately. Not all places

[358] Rowe 1962 and Thrane 2008 provide thorough accounts of Worsaae's Law, its reception and early uses.

[359] Cf. Worsaae 1949, where the same method is applied to British antiquities.

where discoveries are made will be considered here in the same manner, however. […] it is not the places where antiquities can be found accidentally which deserve to be the subject of a more detailed description for the purpose mentioned, but rather our ancient stone structures and burial mounds; for, with regard to the burials themselves, we know that they regularly contain not only the bones of the dead but also many of their weapons, implements and ornaments which were buried beside them in antiquity. Here, therefore, we can in general expect to find together those things which were originally used together at one time. […] One should not, of course, insist on concluding too much from a single burial mound taken by itself, but by combining many observations from all parts of the country we gradually find out what the burials have in common and what their peculiarities are, and we learn thereby to group the different kinds of burials into distinct classes and to assign them to some extent to different times. The importance of this procedure is far reaching"[360].

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360 Worsaae 1943, 60-61, as translated in Rowe 1962 with the aid of the German version. The same passage, in a shorter version is quoted by Thrane (2008, 52) and Gräslund 1987.

It has been argued that the chronological sequence elaborated by Thomsen and strengthened by Worsaae was not just a result of empirical observations. According to most commentators, since the beginning of his cataloguing endeavour, Thomsen wanted to use a chronological criterion for the display of artefacts[361], using them as a form of visual national history[362]. In fact, he was aware that artefacts were the only source of ancient Danish history beyond the northern sagas. Probably the hypothesis that "heathen antiquities" could be divided in three technological stages (Stone Age, Bronze Age, Iron Age) was derived from his learned readings and was only later verified by means of a formal taxonomy of the artefacts and of their co-occurrence in burials and repositories[363]. He might have derived this idea from Lucretius[364] – maybe through the mediation of

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361 Daniel 1943, 2-9.
362 Eskildsen 2012.
363 Heizer 1962 describes the intellectual conditions under which the three-age system was developed. Among other things, he traces the history of chronological subdivisions (usually in three metallic ages) from ancient authors (Lucretius and Hesiod) to modern scholars, such as Goguet (1716-1758), Vedel-Simonsen (1780-1858) and Büsching (1783-1829). Heizer argues that Thomsen was not the inventor of this sequence, but the first one who made it workable and testable through the archaeological excavations conducted by Worsaae. However, Heizer admits that he cannot prove which authors were part of Thomsen's learned background.
364 Lucr. V, 925-1160.

contemporary writers[365] – and he certainly supported it by means of thorough analyses of the references to stone, bronze and iron in ancient sources[366], mimicking the work of a true antiquary. However, in their publication, both Thomsen and Worsaae seem aware that not all the bronze artefacts could be placed in the Bronze Age, nor that all the stone artefacts could be placed in the Stone Age:

> "Bronze tools gradually supplanted the implements of stone, which however continued for a long time to be used by the poorer classes; and hunting and fishing gave way to agriculture, which was then commencing"[367].
>
> "It is quite true that tools and weapons of stone and bronze, and perhaps also of stone, bronze and iron have, as has already been remarked, been in use at the same time *in periods of transition,* when bronze or iron were scarce in the country, and consequently very expensive; yet it is nevertheless no less true that there were three distinct periods, in which the use of stone, bronze, and iron severally prevailed"[368].

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[365] Gräslund 1987.

[366] *Guide to Northern Archaeology* 1948.

[367] Worsaae 1849, 122.

[368] *Ibidem,* 124.

This attention to empirical data goes beyond the cataloguing efforts of antiquaries and shows remarkable awareness of the most recent debates in geology and in the philosophy of history[369]. This becomes particularly apparent when Worsaae argues to disprove the competing hypothesis that different materials are contemporary but belong to different social classes, using contextual information as a probative argument[370]. He explains that the stone tools are unlikely to have been an expression of the lower classes, as they are found in large Cromlechs and Giant's chambers, whose monumental structure is much richer than the barrows bearing bronze artefacts. Moreover, iron artefacts are found in association with precious metals more often than bronze artefacts, therefore denying the possibility for the latter to be the expression of an élite[371]. Context and associations are therefore treated as evidence for wider historical considerations, validating the idea of industrial stages in the history of humanity.

The idea that underlies the three-age system is that of society's continuous technological progress: over time, mankind acquired the ability to manipulate materials which

[369] Eskildsen 2012 provides a thoughtful picture of his intellectual background, with particular interest in the interactions between philosophy of history, comparative anatomy and ethnography.
[370] Worsaae 1849, 125-127.
[371] *Ibidem,* 125.

were more and more complex to be crafted but also more and more effective and useful to human survival[372]. At times, Thomsen seems to combine the idea of technological stages with that of cultural evolutionism as it was presented by Enlightenment scholars[373]. Some quotes from the volume *Guide to Northern Archaeology* appear emblematic:

> "That the stone age is the earliest in which we find our regions to be inhabited by human beings, seems established beyond all doubt, as is also the fact that the people must have borne a resemblance to savages."[374]

> "The ornaments found on stone antiquities are very insignificant […] and are nothing more than rude outlines bearing a sort of similarity to the hieroglyphics of savage nations."[375]

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[372] Thomas 2012, 138-140; evolutionistic in nature, this idea was soon the subject of studies which investigated the correlation between tool construction and craniology cf. Morse 1999 and Rowley-Conwy 2007, 302-316.

[373] Readings from the works of the *philosophes* were usually part of the education of the nationalistic bourgeoisie in Denmark (cf. Gräslund 1987), which was in turn the social class of many northern archaeologists: Thomsen spent a formative part of his youth in Paris, as was also not uncommon for his peers a formative experience, as it was quite common for his peers (cf. Trigger 1989, 78-83).

[374] *Guide to Northern Archaeology* 1948, 64.

[375] *Guide to Northern Archaeology* 1948, 69.

The word '*savages*' was used to establish a comparison between ancient society and peoples of his own time that were deemed primitive. Notoriously, equating prehistoric societies with 'modern savages' had been a common practice at least since Rousseau's *Discours sur l'origine des inégalités parmi les hommes*[376]*.* Using ethnographic observations to interpret the archaeological record became common practice during the second half of the XIX century, and followed the widespread success of the works of sir John

[376] Even earlier, since the sixteenth century, comparisons were made between 'savage peoples' – especially native Americans – and ancient people. These analogies led to the realisation that stone tools did not result from thunderstorms, nor were they religious objects. Instead they were tools, and they comparable to the ones used by the indigenous people of north America and the tribal societies in Africa: among others Mercati (1541-1593), Jussieu (1686-1758), Mahudel (1673-1747), Goguet (1716-1758). This realization was one of the conditions that made possible the theorization of the three-age system (Heizer 1962, 259-261). Several studies have been conducted on the role of ethnographic observations of 'savages' in the development of nationalistic archaeology: e.g. McGuire 1992, Guidi 1996, Cobb 2005, McNiven and Russel 2007. In the philosophy of history, the equation between 'modern savages' and ancient peoples can already be found in Locke (1690) and Hobbes (1651), as well as in Montaigne (1588). As Lévi-Strauss (1962) has shown, however, the intellectual stance of Rousseau and the wide circulation of his writings gave a great boost to ethnological research. Conn 2004 provides an account of XIX century approaches to 'modern savages' and how they were placed within Eurocentric history. A thorough account of earlier studies is given by Trigger (1996, 563) in his bibliographical essay.

Lubbock and of Lewis H. Morgan.[377] Surprisingly, the latter has been often considered to be the main challenger, and the most authoritative alternative, to the three-age system[378]. Indeed, the two books of Thomsen and Worsaae introduced the three-age system in the archaeological discourse, arising a lively debate. The terms of this debate have been widely studied[379] and a lengthy review of the relative arguments goes beyond the scope of this dissertation. However, it should be mentioned that Lewis H. Morgan, a pioneer of Americanist archaeology, criticized the three-age system in his 1877 book *Ancient Society*, questioning its effectiveness as a dating system:

> "The terms 'Age of *Stone*', 'of *Bronze*' and 'of *Iron*', introduced by Danish archaeologists, have been extremely useful for certain purposes, and will remain so for the classification of objects of

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[377] Cf. Trigger (1996, 166-210) devotes an entire chapter to 'Evolutionary archaeology': particularly useful the bibliographical essay on the topic (*Ibiden*, 565-567)

[378] Cf. Heizer 1962, 259 considers the three-age system an alternative to the idea that history is articulated in three socioeconomic stages: savagery, barbarism and civilization. However, a careful reading of Thomsen and Worsaae's works would give a different impression: the technological/industrial stages of human progress appear to be the material counterpart to the same socioeconomic stages (cf. Worsaae 1849, 127-140; *Guide to Northern Archaeology* 1948, 61-69).

[379] The long debate that led to the acceptance of such theories in Victorian Britain is studied in detail by Rowley-Conwy 2007 and Morse 1999.

> ancient art; but the progress of knowledge has rendered other and different subdivisions necessary. Stone implements were not entirely laid aside with the introduction of tools of iron, nor of those of bronze. The invention of the process of smelting iron created an ethnical epoch, yet we could scarcely date another from the production of bronze. Moreover, since the period of stone implements overlaps those of bronze and of iron, and since that of bronze also overlaps that of iron, they are not capable of a circumscription that would leave each independent and distinct."[380] (*italics* in the original)

Morgan was the main proponent of the ethnical approach – where the stages of human development did not depend on the ability to employ a certain material, but on societal organisation and subsistence strategy[381] – which was for a

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380 Morgan 1877, 8-9.

381 van der Grijp (1997) sees an interesting connection between Morgan and his contemporaries, especially those who 'ethnologised' the Greco-Roman past, such as Bachofen and Foustel de Coulanges. Fortes (2006) devoted a monograph to the work and the intellectual biography of Lewis H. Morgan, focusing on his legacy; Moses (2009) centers his research on the ideas of progress that can be found in Morgan's works. Trautman and Kabelac (1994) contribute greatly to his intellectual biography, reconstructing the content of Morgan's library from the inventories he kept since his marriage.

certain time considered to be the main alternative to the Danish account of prehistory. It should be noted that he criticized the three-age system as he did not deem it adequate for establishing chronological sequences. More specifically, this inadequacy depended on the fact that the three-age system did not allow for 'each [period to be] independent and distinct'. In other words, Morgan required a *static morphology* to accommodate his taxonomy of 'developmental stages' where technology, kinship and social organisations were as tightly connected as the footprint of an animal was dependent on its digestive apparatus in Cuvier's anatomic theory. The three-age system provided, instead, a *dynamic morphology*.

3.3 The order of artefacts

Archaeology, palaeontology and ethnology became museum sciences during the XIX century: collections were organised and displayed in dedicated buildings, which soon opened to the public[382].

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[382] The literature on the topic is enormous. Here it will suffice to indicate some useful readings with no claim of being exhaustive: Bennett 1995, the collective volume edited by McDonald (1998), Yanni 1999. Taquet 2007 argues that Cuvier's arrangement of ecofacts in his Cabinet of comparative anatomy established the norm for museum practice applied during most of the XIX century. Jensen 1992 and Eskildsen 2012 describe Thomsen's approach to museum displays and its connection to Cuvier's

As Eskildsen highlighted, this is probably one of the reasons underpinning the methodological affinities among these disciplines, as they have been described so far[383]. Indeed, the *Exposition Universelle* that took place in Paris in 1899 contained several examples of this 'musealisation' of disciplines: from the 'Negro village' where 400 people and their every-day life were put on display, to prehistoric artefacts from French caves, to M. O. Durand-Savoyat's fossils collections[384]. Classification – mostly of objects – was the main task of scholars in these disciplines. Through taxonomy they were able to select some specimens, deemed representative of their taxon, which were put on display.

Some methods of archaeology, particularly those concerned with chronology building, were affected by this 'museographic' approach to ancient remains: artefacts were the main focus and identifying their relative chronological order the main goal.

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approach, but they do not make the distinction between the two different brands of morphology adopted by those two scholars.

383 Eskildsen 2012.

384 Monod 1890 reports the 'attractions' of the exhibition of 1889. For a diachronic perspective see Daniels 2013. It contains a list of the exhibitions held between 1798 and 1900, as they were recorded in documents kept at the British Library.

3.3.1 Oscar Montelius and evolutionary typology

Archaeology handbooks define evolutionary typology as a relative dating method, which allows sorting artefacts in chronological series based on their intrinsic characteristics[385]. Items of similar shape and function are included in the same category and each category is subsequently called a *type.* In each series, artefacts present variations in shape, ornament and/or material. Those variations are used to arrange artefacts into a chronological sequence, to build *series* of remains organized in terms of anteriority/posteriority. To establish the position of artefacts in the series, two criteria are used: similar characteristics point to chronological proximity; artefacts were affected by gradual changes in the direction of a progressively higher technological development. The process of dividing artefacts into *types* and sort them out in chronological *series* is called *typological seriation*[386]. The method used by Thomsen to arrange archaeological remains at the Danish National Museum based on the three-age system followed the same principle. However, the Danish scholar never formalised his approach into a methodology[387]. Even if Hans Hildebrand – the son of

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[385] It is usually specified that evolutionary typology should be tested against stratigraphic evidence and it is from the combination of the two that most of our typochronological tables are obtained, cf. Renfrew and Bahn 1995, 112-115.

[386] Renfrew and Bahn 1995, 112-113.

[387] Vide infra 277.

Bror Emil Hildebrand, who was educated by Thomsen at the Danish National Museum – was probably the first scholar to use the typological method[388], the person that is usually believed to be the founding father of typological seriation is Oscar Montelius (cf. Fig. 33). Indeed, not only did he apply the method extensively, but he also published many theoretical papers that came to define the approach and the methodology as valid means to build relative chronologies[389]. After collecting, cataloguing and dating artefacts all around Europe, he built chronological grids for many European regions and established relationships among them by means of cross-dating. In doing so, he built the first chronologic grid of European history which did not use the Bible as a source[390].

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388 Hildebrand 1871 is an attempt at a typological definition of Bronze Age *fibulae*: the methodology used in this work was later criticized by Sophus Müller (cf. Gräslund 1987, 62-63); as Schnapp (1996, 188-198) points out, some steps in that direction had already been made by antiquaries such as John Aubrey. The work of John Evans on coins and stone tools appears to be another noteworthy antecedent (see the collective volume edited by MacGregor in 2008). While many more examples could be found, it was not until Montelius that typology was established as a methodology.

389 Montelius 1880-1882, Montelius 1899, Montelius 1903.

390 Montelius 1885.

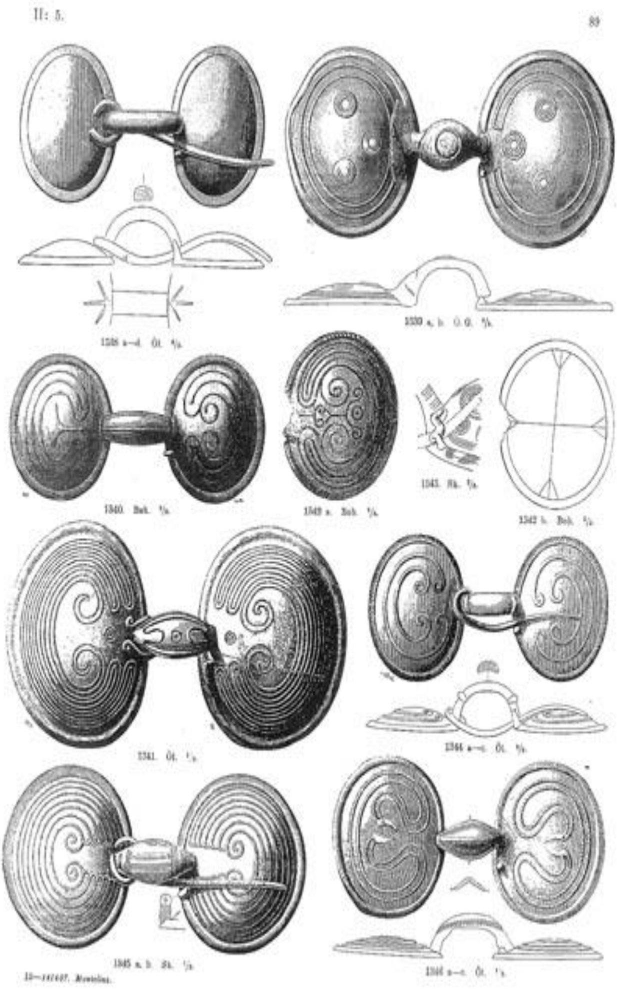

Figure 33 - Oscar Montelius, *Minnen från vår forntid*, 1917 – Tav II: 5.

In his methodological publications he claimed to sort artefacts on the basis of pure evolutionary typology and, only later, to check the results against the tables of co-occurrences in *closed finds*.

> "The archaeologist no longer regards it as his only task to describe and compare the antiquities from different countries and to investigate life in these countries in bygone times. He nowadays tries to trace the internal connection between the types and to show how one developed from the other. We call this *typology.*
> In principle, the typological investigation is very simple. In studying a group of antiquities, one first collects as large a material as possible, arranges it in the way which the internal criteria of the separate types seem to require and then investigates whether the circumstances under which the separate types were found confirm the correctness of the view of the mutual ages of the types which one has adopted."[391]

However, even if he claimed the priority of evolutionary typology, in his daily work Montelius largely used contextual data from the excavations where artefacts were found. This discrepancy has been discussed at lenght by Gräslund[392], who directly quotes from Montelius' work to

[391] Montelius 1884, 1-2 (translation Gräslund 1987).
[392] Gräslund 1987, 86-90.

illustrate the dynamics of his use of typology. In some of his less theoretical works, the Montelius declared that he firstly organized the items on the basis of their context and then he compared the sequence against the typological series. The following quote is taken from O. Montelius, *Om tidsbestämning inom bronsåldern med sårskildt afseende på Skandinavien,* 1885:

> "The results with respect to the ages of the separate types, to which the study of the contents of the graves has led, are confirmed by an investigation of a quite different kind, namely of the different types' relationships to and development from each other."[393]

Moreover, when the Danish archaeologist Sophus Müller (another of Thomsen's students)[394] accused him of not following the evolutionary method he preached, Montelius admitted that he used typology and context in parallel but, in order to allow the reader to easily understand the process, decided to address the two methods sequentially in publications:

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393 Montelius 1885, 8 (trans. Gräslund 1987).

394 Kristiansen 2012, 205-207; Eskildsen (2012, 52) discusses Müller's work for the 1889 *Exposition Universelle*.

> "The method which the Swedish typologists use consists not only in investigating all the known archaeological artefacts and their find circumstances but also in trying to gain all the information which can be obtained by a study of the internal connections of the types. As I have shown in the preceding pages, these two investigations should always proceed in parallel. In the printed account, however, one has, in order to secure order and lucidity, to describe first one side of the matter, and the other."[395]

On the other hand, it is undeniable that, in his works, many statements relating to his method have been inspired by evolutionism and preach a 'pure' use of typological seriation. Knowing that they do not reflect Montelius' practical approach, we should assume that they mirror the way in which he wanted his work to be considered. Thus, we need to investigate why he wanted to depict himself as a committed evolutionist. Or, to say it in Müller's words,

> "why pretend that the results have emerged from a complete and independent comparison of forms, which has only afterwards been tested on the number of observations?"[396]

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[395] Montelius 1884, 25.

[396] Müller 1884, 175 f.

It is possible that some personal and social elements influenced his statements. In 1873, Hans Hildebrand publicly wished for the emergence of 'a Darwin of comparative archaeology'[397]: he was probably referring to Montelius, who had just published a monumental volume on Swedish antiquities. However, Montelius insisted so heavily on a purely evolutionary typological method that it is fair to assume that he had deeper reasons than just intending to fulfill Hildebrand's prophecy. In 1899, he wrote a paper whose title would translate to *Typology or The Theory of Evolution Applied to Human Labour*. In it he stated explicitly that he wanted to equate his own method to that used by biologists and natural scientists: professing the positivistic stance of his method[398], he wanted to confer scientific dignity to archaeology.

> "That I wish to speak at a conference of natural scientists about the typological method is not, however, due so much to the great importance of this method to the archaeologist as to the

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[397] H. Hildebrand 1873, 17.

[398] It should be noted that Positivism in the Nordic regions is characterized by Scandinavian Realism, a movement which draws heavily from the social sciences. This school of thought preached that Natural Law – in the metaphysical sense – did not exist, as only empirical data exist (cf. Carty 2003).

> possibility that it may be of interest to the natural scientist to see, on the one hand, how we use, generally speaking, the same method as he does – in that we collect as large a material as possible and arrange it so that the results are immediately obvious – and, on the other, how we stand, in respect to the theory of evolution, on a purely Darwinian ground. That, as regards the production of nature, it is possible to follow the evolution of one form or one species from the other has, of course, as we are all aware, long been known. But it is only recently that we have discovered […] that a quite similar development can actually be shown as regards that productions of human labour. This should interest the natural scientist so much the more as man is, of course, in himself, regarded as a production of nature, also an object of his studies."[399]

In the same work, Montelius admitted that he had long been reluctant to apply the theory of evolution to the products of human labour, because this would have implied inexorable determinism, thus denying human freedom and agency.

[399] Montelius 1899, 267-268 (transl. Gräslund 1987).

However, he stated that he finally had to give up when faced with empirical evidence:

> "It is in actual fact rather amazing that Man in his labours has been and is subject to the very same laws of evolution. Is human freedom indeed so limited as to deny him the creation of any desired form? Are we forced to go, step by step, from one form to the next, be they ever so similar? Prior to studying these circumstances in depth, one can be tempted to answer such question with 'no'. However, since one has investigated human labours rather more closely, one finds that clearly, the answer has to be 'yes'." [400]

It appears that, even though Montelius did not behave like a pure evolutionist, he strongly wanted to be considered as such. He often professed that *types* were to him what species were to natural scientists[401]. Indeed, his work aims to translate biological, dynamic morphology into archaeology.

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400 *Ibidem*, 268.

401 This statement appears more than once in Montelius' works (e.g. 1884, 1; 1899, 237).

4.3.1 Flinders Petrie and the combinatorial method

In the previous paragraphs, it is repeatedly stressed that context has often been used as a frame for the seriation of artefacts: Thomsen used his knowledge of *closed finds* to sort out the items of the National Museum in his three-age system[402]; Montelius worked using typological seriation and contextual information in parallel. However, it has been highlighted that the value of context was not explicitly recognized by Montelius, who preferred to stress the importance of the evolutionary approach[403]. Indeed, he was heavily criticized for that, not only by Müller, but also by Otto Tischler, a German scholar who built the first chronological system for the La Tène culture[404]. He applied his idea that sound chronological sequences of artefacts could only be built through a comparative analysis of grave goods from a vast necropolis, examining the recurrence and co-occurrence of different types[405]. This approach was eventually formalized into a method and popularised by Flinders Petrie. He was a brilliant Victorian Age intellectual from a bourgeois background, and he is considered one of the founding fathers of Egyptian and Levantine

[402] *Vide supra*, 263-265.
[403] *Vide supra*, 271-279.
[404] Tischler 1885. On his attitude towards Montelius' typological method see Guidi 1988, 59 (footnote 14).
[405] Tischler 1885. See also De Marinis 2005, 16.

archaeology[406]. As a kid, he showed an exceptional vocation for mathematics. In his youth, he applied this ability to archaeology, trying to produce geometrical measures of Stonhenge and Giza[407]. This inclination towards the mathematical reading of archaeological data was crucial during his excavations in Egypt from the 1880s onwards (in the big necropolis of Naqada and Diospolis Parva). His approach to archaeological excavations, as well as his care in analysing artefacts – recording the context, studying all the material, without applying aesthetic or value-based selection – was very unusual for Mediterranean archaeology at the time[408]. This is therefore no coincidence that Petrie was educated in the practical aspects of archaeological excavations by his friend Flaxman C.J. Spurrel, who was an expert of Palaeolithic cultures[409]. Thanks to his unusual background and exploiting the privileges afforded by the British invasion of Egypt (which provided him with a large number of underpaid workers)[410], Petrie was able to uncover an impressive amount of burials and analyse the grave goods through the combinatorial method (Fig. 34).

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[406] On the role of the Victorian Age social environment on the education of Flinders Petrie and on his ideas about archaeology see Stevenson 2012.

[407] Gertzen, Grötschel 2012, 202-203.

[408] For the initial reluctance of classical and Mediterranean archaeology to use scientific and mathematical methods see Altekamp 2004.

[409] Stevenson 2012, 5-7.

[410] For the relations between Petrie, colonialism and nationalistic ideology see Sheppard 2010, and the statements of Petrie himself in Chapter III of *Methods and Aims in Archaeology*, 1904.

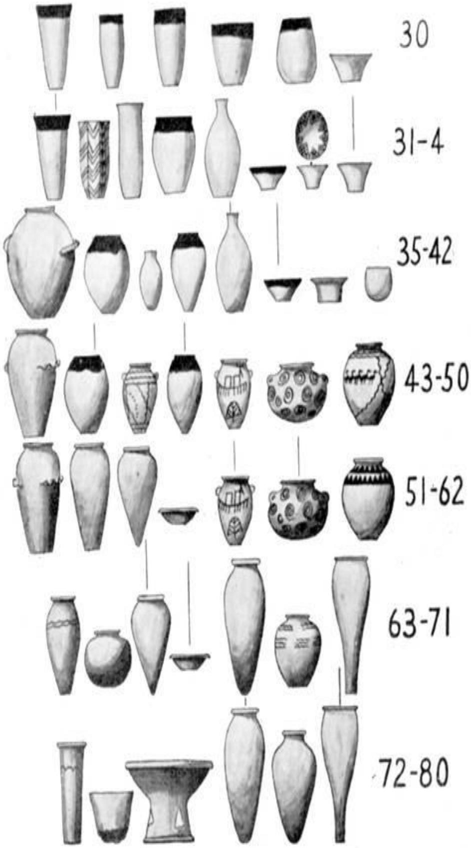

Figure 34 - Flinders Petrie, Sequences in Prehistoric Remains, in Journal of the Anthropological Institute, XXIX (1899), 295-301 – Abb. 1.

In 1901, he published the results of his excavations at Diospolis Parva: in this book, the combinatorial method is not only explained, but also rigorously applied[411].

[411] Petrie 1901, 2-13.

Firstly, he divided pottery findings into nine *classes*, each *class* was further divided into several *types*, and the resulting taxonomy was recorded in alpha numeric code.
For each grave the absence/presence of artefacts pertaining to certain *types* or *classes* was assessed and registered on a wooden stick divided into nine boxes. The sticks were then placed in a sequence on a wooden rail (Fig. 35). He tried different combinations until they were sorted in a series in which similar or identical items were as close as possible[412]. Special emphasis was given to artefacts whose typology suggested they were the most ancient or the most recent of their type:

> "This rough placing can be further improved by bringing together as close as may be the earliest and the latest examples of any type; as it is clear that any disturbance of the original order will tend to scatter the types wider, therefore the shortest range possible for each type is the probable truth."[413]

Combinatorial statistics largely depends on the typological notion that the formal characteristics of artefacts are subjected to gradual change.

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[412] On the probabilistic basis for this method see Gertzen, Grötschel 2012.
[413] Petrie 1901, 5.

Figure 35 - One of Flinders Petrie's combinatorial slips – Petrie Museum, London.

From typology Petrie borrows the idea that similar and identical objects are to be considered contemporary, so that the graves' chronological order can be established by putting together similar artefacts in the combinatorial sequence. One might therefore think that this computational method conveys a similar concept of time as evolutionary typology, that is a unilinear, unidirectional and progressive notion of time. However, this would be a reductive interpretation of the method. As may be seen in the tables of Petrie's publications, one of the main assumptions of the combinatorial method is the acknowledgement that some vessels' types have longer livespans than others, and that they can be found in graves and with grave goods of different ages[414] (Fig. 34).
The combinatorial method applies a different temporality to different *classes* and *types* of vessels, while implying a unilinear idea of time inside the evolutionary series of each *type*. The idea that different objects can respond to different time sequences, mirrors a more fluid notion of historical time, approaching the notion of multi-temporality.

3.4 Conclusion

Chronology can be defined as the arrangement of facts (events, ecofacts, artefacts) in their reciprocal temporal relation. Archaeological chronology – or at least some of the

[414] Gräslund 1987, 8.

methods used to construct it – entails establishing an *order of things,* a morphology. This is an endeavour that has been shown to be dependent on intellectual and social circumstances[415], but also on the personal trajectory and influence of the men and, especially, of the works that elaborated and popularised the methods when they entered the academic discourse. Morphology implies the definition of taxa and the implicit or explicit adoption of a *dynamic* or *static* morphology. Once taxa have been defined, they need to be connected to time in a significant and predictable way (i.e. a model), and through observable variables. The importance of models and variables in the definition of dating methods will become very clear in the next chapter, which discusses the history of radiocarbon dating.

[415] i.e. the *episteme* of Foucault 1966.

Chapter 4

Radiocarbon dating in context

> "Serendipity, perseverance, contingency, sociology, politics, economics, and luck have played a role in the development of all archaeological dating techniques, so much so that the current analytical use of any dating technique must be considered, at least in part, a function of its development history"
>
> Nash and Dean 2000, 10

The use of radiocarbon measurement techniques for determining the absolute dating of ancient organic material was one of the most successful scientific achievements of the postwar period. As it is one of the most popular dating methods in archaeology, there is abundant literature that covers the birth of the technique. Many contributions have described the progressive acquisition of knowledge and technology that was necessary to the development of the method[416]. Some of these accounts were written by Willard

[416] Cf. for example McDougall 2009, 45-71; Taylor and Bar-Yosef 2014; Olsson 2009. Becker 1992, and Leavitt and Bannister 2009 focus on the connection between dendrochronology and radiocarbon dating in its

F. Libby himself, the physical chemist who developed radiocarbon dating with his collaborators at the University of Chicago[417]. Others have been authored by scholars who were involved in the early development and application of this method: they recall the various phases of testing and the academic stand of Libby, by means of biographical memoires[418]. Some contributions focus on the role of American archaeologists – and geologists – in the early years of the method[419], and often the histories of archaeological thought try to establish the impact of radiocarbon dating on subsequent archaeological research[420]. Some accounts report the major achievements and controversies linked to the application of radiocarbon dating to specific fields[421]. Such

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early years.

417 Libby 1967, 1980.

418 Among others: Arnold and Schuch 1992; Suess 1992; Olsson 2009; de Messières 2011.

419 Johnson 1955 and 1965; Libby 1980; Marlowe 1980 and 1999; Taylor 1987 and 2000; de Messières 2001.

420 Taylor 2000, 100-104; Trigger 1996 [1989], 328-330; Johnson 1965, 764; Guidi 1988, 116-125.

421 Several of these contributions can be found in the Proceedings of the conference 'Radiocarbon After Four Decades' (Taylor, Long, Kra 1992), in particular: Fedick and Taube 1992 on Mayan archaeology, Robertshaw 1992 on Sub-Saharan Africa, Henry 1992 on the Near East, Wendorf 1992 on Northern Africa. Evin 2008 focuses on the impact of the first radiocarbon dates before calibration in France; Kuzmin 2009, gives a summary of the most relevant applications of ^{14}C to 'Old World' Archaeology. Of particular interest is Delley 2015, who elaborates on the

contributions are extremely useful and provide fundamental knowledge on the subject[422]. However, there are some aspects of radiocarbon dating history that can benefit from further investigation. For example, some intellectual and social aspects of the history of radiocarbon dating may be of interest. Useful information could be derived from a contextual approach and, more specifically, from the analysis of the historical circumstances under which the method was first developed and applied: the historical context of the postwar period was characterized by political ideology and vivid notions (and fears) of science, particularly of nuclear physics[423]. Moreover, investigators usually focus on the impact of radiocarbon dating on archaeology; it should be noted that, vice-versa, archaeological attitudes and research questions may have affected the development of radiocarbon dating and its several 'revolutions'[424]. Furthermore, as a chemical-physical

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history of radiocarbon dating as seen from the perspective of Swiss Lake-Dwelling research.

422 Notably, Marlowe (1980 and 1999) provides quite accurate information: the author uses archival documents and private correspondence to study the response of Americanist archaeology to the invention of radiocarbon dating, to highlight the social and political factors operating in the academic world and impacting the development and use of the method.

423 *Vide infra* 309-315.

424 Piggott 1959a, and especially Milojčić 1957, questioned the veracity of some of the assumptions underpinning radiocarbon dating, playing an important role in highlighting the urgency of calibration, i.e. the so-

method, radiocarbon measurements must both employ a specific notion of scientific time and – as it is used for understanding historical phenomena – interact with diverse notions and formalizations of human time. This method, and most noticeably the way it was received and used in different contexts, mirrored to the attitude of postwar society towards the past. Histories of radiocarbon dating usually show how the incremental development of new technology, coupled with a progressive understanding of natural phenomena, led to a likewise incremental improvement of the method itself. While this narrative is generically accurate, historical and cultural factors mentioned played a role in the development and application of the method. Therefore, in the following paragraphs, an attempt will be made at complementing the history of radiocarbon dating with considerations on the historical, social and intellectual context, of which it was born and in which it was used, from its early years until its validation through the assignment of the Nobel Prize to Willard Libby in 1960.

The first paragraph will provide a brief account of the processes that led to the of the design of the technical

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called 'second radiocarbon revolution', cf. Renfrew 1973; Bronk Ramsey 2008, Bayliss 2009 and Wood 2015 on the many 'revolutions' of radiocarbon dating: conventionally four 'revolutions' (i.e. changes of practice that radically improved the method) have been identified: invention – calibration – AMS measurement – Bayesian modelling.

apparatus, the development of the physical concepts and the collection of samples, which were instrumental to the birth of the method.

Then, the historical context is analysed, highlighting how factors such as nuclear energy propaganda, a unified notion of time, and a widespread desire for a unifying 'World History' influenced the success and spread of the method.

Finally, the third paragraph provides some insights into the academic *milieux* where radiocarbon dates circulated before the so-called 'second revolution', analysing the response of academic circles, the kind of journals where the first dates were published, and the research questions which might have affected the development and early applications of the method.

4.1 Radiocarbon dating: the development of the method

Ervin Taylor, the most influential historian of radiocarbon dating, built a table summarising "the major technical concepts and discoveries instrumental in the process by which the ^{14}C technique was initially developed"[425] (Fig. 36).

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[425] Taylor 2000, 92, revised from Taylor 1978 and 1987.

Table 5.1. Radiocarbon Dating Method: Initial Developments[a]

Date	Researcher	Development
		Pre-World War II Period (1933–1940)
1933	Libby[b]	Screen-wall counter development (not for ^{14}C)
1934	Kurie[b]	Mode of ^{14}C production advanced (cloud chamber)
1939	Korff	Mechanism of formation of natural ^{14}C proposed (cosmic-ray neutron secondaries)
1940	Kamen and Ruben[b]	Slow neutron production of ^{14}C confirmed Long ^{14}C half-life assumed
		Manhattan Project Period (1940–1945)
1940–1945	Kamen	Investigations of ^{14}C half-life[c]
		Chicago Period (1945–1954)
1945–1950	Libby, Anderson, and Arnold	Difference between biomethane (contemporary ^{14}C) and petromethane (fossil ^{14}C) Adaption of screen-wall solid carbon counting technology Anticoincidence counting Contemporary specific activity as function of latitude and carbon reservoir Curve of Knowns Measurement of unknowns First date list [Chicago I] (1950) Subsequent date lists [Chicago I–V] (1951–1954)

a = Revised from Taylor (1978; 1987) with assistance of Gregory Marlowe (personal communication 1997).
b = At University of California Berkeley.
c = See Table 1 and footnote 2 in Engelkemeir et al. (1949) and footnotes 2 and 3 for Table 4 in Libby (1955:35).

Figure 36 - Table of the major technological and scientific achievements instrumental to the invention of radiocarbon dating (Taylor 2000, 92).

He divides into in three periods: the pre-World War II period, the Manhattan Project period, and the Chicago period. The development of counting devices and the investigation of ^{14}C production in the atmosphere, as well as its half-life, were necessary steps towards the invention of the method. However, it was only when Willard Frank Libby settled in Room 217 of Jones Hall at the University of Chicago, in October 1945, that research started being conducted with the intention of developing a dating method based on the idea that residual radiocarbon could provide information on the date of ancient organic artefacts[426].

4.1.1 The Chicago Laboratory

After the war, the physical chemist at Berkley and former member of the Manhattan project Willard Frank Libby joined many of his previous colleagues at the newly founded Institute for Nuclear Studies in Chicago[427]. There he worked with Ernest Anderson, his Ph.D. student, on the radioactive isotope of carbon (^{14}C), which was thought to be generated

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[426] Initially, Libby kept his aim secret because he was worried that it would have appeared unrealistic, and his research would have been defunded (Libby 1967); Marlowe (1999, 11, especially footnote 9) offers a different perspective on the initial secrecy of this project: according to him, Libby was afraid of being anticipated, as had happened before with his research on samarium. It was only in May 1947 that Ernest Anderson – Libby's Ph.D. student in Chicago – published a paper explicitly prospecting the possibility of using radiocarbon to determine the age of archaeological remains (Anderson et al. 1947a and 1947b).
[427] Libby 1980, 1019; Taylor and Bar-Yosef 2014, 152-154.

by the impact of cosmic rays on the atmosphere[428]: its existence in nature was not ascertained, but it had been artificially produced at Berkley in 1940 for the first time[429]. In May 1946 Anderson and Libby proved the existence of the isotope in nature by enriching six hundred liters of methane coming from Baltimore sewage plant[430]. During the same experiment, they concluded that its half-life amounted to 5,568 years[431], confirming Libby's hypothesis: due to this long lifecycle and to its presence in all organic material, radiocarbon could be a revolutionary dating tool for archaeology and geology[432]. Making the strong assumptions that the concentration of this element in the atmosphere was nearly constant over the radiocarbon timespan, that radioactive decay was the only process affecting the

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428 Korff and Danforth 1939, mentioned by Libby (1960, 593) in his Nobel lecture.

429 Ruben and Kamen 1941.

430 A.V. Grosse of Temple University had an apparatus that allowed concentrating heavy isotopes of carbon, which he used to concentrate ^{13}C (used as a medical tracer): he agreed to concentrate some biological methane to help Libby with his research (Libby 1960, 596). The results have been published in Anderson et al. 1947a.

431 Radiocarbon half-life has since been discussed and different determinations have been suggested (Mann et al. 1961, Watt et al. 1961; Olsson et al. 1962; Bella et al. 1968) but – in order to keep results comparable – radiocarbon age is always conventionally published with Libby's half-life cf. Stuiver and Polach 1977.

432 Anderson et al. 1947a was the first article to explicitly mention the possibility of using radiocarbon measurements for dating purposes.

concentration of radiocarbon in organic materials, and that it was homogeneously distributed throughout different reservoirs[433] – they concluded that it could be used as a 'natural clock':

> "Since the radiocarbon originates in the top layers of the atmosphere, thereby entering the life cycle and all living matter, and since the neutron intensity at sea level is negligible, we are led to the prediction that the intake of radiocarbon by living bodies will cease when they die, and that the period of time elapsed since death will be measurable by direct comparison of the specific activity of the specimen with that of living matter in general. In other words, if we can assume that the specifc activity of living matter has remained constant over the time interval being measured, a specimen 5000 years buried will have 5,3 counts per minute per gram of carbon rather than the

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[433] None of these assumptions are, strictly speaking, true. As a result, the last six decades of radiocarbon studies were concerned with finding corrections for them. This especially affected the concentration of radiocarbon in the atmosphere, which showed even greater variation over time: these variations are accounted for via calibration. The calibration curve was built by comparison with dendrochronology. Extending the time range covered by and improving the precision of the calibration curve is to this day one of the main objectives of research in radiocarbon laboratories (most recently IntCal 2013 calibration curve expanded to 50.000 BP): cf. Wood 2015.

> original 10.5. By invoking isotopic enrichment, it should be possible to measure samples as old as 40,000 years."[434]

The disintegration process, in turn, follows the law of radioactive decay and is therefore predictable. Consequently, ancient organic material may be dated trough an external independent method: the ratio of residual radiocarbon per gram of carbon may be converted into calendar years, giving the age of the sample. Its great application promise and the publication venue's (*Science*)[435] wide readership, resulted in extensive enthusiasm both in the scientific community and among the general public: on 30 May 1947, the scientific breakthrough was reported on the New York Times (Fig. 37).

[434] Anderson et al. 1947b, 936.

[435] Anderson et al. 1947a.

The New York Times

FRIDAY, MAY 30, 1947

PAGE 23 OF 38

FINDS CARBON 14 IN NATURE

Libby of Chicago Traces Origin of Substance to Atmosphere

Special to THE NEW YORK TIMES.

CHICAGO, May 29 — Scientists at the University of Chicago announced today that they have been able to isolate in nature a radioactive substance known as Carbon 14, heretofore believed to be the product only of cyclotrons and atomic furnaces.

Dr. Williard F. Libby, Professor of Chemistry at the university, headed the research effort which tracked down Carbon 14 in the sewer gas of a Baltimore, Md. disposal plant, it was disclosed. Through his achievement, the university said, Dr. Libby confirmed a prediction he made in 1946, that bombardment by cosmic rays should produce radioactive carbon in the vast sea of nitrogen above the earth's surface.

His discovery that radiocarbon has an atmospheric origin, the university observed, will provide a new yard-stick for measuring various earth periods and also prove helpful in studying the intensity of cosmic rays coming from outer spaces.

DESIGNATED FOR BENCH

FIRE AIDE MUST RETIRE

BOY, 9, KILLED BY TRUCK

CITY CLERK'S ROLE TOLD

Policeman Ends Six-Year Search for Witness In Effort to Clear Man Convicted of Murder

Figure 37 – New York Times. May 30, 1947.

In 1948, Jim Arnold[436] joined the team of scientists working on the project. He was a chemist, and his father played a key role in the acquisition of the first historical samples for testing the method: a box of Egyptian artefacts (or fragments thereof) from the Metropolitan Museum in New York[437]. There was, however, a major problem to be faced: the high cost and long duration of enrichment required large samples sizes unsuitable for archaeological artefacts[438]: "Obviously,

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436 More accurately, one should say 're-join', as Arnold had already worked with Libby in Chicago as a postdoctoral fellow for around three months in spring 1946 (Taylor 1987,153; Marlowe 1999, 11 with archival documents).

437 Libby 1980, 1017. Cf. Marlowe 1999, 10-13.

438 According to Taylor's calculations, when Libby presented his work to the 'Conference Supper' of the Viking Fund for Anthropological

radiocarbon dating would have been an impractical method of measuring archaeological ages if this phase of research had been unsuccessful"[439].

The available counters also recorded the noise produced by cosmic rays in the atmosphere, which was ten times louder than the measurement produced by unenriched samples: a new apparatus was needed. It took almost an entire year before Anderson built the "anticoincidence counter", a device made of one big counter surrounded by several smaller ones. The central device registered the counts coming from the sample and the atmosphere, while the others only counted the latter. The noise could then be subtracted from the final measurement of the central counter to obtain the number of counts coming from the sample[440].

The first archaeological samples were published in March 1949: a piece of acacia wood from the tomb of Djoser in Sakkara provided by the curator of the Egyptian Collection at the Metropolitan Museum, Ambrose Lansing; and a piece of cypress wood from the tomb of Sneferu at Meydum submitted by Froelich Rainey, of the University of

Research, his protocol and apparatus would require nearly 1kg of material to be dated (Taylor 2000, 96-97).

[439] Libby 1960, 597.

[440] Anderson 1949, 1953. For a detailed explanation of the process, complete with photographs of the original apparatus, cf. Taylor 1987, 156-165; a simplified account is given by McDougall 2009, 57-60.

Pennsylvania Museum[441]. The good agreement between the known age of these samples and the radiocarbon results were encouraging and the scholars decided to date more samples of known age to validate the theoretical radiocarbon curve: indeed, the selection of reliable samples of known age was of crucial importance to test the reliability of the method. To serve this purpose, between February and March 1948, the American Anthropological Association (AAA) and the Geological Society of America (GSA) had established the "Committee on Radioactive Carbon 14", made of three archaeologists and one geologist: Frederick Johnson (chairman), Donald Collier, Froelich Rainey, and Richard Foster Flint. The Committee was entrusted with the responsibility of providing and documenting suitable samples for measurement[442].

> "The first shock Dr. Arnold and I had was when our advisors informed us that history extended back only to 5,000 years. We had thought initially that we would be able to get samples all along the

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[441] And results were published on *Science* in March 1949 (Libby, Anderson and Arnold 1949).

[442] Johnson 1955; Libby 1980, 1017. Frederick Johnson specialised in Canadian archaeology and he was based in Andover (MacNeish 1996); Donald Collier specialised in Andean and Ecuadorian archaeology (Thompson 1996); Froelich Rainey focused on artic regions, especially Alaska (Bockstoce 1993); and Richard Foster Flint was a geologist specialised in the Quaternary period (Washburn 1976).

curve back to 30,000 years, put the points in, and then our work would be fiished. You read statements in books that such and such a society or archaeological site is 20,000 years old. We learned rather abruptly that these numbers, these ancient ages, are not known accurately."[443]

Consequently, the first samples of known age were well-dated artefacts from Egypt and Mesopotamia or they were dendrochronologically dated wooden chunks. In December 1949, a set of six measurements from samples of known age was published in *Science*, together with a figure that quickly became iconic, the Curve of Knowns (Fig. 38a): the theoretical curve derived from an adaptation of Rutheford's equation of radioactive decay was compared with the measurements of the six samples.

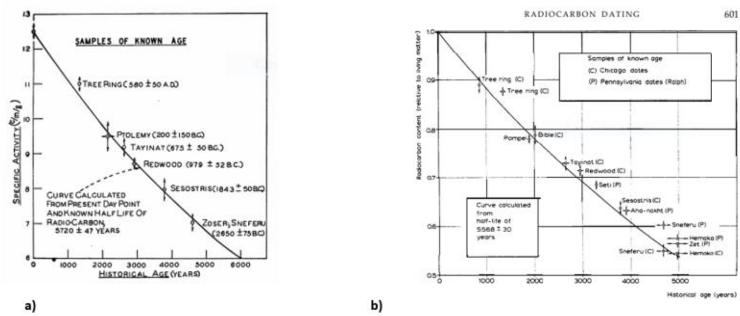

Figure 38 - Curve of Knowns: a) after Arnold and Libby 1949; b) after Libby's nobel lecture (1960).

443 Libby 1960, 600.

Five of the six average values were in agreement with the curve within one standard deviation, confirming the reliability of the radiocarbon method[444].

> "Following the test of the Curve of Knowns, the next step in the research was to test in the great periods of prehistory to see whether the dates obtained were reasonable."[445]

The Committee, then, became responsible for choosing relevant topics on which to test the method. They appointed a group of collaborators for collecting samples from different parts of the world. The samples' selection criteria were based on an array of chronological questions or areas[446]. Within one year, the samples collected by these scholars were dated, and produced the first radiocarbon date list, published by the University of Chicago Institute for Nuclear Studies in 1950[447]. The number of measured samples increased exponentially in the following years, as new laboratories were founded both in the U.S. and in Europe[448]: when the journal 'Radiocarbon' issued its first volume in

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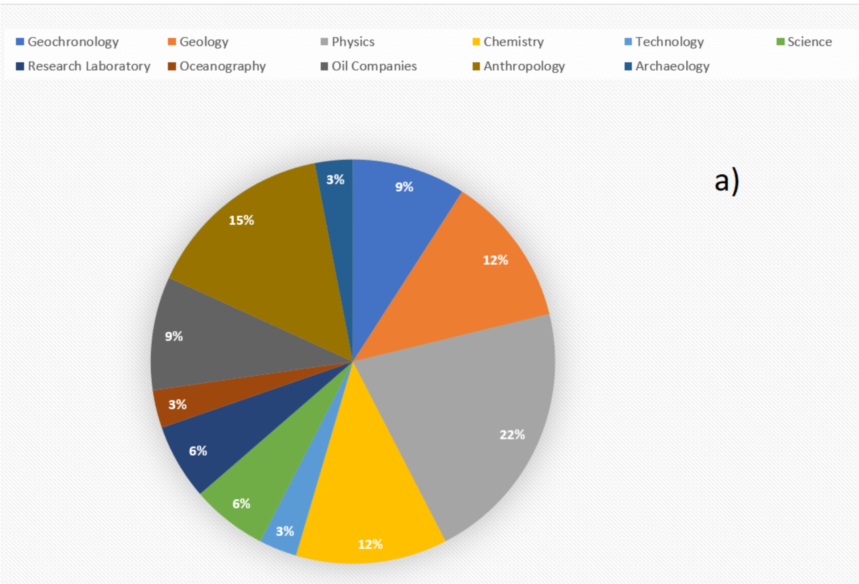
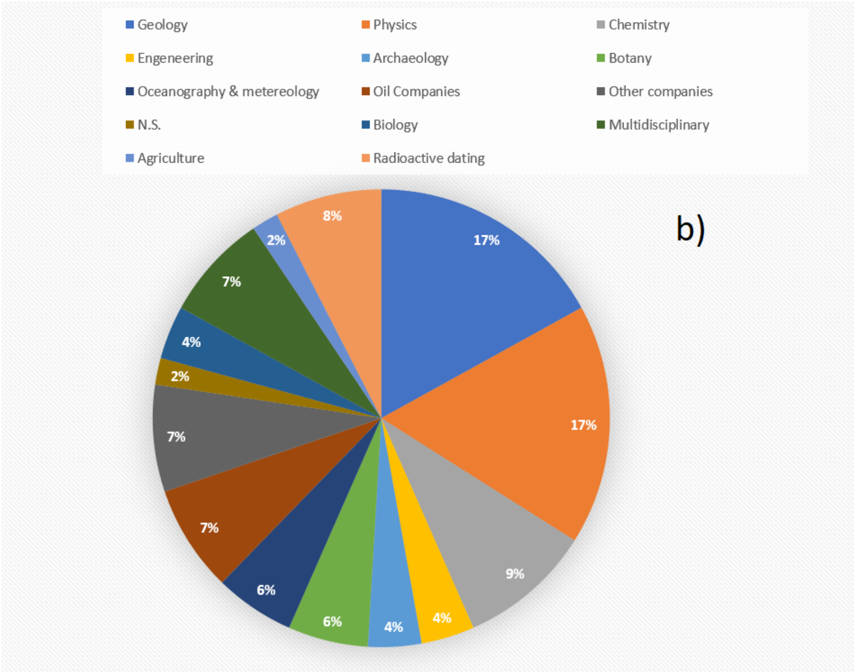
444 Arnold and Libby 1949, 679.
445 Libby 1960, 604.
446 Griffin 1949; it should be noted that 85 out of the 148 submitted samples came from the United States.
447 Arnold and Libby 1950. Jull et al. 2018 re-dated several of these early samples, obtaining very good agreement with Libby's results.
448 Anderson himself helped setting up the Copenhagen laboratory (Anderson et al. 1953); cf. also McDougall 2009, 65-71.

1959, thirty-six laboratories had already been established all around the world (Fig. 39c)[449]. And when Libby gave his Nobel lecture in 1960, the fourteen samples plotted on the Curve of Knowns (including the linen wrapping of a Dead Sea Scroll and one sample from Pompei) had been measured by three different laboratories (Fig. 38b): University of Chicago, University of Pennsylvania and Lamont Geological Observatory[450].



[449] *Radiocarbon* vol.1

[450] Libby 1960, 601.


Geology
Physics
Chemistry
Engeneering
Archaeology
Botany
Oceanography & metereology
Oil Companies
Other companies
N.S.
Biology
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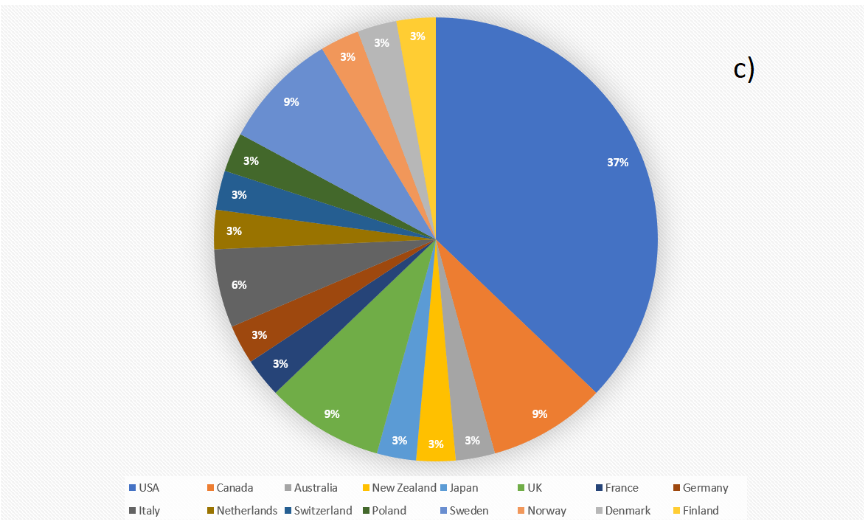
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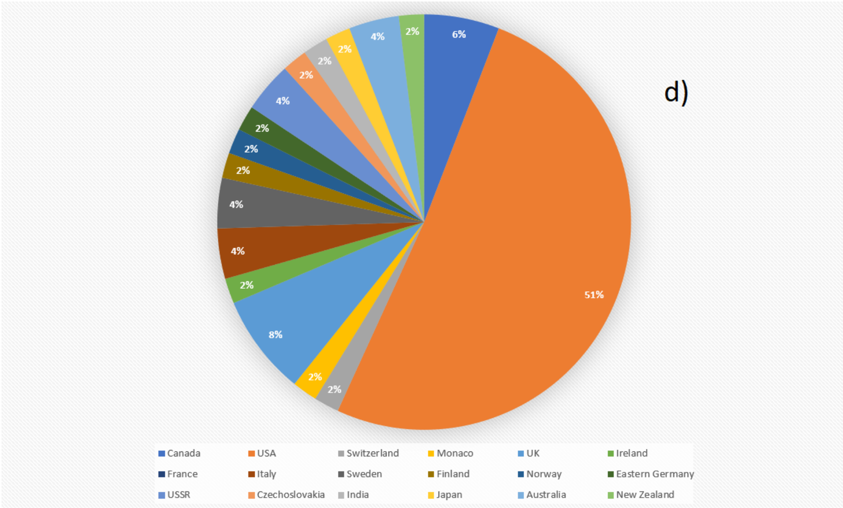

Figure 39 - Radiocarbon dating laboratories: a) divided by faculty, after Radiocarbon 1959; b) divided by faculty, after Radiocarbon 1965; c) divided by country, after Radiocarbon 1959; d) divided by country, after Radiocarbon 1965.

4.1.2 Archaeologists and the gathering of samples

Since November 1947, The Viking Fund for Anthropological Research substantially contributed to the funding of Libby's research, awarding him a 13,000 dollars grant[451]: at the beginning of the year, the Nobel laureate for chemistry Harold Urey, who had been Libby's superior on the Manhattan Project and was now his colleague at the Institute

[451] Libby correspondence, Samuel Allison to Paul Fejos (October 31, 1947); Libby 1960, 599; Taylor 1987, 154; Marlowe 1980, 1007.

for Nuclear Studies of the University of Chicago, saw the presentation on radiocarbon dating, delivered by Libby to an internal audience. Urey contacted a friend of his, Paul Fejos, who was the Director of Research at the Viking Fund and, by the end of that same year, a fruitful collaboration was formed[452]. The Viking Fund was established and endowed in 1941 by the extremely wealthy entrepreneur Axel Lennart Wenner-Gren and it supported scientific research in anthropology:

> "[…] the Board has construed the term anthropology in its broadest significance, realizing, that while certain trends manifestively dominate research at any given time, a policy too closely identified with a particular phase or trend would improperly represent the aims of the field at large. Thus, its program, within the limits of its resources, has sought an ever-widening scope for Fund support, in types of research and the areas of its distribution, in the development of theories and of techniques, and in meeting educational, and institutional needs for the propagation of the science."[453]

A rather well-known event is often reported as symptomatic of the initial reactions of archaeologists towards radiocarbon dating: the 'Supper Conference' held in New York by the

[452] Libby 1980, 1019.
[453] *The Viking Fund Inc.* 1947, 38.

Viking Fund in January 1948[454]. Libby presented the method and its potential to an audience of archaeologists and anthropologists, and at least one geologist, Richard Foster Flint. At the end of the speech, the audience did not seem enthusiastic, nor did archaeologists declare themselves willing to provide samples for testing the method. Only Flint would loudly assert his interest. According to Johnson, who was soon to become the chairman of the Committee for the selection of samples, the reactions of that night arose from the highly technical nature of the presentation given by Libby. It appeared that many archaeologists did not understand the potential impact of his research would have on archaeology[455]. By contrast, Taylor saw the main reason for their reluctance to collaborate in the very large sample sizes required (1-2 pounds of carbon), as at that time, anti-coincidence counting had not yet been developed.[456]

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[454] Frederick Johnson, who attended the conference, briefly mentions it in its 1965 article. The episode is also reported by Fejos in his biographical account (written by Dodds 1973). Marlowe (1980, 1008-1011) thoroughly investigated the event, collecting the personal correspondence and the interviews of those attending.

[455] Johnson 1965.

[456] Taylor 2000, 97 and footnote 12, mentioning his personal correspondence with Arnold and a taped interview where Libby expressed the same idea (April 12, 1979 – Center for the History of Physics, American Institute of Physics); indeed, the advancements in radiocarbon metrology have strongly impacted the applicability of the method to several fields and disciplines (cf. Currie 2004).

Because of the complex issues surrounding sample supply, a Committee was established to select suitable samples for validating the method[457]: they had to be samples of known age, to enable comparison of the radiocarbon date with an actual age (independent confirmation) and they had to cover the radiocarbon timescale as much as possible. Six 1-ounce wood samples (ca. 28 grams) were selected for dating. Two samples were provided by the Tree-Ring Dating Laboratory of the University of Arizona. One was found in 1931 in an Anasazi site, the Broken Flute Cave: dendrochronology established that the first ring grew in 530 A.D. and the tree was cut in 623 A.D. The other sample was a piece of redwood from a giant sequoia fell in the 19th century, known as the 'Centennial Stump': the measured sample was taken from the 1021-928 B.C. tree-rings, allowing exceptional precision in inter-methods comparison. Two more samples came from the Oriental Institute in Chicago: one was from a coffin that was dated to the Ptolemaic period (late 4th to 1st century B.C.) based on its stylistic attributes; the second (Tayinat) was made of two wooden pieces from the floor of a Syro-Hittite *hilani* dated between 725 and 625 B.C. based on the presence of Corinthian pottery. A fifth sample was taken from a funerary boat of Sesostris III displayed at the Chicago Natural History Museum: the chronology of Sesostris' reign was well known from the Egyptian calendar. Finally, the last

[457] Libby 1960, 600.

sample was taken from two different artefacts kept in two different museums, and deemed to be roughly contemporary: the wooden fragment from Djoser's tomb already provided by Ambrose Lansing in early 1948 and another piece of wood from the tomb of Sneferu from the University of Pennsylvania Museum in Philadelphia[458]. All these samples showed good agreement with expected results and with the theoretical radiocarbon decay curve, proving the reliability of the method[459]. Since its very inception, radiocarbon dating was compared with a variety of other dating methods, from dendrochronology, to typology and cross-dating (for the Tayinat sample), as well as historical accounts. The archaeology of Egypt and the Near-East in particular, was crucial to the initial development of the method.

The 148 dates published in the first date list of the University of Chicago explored the "great wilderness of prehistory"[460]: 62% of the 148 samples were of archaeological interest (the rest being mostly geological samples); 52% of the 92 archaeological samples came from the U.S. and reflected an attempt at building the chronology of Paleoindian settlements; only 11 samples from Egypt and the Near East

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458 The last two samples had already been published in March of the same year (Libby, Anderson, Arnold 1949).
459 The information on the samples can be found in Arnold and Libby 1949.
460 Libby 1960, 600.

were included and they were mostly used to check the results against the historical record[461].

4.2 Radiocarbon dating: some context

The previous subsection provided a history of the advancements in theoretical physics and technological equipment that allowed for the invention and testing of the method. Additionally, sample collection has also been introduced as a crucial step for method development[462]. However, these three factors – better knowledge of physical-chemical phenomena, betterment of technological devices, availability of samples – are not the only ones to have influenced the birth and early reception of the method. At least two other elements should be considered as part of the 'conditions of possibility' for the development and success of radiocarbon dating. First, it was a peaceful application of atomic research; second, it was the first brick in the construction of a unified World History.

[461] Arnold and Libby 1950, 3-4.

[462] *Vide supra* 304-308; cf. the articles collected in Nash 2000: collecting samples to calibrate the method has been a major challenge for many scientists who were developing other dating techniques.

4.2.1 Radioactivity is part of life: Libby and the Atomic Energy Commission

In 1959, the first issue of the Journal *Radiocarbon* listed 36 laboratories. Among these, the Saclay Nuclear Research Centre was the only one in France[463]. This was the main facility of the *Commissariat à l'énergie atomique et aux énergies alternatives (CEA):* built in 1947, the laboratory had been devoted to peacetime uses of nuclear energy until Frédéric Joliot was the high commissioner of the CEA[464]. His former student Jacques Labeyrie started building the first proportional counter after having read Anderson et al. 1947, which explained the potential of radiocarbon dating[465]; the first set of dates was published in 1964[466]. In 1958, the CEA began the operations instrumental to the construction of the atomic bomb.

In the case of the Laboratory of the University of Chicago, where radiocarbon dating was first experimented with, many of the people directly or indirectly involved in the development of the method had formerly been members of the Manhattan Project: after the war, many prominent scholars had migrated to the Institute for Nuclear Studies

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[463] *Radiocarbon* I, 217.
[464] Cf. Goldschmidt 1980.
[465] Labeyrie 1955.
[466] Delibrias, Guillier, Labeyrie 1964.

(INS) established in Chicago[467]. Among them were Harold Urey, Ernest Anderson and Willard Frank Libby[468].

Libby himself embodied the contradictions of the political scene of his time. He had been part of the Manhattan Project during the war[469] and he did not sign the petition against the offensive use of the nuclear bomb proposed by his colleagues[470] (Fig. 40). In his Nobel Lecture, he states that the United States Air Force has 'supported' his research 'generously'[471]. In 1954 he became a Commissioner for the Atomic Energy Commission (CAEC) (Fig. 41).

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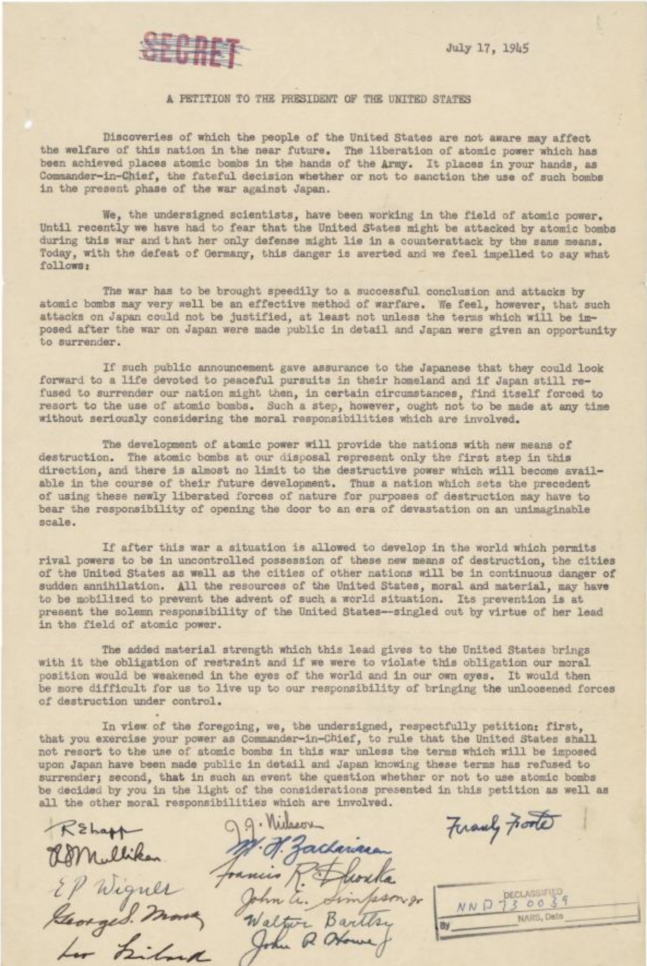
[467] Allison 1947 offers a detailed description of the INS and of its connections with the Metallurgical Project (which, during the war, was responsible for researching the uranium fission chain reaction used in the development of the bomb) and with the Atomic Energy Commission after the end of World War II.

[468] Urey was Director of War Research from 1940 to 1945 (cf. Harold C. Urey – Biographical, 1966); for Ernest Anderson cf. the extended biographical note published in *Radiocarbon* after his death (Taylor 2014). For Libby see the Nobel biographical note.

[469] Willard F. Libby – Biographical, 1964.

[470] U.S. National Archives, Record Group 77, Records of the Chief of Engineers, Manhattan Engineer District, Harrison-Bundy File, folder #76: Szilard and 69 co-signers, employed in the Manhattan Project, sent a petition to the president of the United States asking not to deploy the atomic bomb in Japan.

[471] Libby 1960, 599.



SECRET

July 17, 1945

A PETITION TO THE PRESIDENT OF THE UNITED STATES

Discoveries of which the people of the United States are not aware may affect the welfare of this nation in the near future. The liberation of atomic power which has been achieved places atomic bombs in the hands of the Army. It places in your hands, as Commander-in-Chief, the fateful decision whether or not to sanction the use of such bombs in the present phase of the war against Japan.

We, the undersigned scientists, have been working in the field of atomic power. Until recently we have had to fear that the United States might be attacked by atomic bombs during this war and that her only defense might lie in a counterattack by the same means. Today, with the defeat of Germany, this danger is averted and we feel impelled to say what follows:

The war has to be brought speedily to a successful conclusion and attacks by atomic bombs may very well be an effective method of warfare. We feel, however, that such attacks on Japan could not be justified, at least not unless the terms which will be imposed after the war on Japan were made public in detail and Japan were given an opportunity to surrender.

If such public announcement gave assurance to the Japanese that they could look forward to a life devoted to peaceful pursuits in their homeland and if Japan still refused to surrender our nation might then, in certain circumstances, find itself forced to resort to the use of atomic bombs. Such a step, however, ought not to be made at any time without seriously considering the moral responsibilities which are involved.

The development of atomic power will provide the nations with new means of destruction. The atomic bombs at our disposal represent only the first step in this direction, and there is almost no limit to the destructive power which will become available in the course of their future development. Thus a nation which sets the precedent of using these newly liberated forces of nature for purposes of destruction may have to bear the responsibility of opening the door to an era of devastation on an unimaginable scale.

If after this war a situation is allowed to develop in the world which permits rival powers to be in uncontrolled possession of these new means of destruction, the cities of the United States as well as the cities of other nations will be in continuous danger of sudden annihilation. All the resources of the United States, moral and material, may have to be mobilized to prevent the advent of such a world situation. Its prevention is at present the solemn responsibility of the United States--singled out by virtue of her lead in the field of atomic power.

The added material strength which this lead gives to the United States brings with it the obligation of restraint and if we were to violate this obligation our moral position would be weakened in the eyes of the world and in our own eyes. It would then be more difficult for us to live up to our responsibility of bringing the unloosened forces of destruction under control.

In view of the foregoing, we, the undersigned, respectfully petition: first, that you exercise your power as Commander-in-Chief, to rule that the United States shall not resort to the use of atomic bombs in this war unless the terms which will be imposed upon Japan have been made public in detail and Japan knowing these terms has refused to surrender; second, that in such an event the question whether or not to use atomic bombs be decided by you in the light of the considerations presented in this petition as well as all the other moral responsibilities which are involved.

R E Lapp
R S Mulliken
E P Wigner
George S. Monk
Leo Szilard

J. J. Nickson
W. H. Zachariasen
Francis R. Shonka
John A. Simpson Jr
Walter Bartky
John R. Howe

Frank Foote

DECLASSIFIED
NND 730039
By ________ NARS, Date ________

Figure 40 - Szilard Petition - U.S. National Archives, record group 77, Records of The Chief of Engineers, Manhattan Engineer District, Harrison-Bundy File, Folder #76.

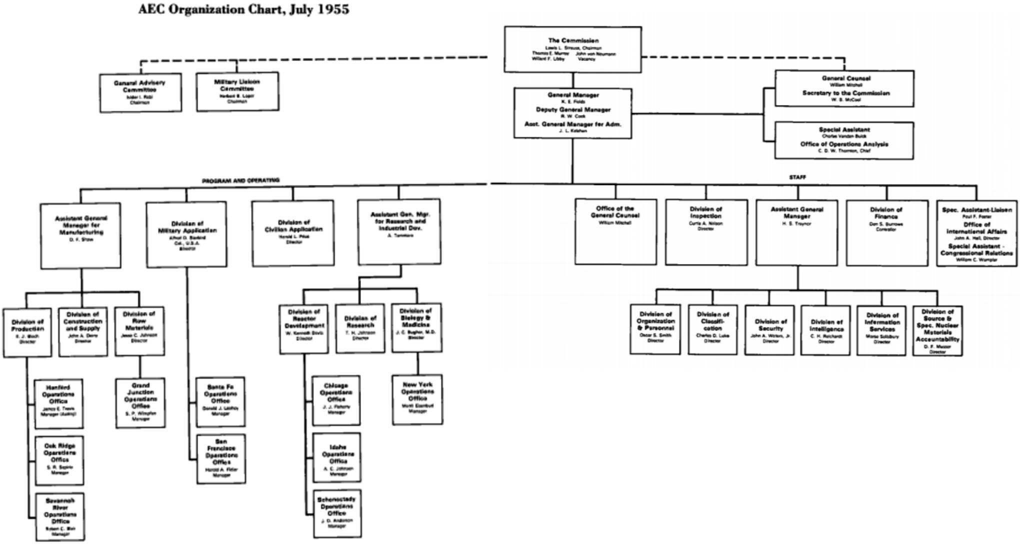


Figure 41 - Atomic Energy Commission chart 1955 (Hewlett and Hall 1989: 586-587)

He had been involved with the CAEC since 1951. As a member of the general advisory committee, he had argued for the benefits of de-classifying the study of long-term effects of bomb testing[472]; in 1953 he had started measuring residues of strontium-90 for the classified *Project Sunshine,* which tried to monitor the possible radiation hazards from bomb testing[473]; after his appointment as Commissioner, he kept working on radioactive fallout and insisted on the necessity of providing information on the matter to the

[472] Hewlett and Holl 1989, 264-266: the authors provide a very thorough report on the history of the Atomic Energy Commission and discuss at length the role of Libby since his appointment as its Commissioner.
[473] Hewlett and Holl 1989, 266 -268.

public[474]. He was convinced that the impact of nuclear testing on living beings would be negligible, and that it would not produce any observable effect on public health[475].

On several occasions he acted as the public face of the peaceful and productive use of atomic research. In 1958, he gave a speech at the Symposium of the Swiss Medical Academy on the noxious effects of low-level radiation, to reassure the public that nuclear experiments were not harmful to their health:

> "Le monde entier s'occupe aujourd'hui de la question des retombées radioactives, en particulier de celles qui suivent l'expérimentation d'engins nucléaires militaires. L'attention générale s'est concentrée sur les problèmes concernant l'effet des radiations, et ces questions qui autrefois ne préoccupaient qu'un nombre restreint d'hommes de science sont discutées aujourd'hui par des millions de personnes. Toutefois, si les effets des radiations et la grandeur des doses des précipitations radioactives étaient mieux connus et compris, l'appréhension générale serait beaucoup moins grande."[476]

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474 Hewlett and Holl 1989, 279-287.

475 Libby 1958.

476 Libby 1958, 344-345.

As he was in perfect agreement with president Eisenhower's propaganda, he also believed that the development of nuclear military equipment would act as a warranty for universal peace:

> "Our peace is largely atomic; knowing what lies ahead in nuclear war leaves no doubt about it. The leaders of our country, of Russia, and of England know exactly this, and none of these countries could conceivably start a war without certain knowledge of such serious effects on itself that it would be essentially purposeful national suicide. So the likelihood of war is reduced by the very terror the atom has given it, and thus in a way, we might say that the first certain peacetime use of atomic energy has been peace itself"[477].

In the context of the Atoms for Peace project, he strongly opposed the widespread opinion that radioactivity was an invasive, unnatural and artificial achievement. The very method of radiocarbon dating was based on the fact that living organisms are and have always been radioactive:

> "Since plants live off the carbon dioxide, all plants will be radioactive; since animals on earth live off the plants, all animals will be radioactive. Thus we

[477] Libby 1959, 633.

> conclude that all living things will be rendered radioactive by the cosmic radiation"[478].

4.2.2 Radiocarbon dates and "World Prehistory"

There was at least one other factor which contributed to the popularity of radiocarbon dating: this method allowed for the synchronization of geographically distant areas and, therefore, was a premise necessary for constructing a global history of humankind. Frederick Johnson, chairman of the "Committee on Radioactive Carbon 14", was aware of the importance of the method for the construction of a universal time scale. When he contributed to Libby's major publication with a chapter on the significance of radiocarbon dates, he wrote:

> "The dated samples have come from several parts of Europe and Africa, the Near East, Oceania, and North, Middle and South America. […] It is apparent that there is in existence the basis for a chronology which is worldwide in scope"[479].

According to him, this was a reason for excitement for many archaeologists in different fields, as the synchronisation of

[478] Libby 1955, 5.
[479] Johnson 1955, 141-142.

distant sites was often a major task of archaeological research and often a problematic one[480].

The existence of an external dating method based on chemical physics would give archaeologists and geologists the possibility to build chronological grids which could encompass the entire earth. This would be the first step towards the writing of a global history of humankind which could reunite what war had divided. This was the aim of Grahame Clark, when he wrote his 1961 volume titled *World Prehistory*. The book was reprinted with significant improvements in 1969 and 1977. Starting from the second edition, the relevant radiocarbon dates were listed at the end of each chapter. Since the very beginning of this project, the declared intention of the author was to build a common history of humanity, going beyond nationalism and towards unity:

> "La cosa più necessaria è certamente una concezione della storia capace di conciliare le esigenze delle società nazionali con quelle di un ordine mondiale. E se accettiamo la posizione di Toynbee, che rifiuta di identificare la storia universale con la storia della civiltà occidentale, la sua insistenza sull'autonomia delle grandi civiltà letterarie corre il pericolo di sostituire alle rivalità nazionali le rivalità culturali.

480 *Ibidem*, 146-147.

> Ma il passato preistorico, che Toynbee in complesso trascura, ci mostra invece l'unica fonte comune di tutte le civiltà e così di tutte le storie scritte."[481]

His aim was to build a historical account that takes into consideration different developmental stages of human societies – within a perspective of social evolutionism – and thereby tracking the qualitative enrichment of human life in its progression[482]. The history of humanity is described as a progressive strife towards complexity and depth, both in technology and social organisation: all peoples follow the same trajectory, but some are delayed on the path to civilisation[483]. The author declares that, while trying to avoid any regionalism, he wants to highlight the spatio-temporal coordinates where certain relevant features emerge. To achieve this goal, a universal chronological grid is necessary:

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[481] Clark 1967 [1961], 9. The last volume of Arnold J. Toynbee *A Study of History* (1934-1961) was issued that same year: this set of ten volumes described the rise and fall of several civilisations, giving vast space to non-western perspectives. The author described progress as the response of creative groups to internal or external challenges, while decline was construed as the inability to creatively respond to challenges. The series was a success and the author became one of the most influential thinkers of his time (cf. McNeil 1989 for an intellectual biography; Momigliano 1963 for a review on a XII book called 'Reconsiderations'; see also Lang 2011 for a recent appraisal).

[482] Clark 1967 [1961]: 292.

[483] *Ibidem*, 291-293.

> "Finché ci si limita alla preistoria di una singola regione, si può combinare un profilo soddisfacente analizzando i cambiamenti della moda e del costume, l'associazione dei reperti relativi a un singolo periodo che presenti una certa varietà di manufatti, e la sequenza dei depositi nelle stazioni archeologiche; inoltre gli eventi svoltisi in regioni diverse possono spesso essere sincronizzati se fra esse esistevano dei rapporti commerciali. Ma non appena il campo di studi si amplia […] si manifesta più chiaramente il bisogno di un sistema cronologico più universale"[484].

This 'universal chronological system' can be found in radiocarbon dating. Clark is aware of the method's limitations, from possible contamination to statistical errors, and warns his readers of the potential inaccuracy of some radiocarbon dates. However, the overall validity of the method and the growing availability of data allow the creation of a general framework for World Prehistory, where discrepancies can be investigated[485]. In the following edition of this volume, radiocarbon dates acquired growing significance[486]: listed at the end of each chapter (Fig. 41a),

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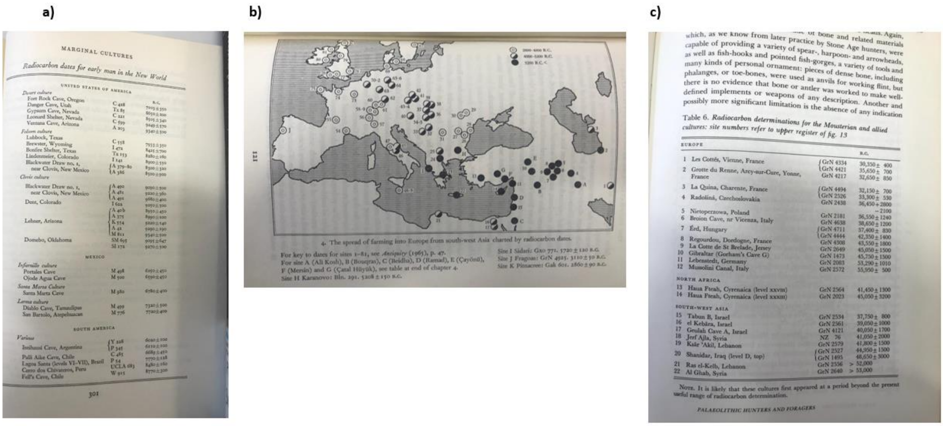
[484] *Ibidem*, 11.
[485] *Ibidem*, 12-13.
[486] Clark 1969.

they served as a reference for years to come – until the third edition introduced the results of calibration[487].



			B.C.
EUROPE			
1	Les Cottés, Vienne, France	GrN 4334	30,350 ± 400
2	Grotte du Renne, Arcy-sur-Cure, Yonne, France	GrN 4421	35,650 ± 700
		GrN 4217	32,650 ± 850
3	La Quina, Charente, France	GrN 4494	32,150 ± 700
4	Radošiná, Czechoslovakia	GrN 2526	33,300 ± 530
		GrN 2438	36,450 + 2800 − 2100
5	Nietoperzowa, Poland	GrN 2181	36,550 ± 1240
6	Broion Cave, nr Vicenza, Italy	GrN 4638	38,650 ± 1200
7	Érd, Hungary	GrN 4711	37,400 ± 830
		GrN 4444	42,350 ± 1400
8	Regourdou, Dordogne, France	GrN 4308	43,550 ± 1800
9	La Cotte de St Brelade, Jersey	GrN 2649	45,050 ± 1500
10	Gibraltar (Gorham's Cave G)	GrN 1473	45,750 ± 1500
11	Lebenstedt, Germany	GrN 2083	53,290 ± 1010
12	Mussolini Canal, Italy	GrN 2572	55,950 ± 500
NORTH AFRICA			
13	Haua Fteah, Cyrenaica (level xxviii)	GrN 2564	41,450 ± 1300
14	Haua Fteah, Cyrenaica (level xxxiii)	GrN 2023	45,050 ± 3200
SOUTH-WEST ASIA			
15	Tabun B, Israel	GrN 2534	37,750 ± 800
16	el Kebāra, Israel	GrN 2561	39,050 ± 1000
17	Geulah Cave A, Israel	GrN 4121	40,050 ± 1700
18	Jerf Ajla, Syria	NZ 76	41,050 ± 2000
19	Ksār 'Akil, Lebanon	GrN 2579	41,800 ± 1500
20	Shanidar, Iraq (level D, top)	GrN 2527	44,950 ± 1500
		GrN 1495	48,650 ± 3000
21	Ras el-Kelb, Lebanon	GrN 2556	> 52,000
22	Al Ghab, Syria	GrN 2640	> 53,000

NOTE. It is likely that these cultures first appeared at a period beyond the present useful range of radiocarbon determination.

PALAEOLITHIC HUNTERS AND FORAGERS

Figure 42 - a) Grahame Clark, 1969, *World Prehistory: A New Outline*, 301; B) Grahame Clark, 1969, *World Prehistory: A New Outline*, 121 C) Grahame Clark, 1977, *World Prehistory in New Perspective* Table 6.

4.2.3 A matter of scales

Another crucial aspect for a contextual analysis of radiocarbon dating is the scientific theory in which the method is embedded and the notion of time it entails: this is largely dependent on the scale of observation.

The radiocarbon method relies on the principle of radioactive decay, i.e. the process by which the nucleus of an

[487] Clark 1977.

unstable isotope emits particles of ionizing radiation. At the microscale, radioactive decay is a stochastic process, so that predictions cannot be formulated about the time when a certain atom will decay[488]. At the macroscale, it behaves predictably, even to the extent that unstable isotopes are used as 'natural clocks' (biological or physical phenomena from which a measure of time can be inferred)[489]. For this reason, radioactive decay is often presented as an example of compatibility between a stochastic and a deterministic view of nature[490].

The formula for the curve of radioactive decay was theorised at the beginning of the 20th century by Ernest Rutherford, a 1908 Nobel Laureate who paved the way for Libby's new method: the equation used for calculating radiocarbon decay rate was an adaptation of the one developed by his predecessor.

> "Therefore, we conclude that the rate of disappearance of radioactivity following death, corresponds to the exponential decay law for

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488 Among the many articles providing an in-depth explanation of the physical processes behind radiocarbon dating (Olsson 2009, Ramsey 2008, Wood 2015), Malainey (2011, 27-34) devotes a chapter to radioactive decay.

489 McDougall 2009 provides an overview of several 'natural clocks' and chronicles the advancement in the history of research.

490 Brakel 1985 provides an insightful analysis of the influence of radioactive decay on the concept of probability.

radiocarbon as represented by the solid curve in Figure 1 (*Fig.38a-b in this volume, ndr.*), in which the world-wide assay of 15.3 for biological materials corresponds to zero time, and the predicted specific radioactivities for various times thereafter are given by the curve. The equation for the curve is

$$I = 15.3 \exp.\left(-0.693 \frac{t}{5568}\right)$$

In which *t* is the age of the organic material in years, age being defined as the time elapsed since death occurred."[491]

At the macroscale, time is treated as a line, visually represented in the "Curve of Knowns": as the decay rate is exponential the visual referent is a curve instead of a linear progression. The decay curve is drawn on a cartesian plan where single events – i.e. the moments when the decay started for the selected samples (death of an animal, growth of a tree ring) – can be plotted. Despite coming from nuclear physics, the realm of a counterintuitive concepts of time[492], radiocarbon time resembles the time of human experience. It can be noted that the language used by scholars to describe

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[491] Libby 1955, 9.

[492] For a didactic treatment of the topic cf. Rovelli 2017; a more in-depth explanation of the concept of time in special relativity can be found in Savitt 2011.

radioactive decay is modelled on biological vocabulary. The very notions of "life-cycle" and "decay" recall the tripartite schema of birth, growth and death which is characteristic of biology. Interestingly enough, these words are used here to describe the cycle of atoms, which are independent from the living organisms they make up:

> "It has been known for many years that the rate of disintegration of radioactive bodies is extraordinarily immutable, being independent of the nature of the chemical compound in which the radioactive body resides and of the temperature, pressure, and other physical characteristics of its environment"[493].

Radiocarbon decay, thanks to his long half-life, can be treated *as if* it mirrored to a universal homogeneous time underlying human and natural events.

Such a concept of time, of course, has been challenged by Einstein's Theory of Special Relativity: one of the most disarming assumptions to emerge from Einstein's work was in fact the relativity of simultaneity[494].

> "That light requires the same time to traverse the path A –> M as for the path B –> M is in reality

[493] Libby 1955, 9.
[494] The same problem was investigated in the same years by Poincaré (1900, 1905).

> neither a supposition nor a hypothesis about the physical nature of light, but a stipulation which I can make of my own freewill in order to arrive at a definition of simultaneity"[495].

The famous example of the train and platform demonstrated this. Two events (e.g. two separate lightning strikes) can be perceived as simultaneous for someone who stands on a platform, but diachronous for a person who moves on a train: perception of simultaneity depends on the system of reference. Less intuitively, simultaneity cannot be measured: two identical (atomic) clocks, one at the feet and one at the top of a 40m high tower, tick at different speeds[496]. If the spacetime coordinates of clocks affect their rate, then they cannot be synchronized and they cannot be used to measure the synchronicity/diachronicity of phenomena. Imagine that phenomenon A is measured by clock A′: it measures an interval of 10 milliseconds between the beginning and the end of the phenomenon A. Phenomenon B is measured by clock B′: it measures an interval of 10 milliseconds between the beginning and the end of the phenomenon B. The 10 milliseconds measured by clock A and clock B will not be comparable as they tick at an infinitesimally different rate[497].

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[495] Einstein 1920, 23. The volume is a translation of his main 1905 and 1916 articles, originally published in *Annalen der Physik.*
[496] Savitt 2011, 5-10.
[497] Savitt 2011, 10-14.

While this issue concerns radiocarbon at the microscale, at the macroscale the time relation between two points on the line of time is not registered by synchronized instruments, it is instead given by the measurement of an inner chemical property of the samples themselves. To this extent Libby's nuclear clock may give the impression of a reassuring objectivity of time.

This way of time measurement, however, defines the temporality of events, i.e. identifies the moment when the organism, from which the sample has been taken, stopped exchanging carbon with the reservoir. The application of this method to (archaeological) objects needs careful consideration, especially when radiocarbon dates are used in connection with other methods. Manmade objects, in effect, are better related to a time duration than to discrete events: they have a biography and dividing them into distinct periods requires approximation[498]. This discrepancy between measurable time and the non-discrete flow of change in technology and manufacture was already acknowledged by Johnson in the chapter on the significance of radiocarbon dates, published in Libby's 1955 handbook on radiocarbon dating:

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498 Numerous works have been written on this matter in the field of theoretical archaeology (see, among others, Gosden 1994, Murray 1999, Bailey 2007). Especially on the connection between radiocarbon dating and human time, see Lucas 1997.

> "It has been customary procedure to compare historical, geological and archaeological measurements of time with the radiocarbon measurements. […] The problem is to find a basis for comparison. Historical data, that is, measurements of time based upon a calendrical system and dependent written ethno-historic record, appear to be comparable with the results of the radiocarbon method as expressed in number of years before the present. In other words, dates in both systems are preferable to a single method of counting time. This is not true of geological and archaeological measurements, except in relatively rare instances. Measurements of time in these fields are inferred from processes, the rates of change or progress of which are not constant and which are, as yet, quite unpredictable"[499].

This problem paves the way for considerations of the reciprocal influence of radiocarbon dating and archaeological practice. The radiocarbon method was heavily influenced by the specific archaeological milieu in which it was born and its traditions.

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[499] Johnson 1955, 143.

4.3 Radiocarbon dating in the academic context before the "Second Revolution"

In the previous section, the scientific and technological advancements instrumental to the invention of radiocarbon dating have been discussed. Some space has also been devoted to the analysis of the historical and intellectual circumstances under which the method was developed and used[500]. Two important facets not yet discussed are the intellectual and social environments in which radiocarbon dates were first used. Especially their inclusion in (or exclusion from) crucial archaeological syntheses, their publication venues and the chronological debates where they were first employed, play a significant role in the history of the method and allow us to frame it in its academic context. These aspects are here investigated through qualitative and quantitative analyses of relevant bibliography from the 1950s and 1960s.

4.3.1 Against Radiocarbon: from Clark and Childe to Milojčić and Piggott

Many scholars have tried to investigate how the birth of radiocarbon dating impacted subsequent developments in

[500] *Vide supra* 293-308 and 308-326, respectively.

the history of archaeological thought[501]. Here, vice-versa, some attention will be devoted to the means through which radiocarbon dates entered the archaeological discourse, highlighting how archaeology affected radiocarbon dating and its development in recent history.

The use of radiocarbon dates in the works of two extremely influential authors – Grahame Clark and Gordon Childe – can help us to understand how radiocarbon dates entered the archaeological discourse: it has been mentioned that Grahame Clark made extensive use of radiocarbon dates in his attempt to reconstruct the (pre)history of humankind[502]. It should be noted that all three editions of *World Prehistory* were extremely well received and impactful, in the archaeological and anthropological communities: the books received more than 25 reviews, some of which in journals that go beyond specialist interest, such as *Man* and *Science*[503]. The author was aware of the limitations and opportunities of radiocarbon dating, as well as of the physical principles

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[501] Taylor 2000, 100-104, provides a summary of these attempts.

[502] *Vide supra* 315-319.

[503] Reviews to the first edition: among others, Ehrich 1961 (*Science*), Willey 1961, Cole 1962 (*Man)*, Rouse 1962, Connah 1963; Reviews of the second edition: among others, Greengo 1970, Shaw 1970, Fritz 1971, Jacobson 1973; Reviews of the third edition: among others, Gabel 1978 (*American Scientist)*, Trigger 1978, Bisson 1980, Guilaine 1980 (*Les Annales*). On the legacy of Grahame Clark see the articles published in the volume edited by Marciniak and Coles in 2010, and Fagan 2001 for an intellectual biography.

the technique is based on[504]. He provided tables with radiocarbon dates, at first interspersed in the chapter and then orderly listed at the end of each chapter, allowing for an encyclopedic use of the data (cf. Figg. 42a, c).

In the Preface to the sixth edition of *The Dawn of European Civilization,* issued in 1958, Gordon Childe mentioned radiocarbon dating as one of the new developments that 'demand a drastic revision' of his text[505]. This volume is famously one of the most influential archaeological works ever written, and directly affected the construction of many local chrono-cultural frameworks[506]. In the sixth edition radiocarbon dates were interspersed in the text and they were cautiously used as evidence in the numerous chronological discussions reported in the chapters[507]: the reader was cautioned that they are 'tentative and

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504 Clark 1967 [1961], 9-10.

505 Childe 1958, III.

506 Several volumes on the fortune of Childe's work have been produced, cf. the volume edited by Harris (1994) and the one edited by Gathercole; Irving and Melleuish (1995) on Childe's role in Australian archaeology and several articles by Bruce Trigger (1980, 1984, 1994). As an example, one could mention that, in the preface to the English edition of Sicily before the Greeks by Bernabò Brea, Glyn Daniel refers to a conversation with the author: the volume is dedicated to Gordon Childe because, admittedly, he did not understand European prehistory until reading Childe's work (Daniel 1957, IV)

507 E.g. the early Neolithic in the western Baltic territory (Childe 1958, 177-182), or the dating of the Almeria culture (*Ibidem* 267-270).

provisional' estimates[508], but the measurement errors (the ± figure) were omitted. The aim of the author was to establish the temporal and geographical boundaries – as well as the specific characters – of European cultures[509]. However, he was mostly interested in relative chronology, and especially in the confirmation of the diffusionist hypothesis, which attributed the main civilising inventions (especially farming and metallurgy) to Near-Eastern cultures, and saw their occurrence in the West as a later transmission – and progression – through diffusion[510]. His construction was therefore dependent on relative chronology and allowed for flexible boundaries, especially for the period before 1,400 B.C., when findings suitable for cross-dating are scarce.

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508 *Ibidem*, III.

509 *Ibidem*, 341-342.

510 In the Preface to the 6th edition of *The Dawn of European Civilization* he states that "Radio-carbon dating has indeed vindicated the Orient's priority over Europe in farming and metallurgy" (Childe 1958, II). The synthesis provided in many of Gordon Childe's books was only seriously shaken by what Colin Renfrew called the 'second radiocarbon revolution' (i.e. by the results of radiocarbon calibration), when the older dates for northern European megalithic constructions disproved crucial diffusionist assumptions (Renfrew 1973, 76-133).

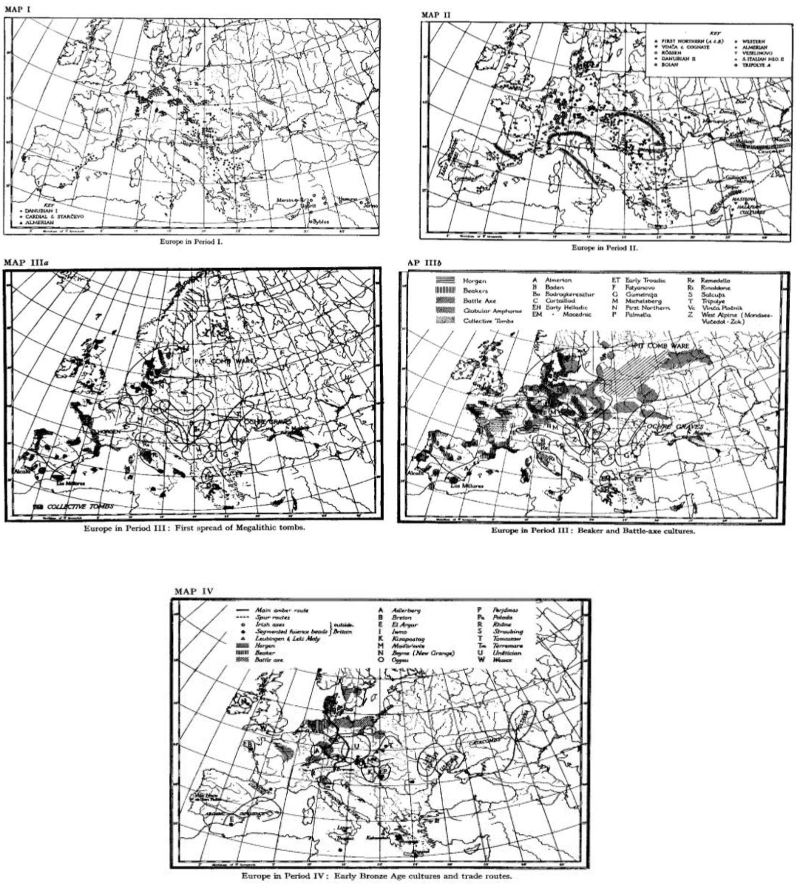

Figure 43 - Gordon Childe, The Dawn of European Civilization, 6th edition, 1958, 348-352.

At the end of the volume he provided a simplified and admittedly approximate picture of Europe's prehistory in five synchronic maps (Fig. 43) and two chrono-cultural tables (Figg. 44):

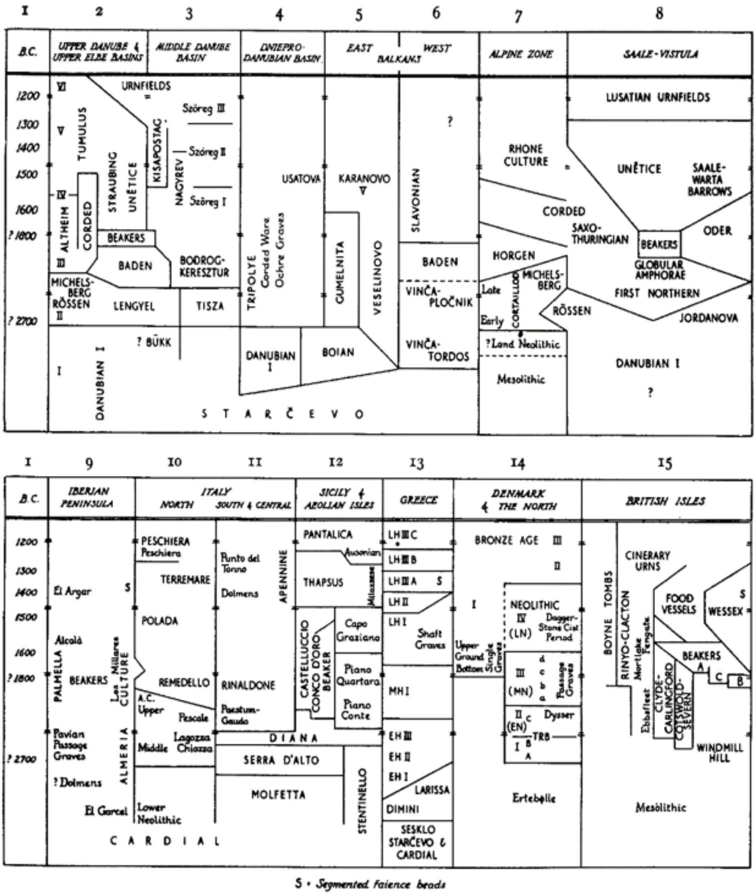

Figure 44 - Gordon Childe, The Dawn of European Civilization, 6th edition, 1958, 346-347.

"The distribution of entries on the several maps is based on the chronological discussions included in all the preceding chapters and summarized in the following tables. In most of the columns the actual order of the entries, the sequence of cultures, is reasonably well established, though here again a reference to the text will disclose doubts as to the order both in the extreme West and in the East. But each column is virtually independent and should be regarded as a single scroll hanging freely from its own roller. The lower end is always loose, so that, as far as pure archaeology is concerned, each scroll could be rolled up at least to the 1400 notch deduced from segmented fayence beads. Nuclear physicists have indeed diffidently offered some provisional radio-carbon dates that might act as pins to keep some scrolls extended. So in column 15 the Windmill Hill culture (at Ehenside Tarn in the Lake District!) might be pinned about 3000 B.C. and the Secondary Neolithic of Stonehenge I at 1850; in column 7 Early Cortaillod about 2740,8 and in column 14 the earliest, A, funnel-beakers at 2650, while in column 2 Danubian I (in Germany!) might go back before 4000. But radio-carbon dating proves to be infected by so many potential sources of error that European prehistorians accept its results with as much reserve as the physicists offer them."

This last sentence may refer to a debate on the validity of the radiocarbon dating method raised in the second half of the 1950s in European scholarship, fostered by those scholars who had recently attempted to establish an absolute chronology for the European Neolithic: notably Milojčić for central and south-eastern Europe[511], and Piggot for the British Isles[512]. Those scholars, especially the former, opened and participated in lively debates in prestigious journals, such as *Germania*[513] and *Antiquity*[514]. In 1957, Vladimir Milojčić publishes a strong paper on the 'Applicability of radiocarbon dating to prehistory'[515]: while recognising the importance of absolute dating for comparing distant sites in prehistoric studies[516], he pointed out that radiocarbon dating relied on under-verified assumptions that, if found untrue, would invalidate the entire methodology:

> "Es ist selbstverständlich, daß, wenn auch nur eine von ihnen nicht zutreffen sollte, die ganze Methode fragwürdig, wenn nich überhaupt unbrauchbar ist." [517]

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[511] Milojčić 1949.
[512] Piggott 1954
[513] Milojčić 1957, Schwabedissen und Münnich 1958, Milojčić 1958.
[514] Barker 1958, Piggott 1959b, Waterbolk 1960, Libby 1963, Smith 1964.
[515] *Zur Anwendbarkeit der C 14-Datierung in der Vorgeschichtsforschung* (1957).
[516] Milojčić 1957, 102.
[517] *Ibidem* 104.

He lists six assumptions on which the method is based: the intensity of cosmic rays and their effect on neutrons has been unchanged in the last 30,000 years; the stability of the effects of neutrons on nitrogen atoms, their transformation in the radioactive isotope of carbon and the ability of radiocarbon of mixing with oxygen to form carbon dioxide; the concentration of ^{14}C in the atmosphere has been constant over the radiocarbon age range; the isotopic composition of organic object only varies due to radioactive decay and for no other reason; no other factor than radioactive decay affects the concentration of ^{14}C in the sample; the calculation of radiocarbon's half-life is correct[518]. However, none of these assumptions are verified facts: the third, which implies the first two mentioned by Milojčić, and the sixth had been the subject of further study since the very inception of radiocarbon dating, as Schwabedissen and Münnich pointed out in their response to the cited article, in the following issue of the *Germania*[519]. What is important to draw attention to is the idea that even if a single one of these assumptions turned out to be wrong, the entire method as it stood at the time, would have been compromised. As Schwabedissen explains:

> "Danach besitz die Methode […] ein durchaus solides Fundament; es kommt nur darauf an, die

[518] *Ibidem* 104.

[519] Schwabedissen und Münnich 1958.

> Fehlerquellen zu erfassen und immer mehr auszuschalten. Fehlerquellen gibt es bei jeder Methode. Selbst ein ungestört erscheinendes Profil oder eine noch so saubere Grabungsfläche können alte Störungen enthalten, die nicht immer erkennbar sein müssen.[520]

Here, the author mentions the possibility that layers in a stratigraphic sequence may be disturbed and that resulting errors can be corrected if the source of the error is identified. Why couldn't this apply to radiocarbon dating? Errors are part of every method but, especially when they are systematic, they can be corrected and do not automatically invalidate the reliability of the method, nor its usability[521]. When analysing Milojčić position, one should consider that the target of his polemic are those archaeologists who employ radiocarbon dates with blind faith:

> "Trotzdem blieb der verständliche Wunsch nach einer "unumstrittenen" und "objektiven" Chronologie hei vielen bestehen, und es lag und liegt nichts näher, als hei der "objektiven" Naturwissenschaft die Erfüllung dieses Wunsches zu suchen. […] Somit ergab keine naturwissenschaftliche Methode bis jetzt brauchbare Handhaben für genaue

[520] *Ibidem*, 141.
[521] *Ibidem*, 145.

> absolutchronologische Bestimmungen in der europäischen Vorgeschichte. Seit aber in Amerika die epochalen Entdeckungen W. E. Libbys und seiner Mitarbeiter mit C 14 gemacht wurden, ist Adelfach die Meinung entstanden, als ob wir uns dank einer naturwissenschaftlichen Methode endlich an der Schwelle der Erfüllung des Wunsches nach einer "objektiven" Zeitbestimmung befänden, für die unser "prähistorisches Gewissen" keine Verantwortung tragen muß."[522]

His criticism is directed at the presumption of objectivity and prefect accuracy often attributed to 'scientific' measurement methods – *as if* they behaved in a perfectly deterministic way –, thus arguing against the practice of using radiocarbon dates as if they were calendrical years[523].

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[522] Milojčić 1957, 102-103.

[523] Cf. pg. 103: "In den sieben Jahren, die verflossen sind, seit die ersten Versuche mit C 14 angestellt wurden, ist bereits eine überwältigende Reihe Zeitbestimmungen an prähistorischen Funden durchgeführt worden, doch ist bedauerlicherweise die Zahl der kontrollierbaren aus historischen Epochen im Verhältnis zu unkontrollierbaren verschwindend gering geblieben. Trotzdem beginnt man unbekümmert mit den "Ergebnissen" sein Spiel zu treiben: Man vergleicht die vom „Radiochemiker" erzielten Zeitansätze völlig unbedenklich mit jenen, die durch historische Überlegungen gewonnen wurden, behandelt sie als gleichwertig und zieht weittragende Schlußfolgerungen, alles in der Annahme, daß die naturwissenschaftliche C 14-Methode völlig

This paper was praised by Schwabedissen and Münnich[524] (who took to advocate radiocarbon dating) for recommending caution when using radiocarbon dates. However, Milojčić's critique seems to imply another argument. He seems to suggest that, since radiocarbon dating does not assure the desired objectivity and precision, this defeats the purpose of 'scientific' methods in Prehistoric research. Archaeology should first exploit its own resources – which are not to be deemed less reliable than the supposedly scientific ones[525] – before turning to other disciplines:

> "Es ist auch selbstverständlich, daß wir Prähistoriker noch weit von einem zuverlässigen absolutchronologischen System für das Neolithikum und auch für die Metallzeiten Europas entfernt sind, <u>das zu erarbeiten unsere eigenste Aufgabe ist</u>. […] <u>Vielleicht ist es besser,</u>

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unbestechlich, objektiv und in ihren Ergebnissen ganz sicher ist. Die Neigung zu solchen Annahmen liegt vielleicht auch dem Prähistoriker umso näher, als er die Schwierigkeiten und die Problematik der naturwissenschaftlichen Atomforschung und speziell die der C 14-Methode nicht überblickt und sie deswegen leicht als nicht existent betrachtet."

[524] Schwabedissen und Münnich 1958, 133.

[525] He refers specifically to Comparative Stratigraphic Chronology, of which he was the main proponent, together with his colleague Schaeffer (cf. Milojčić 1957, 102).

> daß wir zuerst die Möglichkeiten unseres Faches voll ausschöpfen, bevor Übernahmen aus anderen Disziplinen erfolgen. Es ist auch nicht so, daß wir uns grundsätzlich gegen die Zusammenarbeit mit naturwissenschaftlichen Disziplinen wenden, die sicher sehr nützlich ist. Wogegen wir uns aber wenden müssen, das ist eine eventuelle falsche Anwendung der naturwissenschaftlichen Ergebnisse innerhalb der Prähistorie, die darin besteht, daß man diese Ergebnisse als "objektive Tatsachen" hinzustellen versucht und Feststellungen der Vorgeschichtsforschung zugunsten dieser "objektiven Tatsachen" zurückzusetzen geneigt ist, wie es in der letzten Zeit mehrfach geschah." (*added emphases)*[526]

The criticisms expressed by Milojčić resonated with the archaeological community despite the linguistic barrier[527], so that in 1958 Harold Barker – who was in charge of the radiocarbon dating laboratory of the British Museum – agreed to publish an article in *Antiquity* to explicitly address concerns raised by the German-Yugoslavian archaeologist's articles. The following year, in the same journal, Stuart Piggott, one of the most prominent English archaeologists, commented on the accuracy of two dates obtained on one

[526] Milojčić 1958, 410.
[527] Webster 1958, 193.

charcoal sample found beneath the chalk rubble mound of the Henge of Durrington Walls: the results obtained in Groningen were 4575 ± 40 and 4585 ± 70 years B.P. He famously described the dates as being "archaeologically inacceptable"[528]: the charcoal was supposed to be contemporaneous to the Henge, which should in turn be synchronous to the Beaker pottery sequence. However, much younger radiocarbon dates, matching archaeological chronologies, had previously been obtained on the Dutch Beaker sequence[529]. His article did not question the validity of the method – indeed he used previous radiocarbon dates to reject the new ones. Furthermore, he had shown on other occasions (e.g. when commenting on the dating of antler samples from Stonehenge) that he understood the probabilistic nature of radiocarbon results:

> "[There is a] two to one chance of the real date lying anywhere between 1860 and 1560 B.C. […] since statistically any date within this bracket is acceptable, we must interpret the figures in terms of the archaeological evidence. In this instance, if our equation between the building of the first phase of Stonehenge I11 and the rich Wessex Bronze Age culture . . . is accepted, we must take a date as near 1560 as possible, for only by so

[528] Piggott 1959b, 289.

[529] *Ibidem* 290.

doing can the dates obtained by the two methods- that of archaeology and that of physical science- be made consonant."[530]

His comments on radiocarbon dating fueled the debate: the above quote was reported in the Editorial of number 132 of *Antiquity,* together with remarks on the discrepancies between radiocarbon dates and the traditional archaeological framework of prehistoric Europe, especially when it came to the Neolithic period[531]. In the latter case, it was not the reliability of the method that was questioned but its validity for the dating of Prehistoric Europe: while Americanist archaeology could benefit from the method even with a wider margin of error, in Europe, before dismantling the pre-existing chronological sequences, archaeologists needed more assurances on the affordances of radiocarbon dating could provide in terms of precision and accuracy[532].

Already Münnich[533] and De Vries[534] had demonstrated that the radiocarbon concentration in the biosphere could change over time and that this could affect the results of radiocarbon dating. In the 1960 issue of *Antiquity,* Waterbolk reported the

530 Piggott 1959a, 133.
531 *Antiquity,* Issue 132 – Editorial, 238.
532 *Ibidem.*
533 1957.
534 1958.

results of the 1959 Radiocarbon Symposium at Groningen, where the existence of the De Vries effect had been confirmed for a period of 1,200 years[535]. As Schwabedissen und Münnich had done before[536], he devoted great attention to explaining how this problem did not threaten the overall validity of the method and its potential for archaeological research, and which steps had already been taken to resolve it[537]. In 1960, Willis, Tauber and Münnich suggested to build a calibration curve by means of comparing radiocarbon dates with respective dendrochronological measurements[538].

The abovementioned discussion among archaeologists and radiocarbon scientists on the reliability of the method and the accuracy of radiocarbon dates had at least one major effect: it became clear that the development of corrections for the De Vries effect was of the outmost importance to ensure the trust of the archaeological community. Therefore, the study of the atmospheric variations of ^{14}C concentration was expedited, especially in laboratories concerned with Old World Archaeology[539].

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535 Waterbolk 1960, 14.

536 *Loc. Cit.*

537 Waterbolk 1960, 18.

538 Willis, Tauber and Münnich 1960.

539 The laboratories in Cambridge, Copenhagen and Heidelberg were very active in this direction (cf. Willis, Tauber, Münnich 1960).

4.3.2 *Science*, *Antiquity*, *Les Annales*: before *Radiocarbon*

The previous subsection has shown that, in its early years, radiocarbon dating encountered various academic contexts, which it impacted, albeit differently. The intellectual paradigms operating in these contexts, in turn, affected the way radiocarbon was – or was not – applied to archaeological research questions, and how its reliability was perceived.

By the end of the 1950s and throughout the 1960s, archaeologists and scientists interested in radiocarbon dating had two main concerns: the 'Suess effect' and the 'de Vries' effect. The concentration of radiocarbon in samples before decay was normally assumed to be the same as radiocarbon concentration in the biosphere in the present; however, in 1955 Suess published an article where he showed that industrialisation (and later atomic bomb testing) led to an increase in ^{14}C concentration, making present measurements unsuitable as a reference standard: this phenomenon became known as the Suess effect[540]. In 1958 de Vries showed that fluctuations of atmospheric radiocarbon most likely also happened in the past: this phenomenon was therefore named 'de Vries' effect[541]. Investigations and experiments on these anomalies, as well

[540] Suess 1955.

[541] De Vries 1958.

as proposals for their correction, were the subject of intense study and several publications: in the first three issues of the journal *Radiocarbon,* five papers were entirely devoted to these topics[542] and at least twelve date lists tried to adopt a correction for the Suess effect (beyond the adoption of NBS standard oxalic acid as 95% of contemporary activity)[543], and/or added expanded error terms to include the variability due to the de Vries effect[544].

Since its inception in 1959, *Radiocarbon* was the fundamental venue for publishing lists of radiocarbon dates, methodological discussions on the method and, later, chronological research papers involving radiocarbon measurements. At first, the journal wasissued as a specialised supplement to the *American Journal of Science*, and was mostly intended for the publication and discussion of lists of dates produced by the over 36 radiocarbon laboratories that were already active in 1959[545].

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[542] Broecker and Olson 1959; Willis, Tauber, Münnich 1960; Tauber 1960a; Godwin and Willis 1961a; Broecker and Olson 1961.

[543]It became standard practice after the 1959 Groningen International Conference cf. Waterbolk 1960.

[544] Olsson 1959; Barker and Mackey 1959; Olson and Broecker 1959; Östlund 1959; Barker and Mackey 1960; Tauber 1960b; McAulay and Watts 1961; Ralph and Ackerman 1961; Barker and Mackey 1961; Godwin and Willis 1961b; Ferrara, Fornara-Rinaldi, Tongiorgi 1961, Stuiver and Deevey 1961.

[545] List of Laboratories, *Radiocarbon* 1959, 215-218.

Before the birth of this journal, however, radiocarbon dates were discussed in other venues. We have seen that *Germania* and, even more so, *Antiquity* hosted several articles on this matter: between 1950 and 1959 the latter published at least nine papers concerning radiocarbon dating (Fig. 45a) and one review of Zeuner's monograph on geochronology[546].

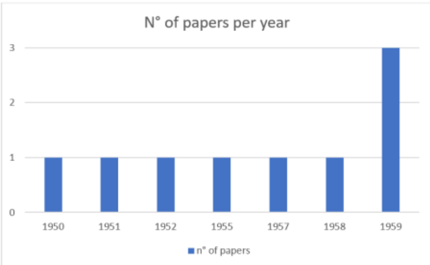

Figure 45 - Radiocarbon dating articles in *Antiquity*, 1949 -1959 a) number of papers per year; b) kind of publication: methodological explanation, list of dates from a peculiar place or laboratory, comments on a specific chronological research question.

Five of them were concerned with specific chronological research questions to which radiocarbon dating had been applied (Fig. 45b): one focused on the ruins of Zimbabwe[547],

[546] OGSC 1951.
[547] Summers 1955.

while the other four addressed the Neolithic period, two on Jericho[548] and two on English archaeology, respectively[549].

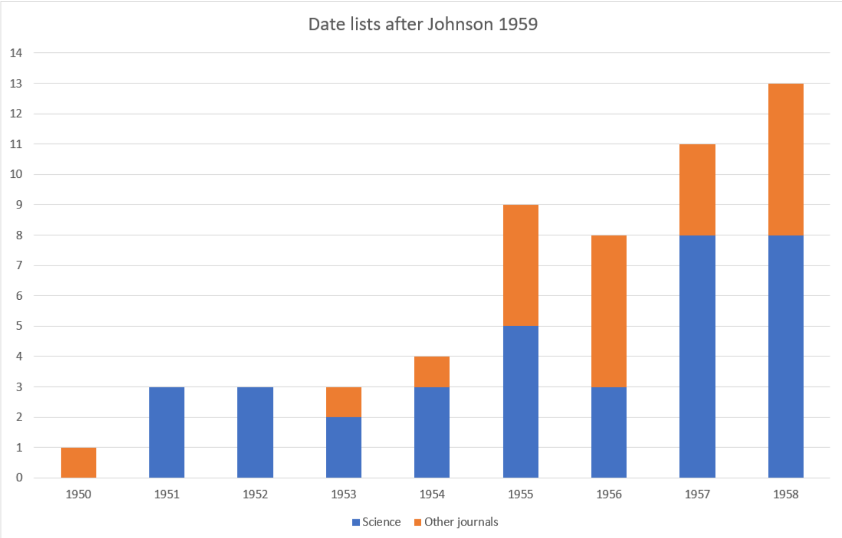

Figure 46 - Lists of radiocarbon dates 1949 -1959 (Johnson 1959).

Newly obtained lists of radiocarbon dates were mostly published in *Science* before the creation of *Radiocarbon*: 35 of the 56 lists of dates mentioned by Johnson in his bibliographical review[550] were published in *Science*; other journals barely published more than one or two lists (Table 4 and Fig. 46). There are some striking national exceptions: French laboratories consistently published in the *Bulletin de*

[548] Braidwood 1957, Kenyon 1959.
[549] Piggott 1959b, de Mallet Vatcher 1959.
[550] Johnson 1959.

l'Institut des Sciences et Technologies[551]; Canadian laboratories in the *Transactions of The Royal Society of Canada*[552]; and dates obtained in New Zealand were published in the *New Zealand Journal of Science and Technology*[553]. A couple of papers were also published in *Nature*[554], and one anonymous private citizen published a list of dates on *The Amateur Scientist (Scientific American)*[555].

Cross referencing the selected bibliography provided by Johnson in the first issue of *Radiocarbon* with a bibliographic research on Jstor and the Web of Science, one can see which journals were mostly concerned with radiocarbon dating, before the creation of *Radiocarbon* (Fig. 12a-b): *Science*, *American Antiquity*, and *Antiquity* were the venues where – in addition to the publication of radiocarbon date lists – the method and its scientific foundation were discussed, and the

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551 Perquis, Delibrias and David 1956; Delibrias and Perquis 1958.
552 McCallum 1955; Cameron 1956.
553 Fergusson and Rafter 1953, 1955, 1957.
554 de Vries and Barendsen 1954; Godwin, Suggate and Willis 1958.
555 Anonymous 1957.

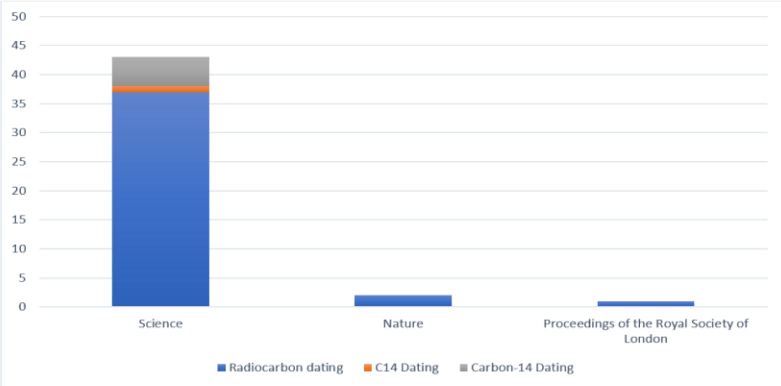
first chronological questions were addressed through radiocarbon dating.

	Science	Scientific American	Chicago Institute for Nuclear Studies	Transactions of the Royal Society of Canada	Bulletin de l'Institut des Sciences et Technologies	New Zealand Journal of Science and Technology	American Journal of Science	Nature
1950	0		1					
1951	3							
1952	3							
1953	2					1		
1954	3							1
1955	5			1		1		
1956	3			1	1			
1957	8	1				1		
1958	8				1		1	1

Table 4 - Publication venues for radiocarbon date lists 1949-1959 (Johnson 1959).

Proceedings of the Royal Society of London	Preliminary Reports of the Research Council of Alberta	Bulletin of the Santa Barbara Museum	Bulletin of the American Association of Petroleum Geologists	Chemical Society of Japan	Eiszeitung und Gegenwart	Annual Report University of London, Institute of Archaeology	Antiquity
			1			1	
	1	1		1			1
1							
					2		

a)



b)

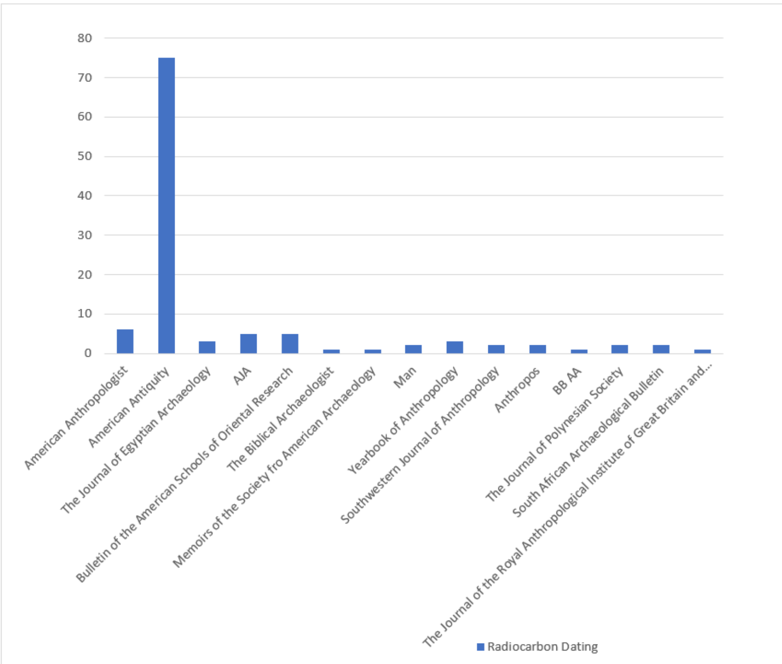


Figure 47 - Journals publishing articles concerned with radiocarbon dating between 1950-1960 a) Web of Science; b) Jstor.

Among the journals that hosted articles on radiocarbon, one might arouse the reader's interest: *Les Annales*. In 1955, the *Annales* published two papers concerned with the application of scientific methodology to archaeological questions– and one of them specifically focused on radiocarbon dating[556]. That same year Libby published the second edition of *Radiocarbon dating,* the first handbook on the subject, a book which had a very large reach can, to this day, be found in most University libraries[557]. From 1955 to 1965, the *Annales* printed several reports, reviews and articles featuring scientific analyses of archaeological remains[558], which covered topics from Raman spectrometry to dendrochronology applied to paleoclimatic studies[559], and regions from Africa to Siberia[560]. This interdisciplinary approach to artefacts and to all traces of historical societies reflected the *Annales* school's view of historical research. Aiming to construct *une histoire économique et sociale* and *une histoire des mentalitées*, this school adopted several different

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556 Wagret 1955; The other article (Salin 1955) insists on the importance of chemical analysis for the study of Merovingian metallurgy. On the complex interaction between the Annales School and archaeology cf. Schnapp 1981.

557 Libby 1955.

558 Wagret 1955, Salin 1955, Mazahéri 1958, Le Roy Ladurie 1959, Le Roy Ladurie 1960, Varagnac 1962, Le Roy Ladurie 1963, Vidal-Naquet 1965.

559 Respectively Salin 1955, and Le Roy Ladurie 1959.

560 Respectively Wagret 1955, and the Russian studies on Chinese metallography criticised by Mazahéri 1958.

methodologies and embraced the contribution of archaeology, of social and hard sciences, and of statistical modelling. The influence that this school exerted on scholars all over Europe likely contributed to the application of radiocarbon dating to historical investigations.

The first *Annales* article involving radiocarbon dating, published in 1955, was concerned with the interpretation of the Great Temple and the ruins of the dead town of Zimbabwé[561] (located in the English colony of Southern Rhodesia), a topic that was being discussed that same year in *Antiquity*[562]. The very first sentence of the article sets the tone for the rest of the argument:

> "Ignorera-t-on longtemps encore le problème des civilisations africaines? C'est un thème bien connu des lecteurs des *Annales,* familiers avec le bon combat « pour l'Histoire » mené sans arrêt par Lucien Febvre, que le scandaleux oubli où son trop longtemps demeurés les problèmes extra-européens. »[563]

The ruins had been explored and described during the second half of the 19th century by Karl Mauch. Afterwards, Randall MacIver (in 1905) and Miss Caton-Thompson (in

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561 Wagret 1955.
562 Summers 1955.
563 Wagret 1955, 363.

1929) conducted excavation campaigns[564]. Nonetheless, in 1955, the civilisation that had built and inhabited these ruins was still unknown and several extremely different chronological hypotheses had been formulated – the archaeology of Sub-Saharan Africa was so unstudied that typological identifications were not possible. Hypotheses spanned from the identification with the biblical kingdom of Ophir governed by the Queen of Saba, to the legendary king Monomotapa mentioned by Portuguese travellers of the 14th and 15th centuries[565]. In 1950, the ruins' conservator found a chunk of wood inside the Great Temple and sent it to Chicago and to London to be dated through the newly established radiocarbon method: the result (543 -752 A.D.) was published in the governmental newsletter of the Federation of Rhodesia[566]. In this sense, radiocarbon dating acted as an instrument of equality in history writing: being unrelated to previous archaeological finds and studies, it would not share in pre-existing political or social biases. At the end of his essay, Wagret promoted the potential of radiocarbon dating – and scientific methodologies in general – in the study of history:

> "D'une façon plus générale, la collaboration de ces sciences est susceptible de rendre à nos disciplines

[564] *Ibidem* 364.
[565] *Ibidem* 365.
[566] *Ibidem* 366.

> d'incalculables services. La seule généralisation de l'expérience du test de radio-activité fournirait des réponses précieuses à des multiples questions : par exemple le problème des Étrusques ; il n'est pas interdit d'envisager sa solution pour la seconde moitié de notre siècle."[567]

This optimistic view, which clearly did not take into account the limitations of radiocarbon dating in terms of precision, can be seen as emblematic of the positive approach towards radiocarbon – where this method does not contribute to the discussion, but it offers *the solution to a problem* – that can be considered polar with respect to the skeptical position, of which Milojčić has been taken as example.

4.3.3 Radiocarbon dating in the academic world: people and questions

The academic social context in which radiocarbon dating took its first steps included two very different worlds: the developing field of archaeometry; and the well-established field of archaeology, with its various sub-disciplines and intellectual traditions[568].

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[567] Wagret 1955, 366.

[568] It should be mentioned, however, that geology and oceanography became involved quite early on in the history of the method, as Westgren

It should be mentioned that in the 1950s the radioactive isotope of carbon was mostly used for clinical purposes: searching the bibliographical database Web of Science for the years 1950 -1960, it can be shown that out of the nearly 1200 articles mentioning radiocarbon (or ^{14}C) only around 4% has used this isotope for dating (Fig. 48).

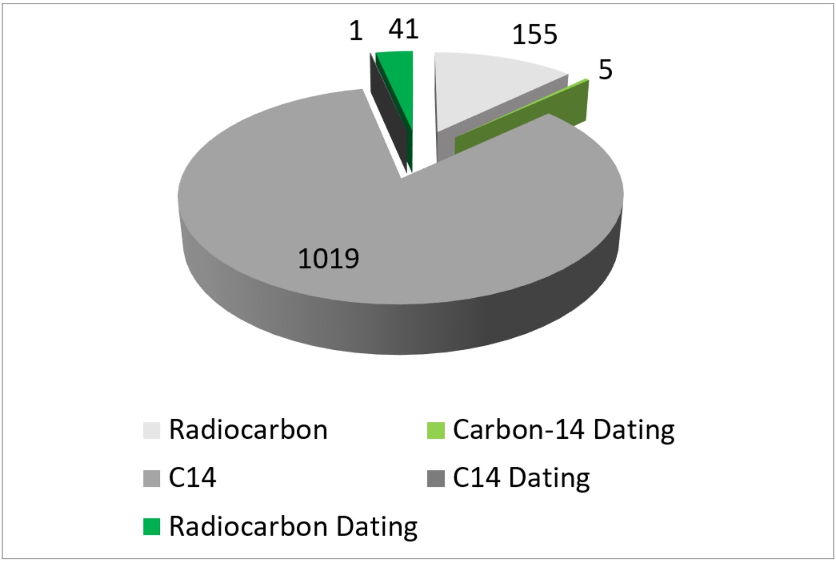

Figure 48 - Theme recurrence in 'Web of Science' bibliographic database 1950-1960.

When considering the chronological applications of the method, the main social actors involved are scientists (either

(1960) pointed out in its introductory speech of Libby's Nobel Prize ceremony. The role of Richard Foster Flint in the "Committee on Radioactive Carbon 14" has been discussed above (*vide supra* 298-299).

chemists or physicists), who measured the radiocarbon residues in samples, and archaeologists who submitted those samples for analysis and used the resulting dates. In 2009 Yaroslav Kuzmin writes:

> "Successful application of ^{14}C dating requires detailed consideration of the many complex requirements which must be met to satisfy the assumptions underlying the method. While some of these requirements are the sole responsibility of *the ^{14}C laboratory*, most are not, and they must be carefully evaluated by the *archaeological user* if reliable chronological information is to be obtained. In particular, the archaeologist must carefully specify the chronological question of interest, and must then carefully identify, choose and evaluate samples which can be expected to reliable answers to those questions."[569] (*emphases added*)

The traditional division of roles – laboratories as producers and archaeologists as users of radiocarbon dates – might appear simplistic and strict compared to today[570]. Producers

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569 Kuzmin 2009, 162.

570 Since the establishment of Archaeological Science programs all over Europe and US, more and more professionals can bridge these two fields. However, this was not the case for the early years of the method and even nowadays this division of roles is still often encountered.

include laboratories – and scholars and technicians working there – but also archaeologists who select samples for analysis and design dating campaign (e.g. for a site, or a region, or a specific chronological problem) with scientific experts. They are the authors of date-lists and first publications of radiocarbon dates. An analysis of *Radiocarbon* articles shows that the number of archaeologists listed as authors only increased substantially after the 1970s, when calibration had been adopted as a routine process. The number of archaeologists among the producers, however, has remained lower than that of scientists. A certain pre-eminence of chemists and physicists, followed by geologists and anthropologists, seems to be evident also from the types of departments radiocarbon laboratories were affiliated with, between 1959 and 1965 (Fig. 39a-b).

Radiocarbon dating entered different intellectual and social contexts and left its mark on all of them. At the same time, they had an impact on radiocarbon or at least on the questions asked of the method. An analysis of the articles listed by Frederik Johnson in his first bibliography of radiocarbon dating, published in the first issue of the journal *Radiocarbon* (1959), provides some insight on the matter. As early as 1959, samples coming from all five continents had been analysed, but a large majority of them came from North America (Fig. 49).

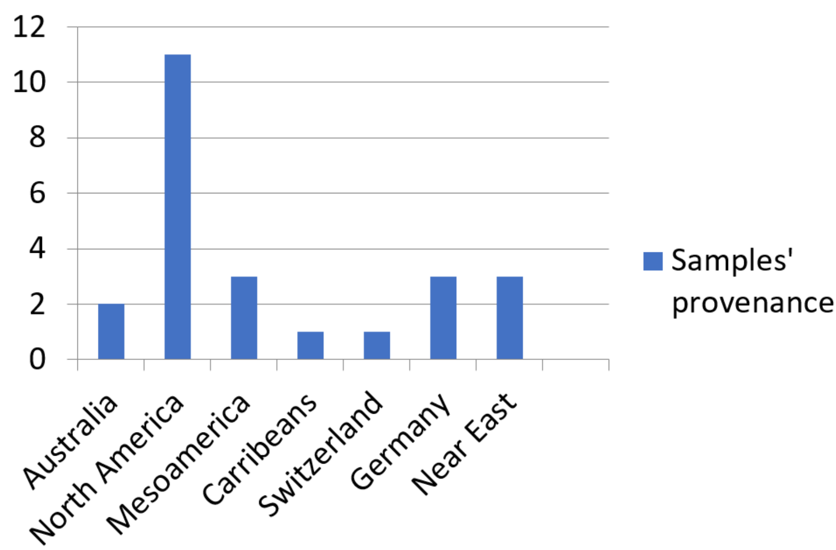

Figure 49 – Radiocarbon sample provenance according to Frederick Johnson's (1959) publication.

Some periods were also more investigated than others: looking at the sample expected age, it becomes evident that both the Neolithic and the Palaeolithic were periods of great interest to the radiocarbon community.

4.4 Conclusion

In conclusion, we observe that the major achievement of Libby's theory was one of independence and unification. One might erroneously think that the academic and historical conditions under which dating methods were generated are irrelevant to current praxis of the discipline. On the contrary, the contextual analysis of such a prevalent method can provide a useful tool for understanding the subsequent attitudes of different scholars and archaeological trends towards the method itself. Indeed, some chronological problems appear to have been more linked to the use of this method – to its affordances and limits – than others. This is, for example, the case for the definition of the Middle to Upper Palaeolithic transition: the complexity of this long-standing chronological controversy cannot be understood (nor resolved) without a careful consideration of the history of radiocarbon dating and the way it was used by different scholars[571].

Hopefully, through this attempt at a social and intellectual history of radiocarbon dating, it is possible to show that many factors contribute to the development of dating methods and that many historical and cultural elements affect the formation of chronological controversies.

Chronology and chronometry have a complex history that cannot be reduced to the implementation of new techniques: the perceived reliability, the intellectual traditions they refer to, and the value they assume in the social place are all relevant to the questions asked of them and, therefore, influence the results delivered.

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571 *Vide supra,* 326-355.

Conclusions

> "La competenza dello storico si riconosce da ciò che egli non dà per certo quello che è dubbio e non generalizza il caso isolato. In taluni casi lo storico deve dire: non capisco. In altri avventurerà con esitazione una ipotesi. Ma non basta che una ipotesi sia plausibile. L'ipotesi avanzata deve essere più plausibile di ogni altra ipotesi. Prima di proporre una ipotesi lo storico deve fare lo sforzo di cercare e valutare alternative ipotesi."
>
> Arnaldo Momigliano, Le regole del gioco nello studio della storia antica, in *Sui fondamenti della storia antica*. Torino: Einaudi

The aim of chronology as a historical discipline is to locate ancient events, artefacts and contexts in time, specifically in (one of) our modern system(s) of time measurement. Through this process we establish their distance to the present and build the foundation for understanding from a diachronic perspective. There is no general rule to resolve chronological controversies, as each instance constitutes a specific historiographic problem.[572] And yet, some

[572] This notion of chronology is an adaptation from Bickerman (1963, especially 83-84). Dealing with ancient historical chronology, he was only concerned with the 'reduction' of ancient dates to modern chronographic systems, but his considerations can be easily be adapted to a wider concept of chronology.

principles can be extrapolated from the case studies examined here.

The four chapters of this dissertation investigated the social, intellectual and historical conditions under which chronology was constructed. The case-studies were used to highlighting the impact of explicit and implicit theoretical constructs on the invention, selection and adoption of dating methods.

Political and ideological concerns – as well as methodological habits and discipline dependent preferences – were shown to be at work in many chronological controversies.

It appears that theoretical assumptions - and sometimes ideological positions - can hide in the units selected for ding (e.g. the type, the class, the technocomplex), in the variables that we deem time sensitive (e.g. shape and decoration for Proto-Corinthian pottery), or in the distance between the dated event and the target event.

How is this useful to archaeological praxis? While it may be naïve to think that self-awareness is the key to defeating all biases, the exercise is not futile. Indeed, the historian – as Momigliano writes – needs to make an effort to evaluate different hypotheses and to select 'the best one' to make sense of the gathered evidence. Analysing the units, the variables, the models and the theories that underpin our inferences allows us to think of new questions and to formulate new hypotheses.

The history of archaeology is, in the intentions of the author, a heuristic tool.

The following Appendix gives an example of the impact that this line of research can have on dating methods and research praxis. In the Appendix, the authors identify the units, variables, assumptions and generalisations embedded in the radiocarbon dating method. And they show how in-depth analysis can produce useful heuristic tools for integrating old and new results. A similar approach can be used with other dating methods, developing tools for the interoperability of results and the critical understanding of archaeological time.

Appendix

This appendix is the result of research conducted with Rachel Hopkins at the RLAHA (Research Laboratory for Archaeology and the History of Art) at the University of Oxford. Here I present selected material relevant to this dissertation, especially to Chapter 1 and Chapter 4. A revised and extended version of this text has been submitted for publication. Rachel Hopkins is the author of all the images, with the exception of the flow chart, which is the result of team work.

Working with 'old' radiocarbon dates. Guidelines for archaeological applications

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&

Rachel J. A. Hopkins

1 Introduction

Ever since its invention, radiocarbon dating has been a crucial tool for archaeological investigation and interpretation, while simultaneously being the object of major debate among archaeologists (e.g. Delley 2015, pp. 95-114; Shanks and Tilley 1987, pp. 118-136). In fact, the robustness of the physical and chemical principles underlying the method has earned it great authority and led to a Nobel prize in Chemistry being awarded to W. F. Libby in 1960. However, the increasing numbers of ‘revolutions’ in understanding and application of radiocarbon dating has raised concerns in the archaeological community regarding the reliability of ‘old’ radiocarbon dates, i.e. previously published measurements, often obtained in the early years of radiocarbon dating.

It is true that – like most scientific methods – radiocarbon dating is continuously being improved: researchers constantly strive to better understand the mechanisms involved in altering radiocarbon concentrations (e.g. sources of contamination) and to develop new techniques accordingly, which may increase the accuracy, precision and reliability of radiocarbon results. For this reason, incorporating ‘old’ dates into new research can be problematic. However, the costs associated with obtaining new dates, together with the destructive nature of radiocarbon analysis, makes it sometimes impossible to repeat the dating for a new study. In such cases, carefully evaluating the inherited radiocarbon measurements might provide fruitful new insights with relevance to answering archaeological questions.

This article aims to provide some guidelines for archaeologists working with such ‘older’ datasets, by providing a brief overview on radiocarbon dating and trying to illustrate under which conditions previously obtained radiocarbon measurements can be reused in new research.

2 Key concepts of radiocarbon dating

In order to allow a thorough understanding of the guidelines to be presented in this article, some key concepts regarding the scientific language used and radiocarbon dating in general are presented below:

2.1 Accuracy & Precision

Accuracy is a concept used to assess the proximity of the measurement to the real date; it is also defined as *trueness* by the International Organization for Standardization. In radiocarbon dating, accuracy can only be measured for samples of known age (JCGM 2008, pp. 21-22).

Precision is a measure of statistical variability. It refers to the closeness of two or more measurements of the same sample. A radiocarbon measurement is considered more precise when the error is smaller (JCGM 2008, pp. 21-22).

Even though each scientist would love to perform high accuracy and high precision measurements, these two values are independent and can sometimes be conflicting (see fig. 1). For example, two measurements of one sample may be close to the known age of the sample itself while being far apart from each other. In this case, the measurement would have high accuracy and low precision. On the other hand, when the results are coherent but far from the known value, the measurement would have high precision and low accuracy.

2.2 Validity & Reliability

Validity is a concept used to assess the consistency between the targeted event and the dated sample. It often happens that samples are dated in order to establish the age of a phenomenon different from the sample itself. The temporal interdependence between the sample and the investigated phenomena gives an idea of the validity of the measurement (Ramenofsky and Steffen 1998, p.9).

Reliability is a concept used to assess the performance of a method in terms of precise and accurate measurements. In order for a method to be reliable, different measurements will produce comparable (i.e. consistent) results (Ramenofsky and Steffen 1998, pp. 8-9).

2.3 Principles of Radiocarbon Dating

The principles of radiocarbon dating were established by Arnold and Libby (1949) and the method is based on the radioactive decay of ^{14}C in the sample of interest. For those with a more in-depth interest in the physics see Bowman (1990) and Aitken (1990).

Radiocarbon cycle (see fig.2). Radiocarbon is generated in the upper atmosphere, where cosmic rays produce neutrons, which in turn react with nitrogen (^{14}N) to form ^{14}C. Like the stable isotopes of carbon (^{12}C and ^{13}C), radiocarbon reacts with oxygen. The resulting CO_2 is subsequently incorporated into terrestrial plant life through photosynthesis, from where it disseminates through the food chain. In the case of marine or fresh-water organisms, the CO_2 from the atmosphere has to be initially absorbed by the water, before it dissipates through the food chain. During formation or renewal of tissue, a dynamic equilibrium with the atmosphere is maintained, i.e. radiocarbon is steadily replenished. Renewal stops when the organism dies, or the tissue formation is completed (e.g. formation of a tree ring). While the two lighter isotopes are stable, radiocarbon decays and its concentration reduces over time.

Radioactive decay*.* ^{14}C is radioactive and progressively decays to ^{14}N. Consequently, if radiocarbon is not replenished, the concentration of ^{14}C decreases over time at an exponential rate. Libby calculated the radiocarbon half-life to be 5568 ± 30 years (Libby 1952): this is the time interval needed for radiocarbon concentration to decrease by one half; the remaining concentration will take another 5568 ± 30 years to decrease by 50% once more, and so forth. Knowing the half-life of radiocarbon and the original concentration in the sample, it is possible to calculate when the selected sample stopped being in dynamic equilibrium with the atmosphere. In 1962, H. Godwin established the more accurate value of 5730±40 years for the radiocarbon half-life (Godwin 1962), currently known as the Cambridge half-life. Nonetheless, Libby's half-life continues to be used for the calculation of the "conventional radiocarbon age" to maintain comparability between ^{14}C dates.

Fractionation. ^{13}C and ^{14}C are slightly disadvantaged by photosynthetic and metabolic processes with respect to the lighter and more prevalent isotope ^{12}C. Consequently, their isotopic ratio will differ somewhat in living tissues compared to their ratio in the atmosphere, causing an overestimation of the sample's age. However, this variability can be overcome by measuring $\delta^{13}C$ (the ratio between the two stable isotopes ^{13}C and ^{12}C) in the investigated sample and normalising to a standard value (i.e. the postulated mean value for terrestrial wood).

Reservoir effect. Some environments have a radiocarbon concentration that differs from the atmosphere. This is especially the case for deep ocean water and rivers flowing over limestone. In these environments, ^{14}C concentration is often significantly lower than in the atmosphere. Therefore, organisms incorporating marine or fresh-water carbon, as well as animals higher up in the food chain, will appear older than they actually are. If the offset is known or can be estimated, it can be corrected for. It is important to note, though, that the reservoir effect can show significant local variation, requiring different correction factors depending on the reservoir, geographic region and time period of interest. For this purpose, datasets are being created measuring known age samples from different environments, regions and time periods to calculate their radiocarbon offset. When correction is applied, the age obtained should be referred to as a "reservoir corrected radiocarbon age".

2.4 Radiocarbon Revolutions

Major advancements in the field of radiocarbon dating have been regarded as 'revolutions' because of the disruptive body of knowledge they generated (Bronk Ramsey 2008; Bayliss 2009). Three of them are of particular interest for the aim of this paper.

Calibration. Less than two decades after the inception of radiocarbon dating, it became apparent that the concentration of ^{14}C in the atmosphere has not remained constant over time, so that the calculated age could diverge from the true age of the sample on the order of several hundred years. During the 1960s these variations started being systematically accounted for by measuring wood samples previously dated by dendrochronology (Suess 1967). Since then, various calibration curves have been produced – and are still being developed – in an attempt at increasing dating accuracy. Since IntCal09, the calibration curve extends beyond the dendrochronological record and covers not only the Holocene, but also the Palaeolithic up to the limit of the technique (ca. 50,000 BP). At the time of writing this article, the newest internationally recognised calibration curves are IntCal13 (Reimer et al. 2013), for the terrestrial record of the northern hemisphere, SHCal13 (Hogg et al. 2013) for the terrestrial record of the southern hemisphere and Marine13 (Reimer et al. 2013) for the global marine record.

Accelerator Mass Spectrometry (AMS).The first device used for counting radiocarbon atoms was a modified Geiger counter, built to measure ionizing radiations from the decay process. Shortly afterwards CO_2 decay counters were developed, but they were soon replaced by beta counting methods (especially Liquid Scintillation Counting), which some radiocarbon laboratories still use today. This method does not measure ^{14}C atoms directly. Instead it measures beta particles emitted during the decay of ^{14}C to ^{14}N. This technique requires a sufficiently large amount of radiocarbon (and, consequently, of sample) as well as time in order to obtain a precise measurement. In the 1980s, radiocarbon dating by AMS became available. AMS directly measures radiocarbon isotopes by separating them according to their atomic mass, thus allowing for sample sizes, background level and measurement time to be drastically reduced.

Bayesian Modelling. In the early 1990s researchers began using Bayesian modelling to combine radiocarbon determinations with archaeological information (e.g. stratigraphic relationships) to create statistical models that improved dating precision, assessed dating consistency, and enabled the evaluation of previously undatable phenomena (e.g. duration of occupation, start and end of a phase, etc.). Unmodelled radiocarbon dates represent the likelihood of when single events took place independent of each other. When dealing with several radiocarbon measurements, archaeological (='prior') information can be used to constrain these probability distributions, excluding impossible scenarios and increasing the likelihood of scenarios consistent with the archaeological information. The Bayesian model adjusts the probability distributions of each event accordingly, resulting in modelled radiocarbon ages (i.e. 'posterior' information). In current research, the majority of prehistoric chronological frameworks are built through Bayesian modelling.

3 Assessing radiocarbon dates

3.1 Step 1: Preliminary information (fig.3a)

Radiocarbon dating measures the ratio between ^{14}C and ^{12}C and then converts it into a radiocarbon age by using the Libby half-life and correcting for fractionation. Calendar years are subsequently obtained through calibration and further mathematical corrections where necessary. It is these adjustments (calibration and correction factors) that form one of the three areas in which major improvements have been achieved since the invention of the method. The other areas include contamination removal prior to dating (i.e. pre-treatment strategies) and sample selection (i.e. our understanding of what event is represented by the radiocarbon measurement). The dating of cremated bone is an illustrative example of our advances in understanding: it has been recently pointed out that the radiocarbon date obtained from well cremated bone represents to a much larger extent the radiocarbon ratio found in the fuel used for cremation rather than the death of the individual (Snoeck et al. 2014). Despite recent improvement, 'old' measurements, expressed either as $F^{14}C$ or as conventional radiocarbon ages, are sometimes compliant enough with modern standards to be fruitfully incorporated into new research. In these cases, it is a matter of updating calibration and corrections using contemporary methods. Therefore, when dealing with 'old' radiocarbon dates, it is crucial to obtain the necessary information related to the radiocarbon measurement itself as well as to the archaeological context of the dated sample in order to assess their quality. This information can be retrieved from multiple sources:

- Publications. Date lists are regularly published by laboratories: while in the early years of radiocarbon dating this was mainly in the journal *Radiocarbon*, nowadays the dates are often published online in databases maintained by the various laboratories. Radiometric information can often be found in the appendices or supplementary information of archaeological articles and monographs. Moreover, the results of radiocarbon analysis are often discussed in dedicated journals, especially in *Radiocarbon*, but also in *Archaeometry* and the *Journal of Archaeological Science.*

- Laboratory archives. Laboratories often keep a detailed record of their treatment procedures, either on paper or, more recently, digitally. Most laboratory archives are not publicly accessible and require contacting laboratory staff for further information. However, some exceptions exist, e.g. ORAU (University of Oxford), which lists all published radiocarbon dates measured at their lab in an online database and also provides radiocarbon databases for completed research projects (RESET, INTIMATE, Egyptian radiocarbon). To facilitate your search, a useful source of information can be the code of a radiocarbon date. It indicates the laboratory where the measurement was done: a complete list of former and current laboratories and their codes is available on the website of *Radiocarbon* (http://www.radiocarbon.org/Info/lablist.html). Furthermore, the code may provide additional information on counting methods and the confidence in the date at time of measurement.

- Excavation records and journals. When the archaeological context of samples is not sufficiently discussed in a publication, excavation journals and, more recently, GIS and 3D records of the site can provide helpful information on their spatial and functional position in the site.

- Online databases. With an increase in research based on large datasets, it has become more common to make the collated information available online. These databases can provide a useful overview and should list additional information for individual radiocarbon measurements. However, they are not always updated and maintained, and their content has to be verified using the primary literature. Some of the most useful databases are: ^{14}C Database for Southeast Europe and Anatolia (14SEA Project); Radiocarbon CONTEXT Database; Stage3 Databases; Louisiana Radiocarbon Database; Radiocarbon Palaeolithic Europe Database; The Canadian Archaeological Radiocarbon Database (CARD); Wales and Borders radiocarbon database; Radiocarbon dates online (RADON); Scottish Radiocarbon Database.

3.1.1 Radiocarbon Measurement

Initially, information on the radiocarbon measurement itself needs to be gathered. It is both crucial to distinguish a conventional radiocarbon age (or fraction modern value) from the results of later conversions (e.g. calibration, reservoir correction) and to verify that the measurement is compatible with modern dating standards. The following data is required for an assessment:

1. <u>Conventional radiocarbon age, or F^{14}C</u>

There are different ways in which radiocarbon measurements were reported. Most commonly found in the archaeological literature are:

a. *Conventional radiocarbon age* (e.g. 3750 ± 35 BP). This value expresses the conversion from the isotopic ratio to a standardised measure of age, which makes different dates comparable while giving an immediate indication on the time passed. According to current reporting conventions, a conventional radiocarbon age fulfils the following conditions:

- It is calculated using Libby's half-life
- It is fractionation corrected (using $\delta^{13}C$)
- It is expressed in years before Present (BP), using 1950 as conventional present
- It is not corrected for reservoir effects;
- It is not calibrated.

It is important to note that, albeit a first attempt at normalizing the reporting conventions was made in the 1970s by Stuiver and Polach (1977), the above-mentioned norm was only fully implemented in the mid-1980s. When dealing with radiocarbon measurements obtained before then, it is necessary to check whether these conditions are met or, in cases where they are not, whether sufficient information is known to calculate the conventional radiocarbon age. Moreover, minor modifications of reporting conventions were introduced even later (Mook and van der Plicht 1999; van der Plicht and Hogg 2006; Millard 2014) and may be worth checking.

b. *Calibrated age* (e.g. 2815–2745 cal BP). This value takes into consideration the variations of ^{14}C concentration in the atmosphere over time and places the radiocarbon age on a common time scale with other dating methods, using the calendar year as the unit of time. As a result of continuous improvements in the understanding of atmospheric radiocarbon variations, a calibrated age – unlike the original radiocarbon measurement – is not a fixed value and requires updating. The basis for such work is the "conventional radiocarbon age" or $F^{14}C$ value. Either can be calculated from a calibrated age if the following is known:

- Calibration curve (see Reimer et al. 2009 for an overview of available calibration curves prior to the widespread adoption of IntCal09);
- Software.

c. *Calendar age* (e.g. 1560 ± 35 BCE or 1595-1525 BCE). This is the least standardised use of a radiocarbon measure. When a radiocarbon age is reported as a historical date without any "cal", it is often the result of subtracting the "present" of 1950 from the radiocarbon age. This method of obtaining a calendar date is nowadays scientifically unsound and should under no circumstances be used in modern publications. However, where such a procedure can be identified, obtaining a conventional radiocarbon date might be as straightforward as adding 1950. In other cases, the age might be the result of additional corrections and adjustments. For example, sometimes the age range is given in 'cal BC' or 'cal AD'. In these cases, the calendar age has been obtained using a calibration curve and the date should be treated as a calibrated age described above. Corrections and adjustments used are not always explicitly stated in the literature. To reverse engineer the conventional radiocarbon age, the following knowledge is required:

- *Half-life.* Has the Libby or Cambridge half-life been used?
- *$\delta^{13}C$ and fractionation*. What is the $\delta^{13}C$ value? Has it been used to correct for fractionation?

- *Reservoir correction*. If the sample has been corrected for a reservoir effect, what dataset was used?
- *Calibration*. If the date is calibrated, what calibration curve and software was used?

d. *$F^{14}C$* (e.g. 0.7809 ± 0.0029). The scientific literature often refers to 'fraction modern carbon' ($F^{14}C$) or 'percent modern carbon' (pMC) instead of radiocarbon ages. Although this reporting convention is preferred by most physicists and radiocarbon specialists, it remains rare in the archaeological literature, due to its not directly visible relationship to calendar years – the preferred measure of time in archaeology and history. Fraction modern carbon values allow easy conversion to conventional radiocarbon ages using any of the standard software packages available: some of the more commonly used are OxCal from the University of Oxford, BCal from the University of Sheffield, and CALIB from the University of Washington. The reporting convention is mathematically well defined (Reimer et al. 2004) and the values are:

- fractionation corrected
- not reservoir corrected
- not calibrated
- reference for modern carbon concentration is 1950, where value is = 1

e. *Modelled age* (i.e. 3520-3450 modelled BP). Recently, this new type of radiocarbon based age determination is gaining importance in the literature (e.g. Bayliss 2009). A modelled age is the outcome of a statistical model applied to several radiocarbon ages. It can represent one of two types of dates: (1) a posterior probability distribution for an individual radiocarbon age, i.e. the probability of an event happening taking other (='prior') information into account (=an 'updated' radiocarbon age), or (2) a probability distribution of an individual event/phenomenon taking place, that cannot be directly radiocarbon dated (e.g. time of appearance of a certain culture on a site, disappearance of an artistic style in a region, etc.). Such models can be applied to local stratigraphies or to regional and pan-regional phenomena depending on the archaeological question. In order to use a modelled age from the literature for new research, the model as well as the dates used need to be evaluated and possibly updated according to the newest research. The following information requires assessment:

- Calibration curve
- Modelling software or algorithm
- Model code

2. <u>Errors</u>

Radiocarbon dates are either accompanied by a ± sign followed by a figure or reported in the form of an age range. They indicate the time interval when the actual date is more likely to have occurred. A radiocarbon date cannot be used with any confidence without knowing what exactly the error stands for (for those with a more in-depth interest in statistics see Drennan 2010). Different errors can be associated with different kinds of ages:

a. *Conventional radiocarbon age and $F^{14}C$.* In conventional reporting the error represents 1 standard deviation (SD) from the mean. This means that the actual date will be found within the ± range in 68% of the cases. While $F^{14}C$ is always reported with an error of 1 SD, conventional radiocarbon ages can also be found in the literature – against reporting convention – with an error representing 2 SD (95%)

or more rarely 3 SD (99%). Furthermore, the positive and negative error (deviation from the mean), although symmetric for $F^{14}C$ values, is asymmetric for conventional radiocarbon ages as a result of the exponential decay. Different laboratories have resolved to varying reporting conventions: for example, Groningen reflects the asymmetry in reporting separate positive and negative errors for each radiocarbon date where applicable; Oxford on the other hand standardises to symmetric errors for conventional radiocarbon ages. These deviations are larger the older (i.e. further away from BP) a radiocarbon age determination is.

b. *Calibrated radiocarbon age.* The time interval reported for a calibrated radiocarbon age is normally, though not always, associated with a 95% probability of the actual date falling in the reported age range. However, the probability distributions for calibrated ages are not normal (i.e. they do not follow a Gaussian distribution). Therefore, a date closer to the average is not necessarily associated with a higher probability of being true. This is often visualised in a graph representing the probability distribution function (see fig. 4). In order to compare and reuse a calibrated age the error range must be verified:

   - Calibrated age range reported at 1 SD (68%), 2 SD (95%) or 3 SD (99%)?

c. *Modelled age.* Similar to calibrated radiocarbon ages, the error is visualised by giving an age range – normally at a 95% confidence interval, more rarely at 68% or 99%. This is done to better account for the non-normal probability distribution. As above, the probability range covered has to be verified before a modelled age can be meaningfully used.

3. <u>Material characteristics of the sample.</u>

a. *Material dated.* Radiocarbon measurements can be performed on a wide range of materials: wood, charcoal, bone, antler, ivory, paper, teeth, hairs, leaves, seeds, shells, and other organic remains. However, different materials require different pre-treatment methods, might need specific corrections, and show different relationships with the associated archaeological material or the archaeological question in mind (Taylor 1987, pp.39-69). Therefore, it is crucial to know what material was dated and what characteristics said material has.

b. *Single entity or bulk sample (containing multiple entities).* It is important to understand whether the radiocarbon measurement was carried out on a single entity or a bulk sample. In the first case, the date is obtained on a single sample (e.g. seed, piece of charcoal). In the latter, several organic remains from the same archaeological context are mixed to form a sample, that incorporates different radiocarbon ages (e.g. sediments, several charcoal fragments). Whether a single entity or a bulk sample was dated, was not always a deliberate decision. More often it was a matter of size, i.e. the amount of sample needed to obtain a radiocarbon date. It was only with the introduction of AMS, that smaller samples such as seeds became datable and consequently, dating single entities became more common. Single entity measurements should always be preferred to avoid dealing with fictional dates that are the product of different radiocarbon ages, but actually represent none of them (Ashmore 1999). As an example, take two charcoal fragments from the same fireplace. For this example we assume that they are both from fresh wood cut down shortly before burning them, but the fireplace has a long continuity of use and the two charcoals might be representative of any one of the many fires burnt. Dating them as one sample will give a radiocarbon date that lies somewhere in-between the two actual dates of the charcoals – and refers to neither of them, thus remaining archaeologically meaningless. Bulk sample dates can only provide some age indication in cases where there is well founded reason to think that they originally belonged to the same artefact (e.g. bone fragments with coinciding fractures), where it can be reasonably assumed that the dating error is larger than the differences between the dates mixed, or where the archaeologically required resolution is lower than the dating resolution. If none of these three conditions are fulfilled, it is preferable to discard measurements from bulk samples in favour of those done on single entities.

c. *Conservation.* Archaeological remains are often subjected to treatments intended to repair them or prolong their life (conservation and consolidation). This is especially the case if a sample is deemed precious (e.g. human remains, artefacts). Furthermore, the longer the time between excavating and radiocarbon dating a sample, the more likely conservation treatments become. These treatments add contaminants that can alter the result of the radiocarbon measurement. Consequently, it is important to know whether a dated sample had been conserved in the past and what chemicals might have been used (while current conservation methods normally – though not always – follow international standards to be found in handbooks such as Rodgers 2004, it is often very difficult to retrieve older conservation recipes). This will help assessing the suitability of the pre-treatment protocols applied to remove contamination from the conservation environment.

4. Year of measurement and laboratory protocols

In radiocarbon dating laboratory protocols, i.e. the steps needed to obtain a date from a sample, have significantly changed over time, but also often vary between laboratories. As a result, the dating quality is dependent on the pre-treatment and measurement protocols used. Some of the major radiocarbon laboratories in the world, have published their pre-treatment protocols (Brock et al. 2010; Reimer et al. 2015). In cases where they remain unknown, the year in and the laboratory at which a radiocarbon date was obtained can help to determine whether the 'old' date provides any constructive indication of age.

a. *Pre-treatment.*

- Solvent wash. In the case of conserved samples, it is important to know whether the sample has been chemically treated to remove the contamination caused by conservation and consolidation products.

- Extraction of carbon fraction. Various pre-treatment methods have been put into place to chemically separate the exogenous carbon (= contamination) from the endogenous carbon (= carbon original to the sample) portion of the sample. Knowing if a sample has been pre-treated, and which protocol has been used, is a key requirement in order to be able to assess the quality of the measurement.

b. Measurement method

- The technological differences between decay counting (Taylor 1987, pp.71-95) and AMS (Fedi 2009) might have an impact on the error associated with the analysis, as well as on the kind of material required for the measurement.

Therefore, knowing which counting method has been used can be beneficial to identifying possible issues surrounding, for example, sample size and contamination/mixing, or measurement background levels and the age beyond which radiocarbon dating is no longer possible.

3.1.2 Archaeological context

Finally, the suitability of a radiocarbon date not only depends on its measurement, but also on its archaeological context and how said context relates to the archaeological question in mind. The more is known about a sample's provenance, the easier can the validity of the date be assessed.

1. <u>Site of provenance and stratigraphic unit.</u> It is crucial to have information on the spatial coordinates of the sample in the site stratigraphy. However, stratigraphic units were not always documented in a standardized fashion previous to Harris' publication in 1975, and not every site recorded the absolute co-ordinates for each sample collected. Therefore, the level of detail required heavily depends on the archaeological question that is being asked: the higher the expected dating precision, the more comprehensive the provenance information has to be. Moreover, it is crucial to evaluate the likelihood of the sample being in his primary or secondary deposition.

2. <u>Year of collection.</u> Knowing when a sample was excavated (and possibly by whom) can be tremendously useful. It can help assessing the reliability of excavation techniques and the reported archaeological information, while giving an indication on the meaning of the terminology used to describe it. Furthermore, it

provides a chance to directly access the excavation documentation – which often contains more detailed information than subsequent publications. This is especially true in the early days of radiocarbon dating when the technique was seen as a means to a date and the information on sample and laboratory treatment were kept to a minimum or even omitted.

3. Taphonomic and geological information. An understanding of the taphonomy and geology of the archaeological site is necessary to evaluate possible contaminations from the burial environment, the samples position in the stratigraphy and to understand the compliance between the dated material and the target event.

4. Associated material and original research questions. The spatial and functional relationship between the dated material and other archaeological remains associated with it indicate whether the radiocarbon date is indeed able to provide data relevant to the current research question. Often the assessment is aided by understanding how the original samples were chosen for dating in the first place and what research question they were intended to address. A date can be perfectly reliable from a measurement perspective and come from a well-established archaeological context, but it may still be deemed unsuitable as it is not sufficiently related to the current research question.

3.2 Step 2: Validity Assessment (fig. 3b)

Once the data described in the previous section has been gathered, the quality of the radiocarbon measurement and its suitability for current research can be assessed. If reliable information as outlined above (see step 1) is not forthcoming, assessment becomes increasingly difficult to impossible. In such cases it is recommended that the date is set to the side and not used in research publications.

The quality assessment of the laboratory process itself will be discussed under step 3, as it becomes obsolete if a radiocarbon measurement – no matter how reliable – is not adequately related to the archaeological question at the heart of the research. The following section focuses on the relationship between the archaeological question and the radiocarbon measurement. The validity of this depends on two overarching factors: certainty of association and inbuilt age.

3.2.1 Degree of Certainty

First, we evaluate the degree of certainty of the association between measured radiocarbon age of a sample and the archaeological event of interest, after the work of Waterbolk (1971; 1983).

[1] Full certainty. In the best case scenario, the sample directly represents the event of interest, so that we have full certainty about the association. Examples are: human bones to date their presence at a site, or artefacts to date their time of manufacture.

[2] High probability. A high probability of connection between the sample and the event of interest can be concluded when they are linked by a functional relationship. Examples are: food residues to date the use of the vase in which they were found, or wooden beams analysed to date the construction or renovation of a house.

[3] Probability. A lower degree of probability has to be attributed to associations that only rely on the coexistence of the dated sample and the targeted material/event in the same occupation layer. A distinction ought to be made between the cases in which the closed

context, the quantity of organic material and the size of fragments argue in favour of a relationship, and the cases in which this relationship is much looser. As an example: charred wheat is often used to date the destruction of a settlement; yet the ^{14}C measurement in this case gives a date for a time the settlement was used and therefore only a *terminus post quem* for its destruction.

[4]Possibility. Finally, when a low quantity of organic material is associated with other remains in an open context, there is only a small likelihood of the measured sample and the targeted event being adequately related.

It goes without saying that full certainty in association is at all times preferred. In reality, samples from the second or third category may be usefully applied, especially in association with other dates and well-developed models. If the date falls under the fourth category, its result is more likely to be misleading than constructive.

3.2.2 *Inbuilt Age (fig. 5)*

Secondly, the inbuilt age of a sample needs to be quantified. 'Inbuilt age' refers to the time elapsed between the moment when the individual components of a dated sample stopped being in dynamic equilibrium with the atmosphere (e.g. end of material formation) and the time of the targeted event.

As such, inbuilt age results from the combination of a **bio-chemical** and a *post mortem* component (such a distinction, often implied in the literature but seldom explicitly defined, can be found in McFagden 1982). The first relates to the rate at which carbon is renewed in different organic material (i.e. radiocarbon turnover) and the way it is stored in the sample. The distinction between short-lived and long-lived samples can be described as follows:

1. Short-lived samples (< 20 years). This is the case for seeds, leather, and bones. As an example, a human bone has a turnover time of around 7 years (though variances between different bones exist), i.e. its carbon component is fully renewed approximately every 7 years during a human's life (Hedges et al. 2007). In contrast, a seed is produced in less than a year. In both cases, the radiocarbon age dates to slightly before the death of the organism, though in the first scenario this will lead to a larger offset than in the second.

2. Long-lived samples (>20 years). They can have an inbuilt age of several decades and even centuries. This is, for example, the case for wood and consequently charcoal. Tree-rings grow annually and their dynamic equilibrium with the atmosphere stops whenever growth is completed. Therefore, each tree-ring accounts for the year of its growth and samples from the same tree, depending on their relative position, can provide diverging radiocarbon measures. When available, wooden samples from the outermost tree-rings tend to be selected for dating, because they grew just before the tree was cut and give a date that roughly coincides with its death. However, more often the available sample comes from the heartwood, which, depending on species and size of the tree, can date to centuries before the tree was cut.

A similar distinction between short-lived and long-lived sample can be made in relation to the ***post mortem*** component of the inbuilt age – which is typically the main contributor to inbuilt age. This part should not be confused with the 'degree of association' discussed previously, as it is not a measure of the soundness of the relationship between two events, but of their distance in time.

3. Short-lived sample (<20 years). The exchange of carbon with the atmosphere (e.g. death, end of material growth) has stopped less than 20 years prior to the targeted event. The targeted event may be the realization of an artefact. In this case a short-lived sample is an artefact produced shortly after the death (or end of material growth) of the organic material. This is often the case of ordinary artefacts made of widely available short-lived organic materials, such as bone points, leather containers, or wicker baskets. If the targeted event is burial, the sample has been stored or was in circulation for less than 20 years prior to deposition. This could be a bone awl that broke during manufacture and got discarded at the workshop, or residue from fresh food buried with the deceased.

4. Long-lived sample (>20 years). The exchange of carbon with the atmosphere has stopped (e.g. death, end of material growth) more than 20 years prior to the targeted event. If the targeted event is the realization of the dated artefact, longevity can be caused by various factors. Apart from having a large bio-chemical inbuilt age as described previously, the organic material itself may be precious, treasured for a long time before being used, or may be fossilised or frozen material, found and used long after its death. For instance, mammoth ivory can still be found in permafrost regions today in a condition suitable for reworking. Its bio-chemical inbuilt age is that of bone and therefore low, yet if the material has been worked decades, centuries or even millennia after the death of the animal, the human action cannot be dated by measuring the formation of the mammoth ivory in question. When the targeted event is the

deposition of the item, it is important to consider that while seeds, leather and bones show a small bio-chemical inbuilt age, it might take significant time for them to end up in the archaeological record. A decorated bone comb, for example, may be handed down generations before it is finally deposited in a grave. The so-called "old wood" effect is an emblematic case of large inbuilt age (Schiffer 1986). Part of it can be the large bio-chemical inbuilt age of wood, as described earlier. However, it has also been suggested that wood can be stored for decades before use, and is frequently recycled (e.g. in architecture), increasing the time between wood formation and human action of interest or burial.

Although each case presents its own set of unique conditions, we recommend to only consider samples whose combined (i.e. bio-chemical and *post mortem*) inbuilt age is small, and whose degree of certainty falls within the first three categories (with the first being most optimal). If these conditions are not fulfilled, a radiocarbon date might still be considered valuable as a boundary function, i.e. a terminus *post* (or *ante*) *quem,* in certain circumstances. Overall, the possible dating error added as a result of inbuilt age and degree of association has to be smaller than the dating resolution required to answer the archaeological question. This also applies if modelling of a set of radiocarbon dates is intended. In fact, inbuilt age can be integrated in Bayesian models (Dee and Bronk Ramsey 2014; Fedi et al. 2015), though if the error caused by it is not quantifiable, the resulting modelled ages will be misleading, showing high precision, but hiding their poor or even decreased accuracy.

It might have become apparent that a (single) radiocarbon date is not a suitable means to date an archaeological layer. In fact, a radiocarbon age gives a date for a specific event – the interruption of the interchange between the measured sample and the atmosphere – which is usually older than the date for the deposition of the dated sample. In contrast, the archaeological layer encompasses multiple, sometimes independent, events and represents a duration (e.g. duration of material accumulation). If the duration of occupation – as manifested in the archaeological layer – is of interest, an answer can only be obtained through Bayesian modelling and the use of several radiocarbon dates from within said layer. Ideally, further dates from different stratigraphic units with a known relationship to the layer of interest will be included in the model in order to establish a likely upper and lower boundary for the period in question, thus better understanding the possible start and end date of the occupation (Buck et al. 1994).

3.3 Step 3: Measurement and Treatment Assessment (fig. 3b)

At this stage, the radiocarbon dates should have passed both the information collection stage as well as been favourably assessed regarding their archaeological validity. What remains is assessing the suitability of the laboratory process adopted, which included both pre-treatment and measurement protocols.

3.3.1 Pre-treatment

The pre-treatment of a sample describes the process by which exogenous carbon (= contaminants) is being (chemically) removed, and/or endogenous (=original) carbon extracted. Two main forms of contamination are targeted: those deriving from a possible conservation procedure and those from the burial environment. Laboratory inter-comparisons can be very useful in assessing pre-treatment protocols. In the early years of radiocarbon dating they have been for long time promoted by the International Atomic Energy Agency (IAEA). More recently they have been published in the proceedings of the annual International Radiocarbon Conference (e. g. Scott et al. 1998; Fiedel et al. 2013). If a sample has neither been conserved nor consolidated, the following section does not apply and pre-treatment assessment can directly start at point 2.

1. Solvent wash. Conserving and consolidating a sample can severely contaminate the radiocarbon signal, and the materials used in restoration laboratories and museums are often challenging to remove. Consequently, numerous strategies to remove contaminants deriving from the conservation procedure have been and continue to be developed (Bruhn et al. 2001). They can be divided in two main groups:

    a. *Targeted solvent wash.* Ideally, the conservation material is known and an effective contamination removal protocol has been established. In this case, a targeted chemical treatment can be applied.

    b. *Generic solvent wash.* Most often there is no record of what conservation material has been used. As a consequence, a generic solvent wash removing most known preservatives should be applied. Examples can involve washes with highly volatile solvents such as acetone, methanol, hexane and chloroform.

    If a conserved sample has not undergone any treatment to remove conservatives and consolidants, the radiocarbon date should be discarded as untrustworthy. The outlook is more positive if a solvent wash or a different targeted treatment was applied. Nonetheless, it is worth consulting the latest literature to evaluate the observed effectiveness of the treatment used (Fedi et al. 2014).

2. Extraction of the endogenous (=original) carbon fraction. Various protocols have been developed and continue to be developed, aiming to most effectively and efficiently eliminate any exogenous carbon without losing too much material. There are two main strategies available: the elimination of exogenous carbon from the sample, or the extraction of a fraction of the carbon known to originate only from the sample (compound-specific dating). In reality, many pre-treatment protocols incorporate both approaches to varying degrees. In the following, we differentiate between organic and inorganic carbon, as the chemical nature of the endogenous carbon fraction of interest requires different treatments.

    a. *Organic carbon.* Depending on the nature of the sample, the pre-treatment targets different organic fractions: e.g. keratin from hairs, collagen from bones, dentine from teeth, etc. The majority of these protocols is based on a series of acid-base-acid (ABA) washes, in which hydrochloride acid (HCl) solutions should remove carbonates that, in an open system, are highly susceptible to contamination from the burial environment, as they are chemically indistinguishable from carbonates found in water that could percolate through sediments. Alkaline solutions are used to get rid of humic acids from the burial environment and a final acid wash is used to eliminate carbon fixed from the atmosphere during laboratory procedures. Depending on the sample type, more rigorous treatments have been implemented in many laboratories, including extra filtering, and oxidation steps. For example, another method has been proposed for bones' treatment by R. Longin (1971): it uses an acid demineralization of the mineral matrix followed by collagen solubilisation at high temperature under mild acid conditions. A modified version ("modified Longin") has been recognized to be quite effective in further purification of samples (Brown et al. 1988). Also protocols extracting amino acids from proteins are expected to become more common in the future. For example, hydroxyproline is an amino acid indicative of bone collagen, therefore significantly reducing the risk of dating any carbon non-native to the bone in question (A. Marom et al. 2013). Pre-treatments using ABA, or modified and upgraded versions thereof, are commonly applied and thought, for most samples, to be sufficient to decontaminate the material. A more decisive answer is only possible by assessing the reliability of dates obtained from standards that have the following in common with the dated archaeological sample of interest: identical pre-treatment, measured at the same laboratory, and dated at roughly the same time. This is, of course, rarely possible for 'old' dates. Therefore, we recommend using the simple approach described in the flow-chart and only delve further into pre-treatment details if discrepancies in the dataset need to be evaluated and explanations for possible outliers are thought to be related to differences in pre-treatment.

b. *Inorganic carbon.* Carbonates (shells, dental enamel, mortar, etc.) constitute a particular challenge for radiocarbon dating, because the most common form of contamination and the endogenous carbon fraction of the sample are, on a molecular level, chemically indistinguishable. Often, simple acid etches are used in pre-treatment. They are thought to preferentially attack the labile components of the sample, which are believed to be more susceptible to contamination. Sometimes, different CO_2 fractions are collected during the process with the aim to date the more stable and robust carbonates (Russo et al. 2010). Other protocols implement pre-screening methods, such as Fourier-transform infrared spectroscopy (FT-IR), to identify unaltered samples (Vagenas et al. 2002). Caution is advised when dealing with radiocarbon measurements obtained from carbonates. While they have been successfully employed in certain cases, they have been shown to be problematic when used as single dates without any other chronological information (e.g. Zazzo 2014).

3.3.2 Counting method

Two main counting methods have been used to measure the $^{14}C/^{12}C$ ratio: beta (=scintillation) counting, commonly using either purified CO_2 or benzene; and accelerator mass spectrometry (AMS). Knowing whether the measurement was performed through the former or the latter method can be insightful and is necessary to assess the dating quality, though it should not be interpreted as a given, that one method results by default in a higher quality date. While AMS dating has largely superseded decay counting methods in radiocarbon dating, some laboratories still operate beta counters and provide reliable, high-precision data. The major differences between the two techniques are:

1. Accelerator Mass Spectrometry (AMS). By directly measuring the carbon isotopes, AMS allows for reliable measurement of

significantly smaller amounts of radiocarbon than beta counting (Taylor 1987, *loc. cit.*). On the one hand, this translates into dating smaller as well as older samples, extending the application of radiocarbon dating in time (e.g. to the Palaeolithic) and the range of samples used (e.g. to seeds or precious artefacts). In addition, due to the very small sample sizes required, AMS enables a more rigorous pretreatment chemistry to be applied to the sample. Compound specific dating is therefore restricted almost wholly to AMS laboratories. On the negative side, heterogeneities in samples and contamination will have a stronger effect on dating reproducibility (Bronk Ramsey et al. 2004).

2. <u>Beta counting (= conventional radiocarbon measurement method)</u>. For younger or very large samples, conventional methods can produce dates as accurate and precise as AMS, in fact more so in some facilities (Gupta and Polach 1985). The radiocarbon calibration curve is almost entirely produced by conventional means due to the higher precisions traditionally attainable. The methods produce larger errors for samples containing little radiocarbon (e.g. small or old samples). As it requires significantly larger amounts of datable carbon compared to AMS methods conventional measurements were often obtained on long-lived or bulk samples, and sometimes mixed assemblages. As a consequence, dates older than 30,000 BP must often be viewed with caution. For all other dates, it should be remembered that bulk rather than single-entity samples become more likely in the absence of records stating otherwise.

4 Conclusion

Once an 'old' radiocarbon measurement has passed through all the three steps of assessment, it can be considered reliable enough to meet presently accepted standards and suitable for archaeological interpretation. As a single date, it can be calibrated with the most recent calibration curve to obtain a calendar age. As part of a sequence or context, it may be used together with other dates to build chronological models. It would remain prudent, though, to distinguish these measurements from other dates obtained from samples specifically selected for new research. This is most elegantly achieved by applying an outlier model with adjusted parameters.

Research on radiocarbon dating is still ongoing and further "revolutions" are likely to happen in the (near) future. This is the nature of research. Therefore, it is essential that each archaeological publication that makes use of radiocarbon dating – irrespective of whether it implements 'old', new measurements or both – provides its readers with all preliminary information needed for their assessment. The never-ending process of refinement in the understanding of radiocarbon dating does not necessarily reduce the reliability of the method itself, provided that sufficient information is available to update and adjust 'old' results with respect to new discoveries.

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Figures

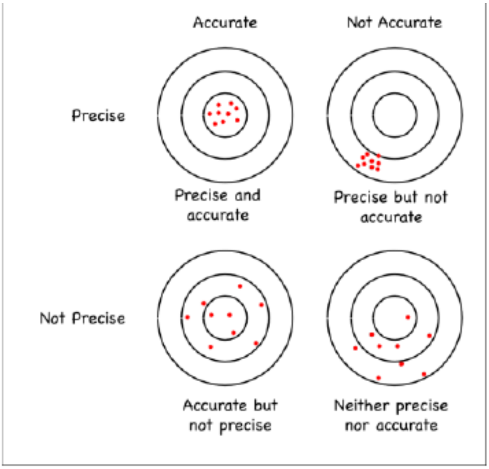


Fig. 1 Visualisation of accuracy and precision using a target showing the true age at the centre and red dots representing the radiocarbon measurements.

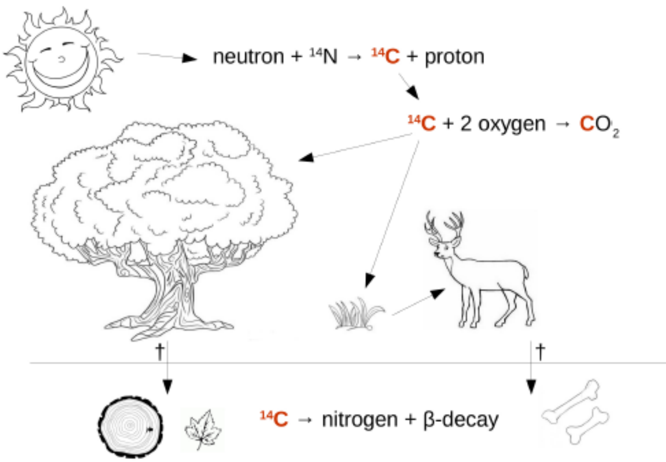


Fig. 2 A simplified illustration of the terrestrial radiocarbon cycle: ^{14}C is generated in the upper atmosphere, and subsequently reacts with oxygen. The resulting carbon dioxide is incorporated into

terrestrial plant life, from where it dissipates up the food chain. After dynamic equilibrium between tissue and environment is broken, radiocarbon decay leads to an exponential degrease in radiocarbon concentration in the specimen.

Fig. 3 Schematic illustration of the radiocarbon date assessment process. For further details and discussion consult the respective sections in this article.

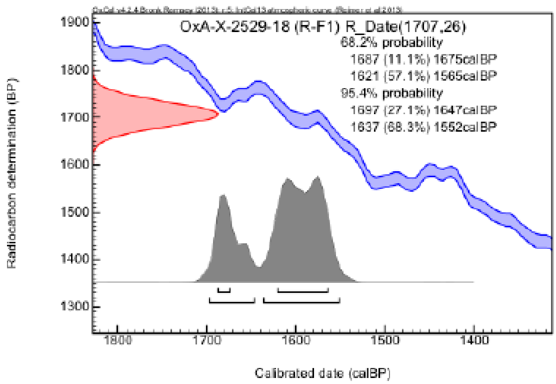


Fig 4 Visualisation of different aspects of radiocarbon dating for a ^{14}C date of 1707 ± 26 BP: red – probability distribution for conventional radiocarbon age; blue - calibration curve IntCal13 (Reimer et al. 2013); grey - probability distribution function for calibrated radiocarbon date with bars indicating age ranges at 68% (upper) and 95% (lower) confidence intervals. The title shows (1) the date's lab code with X indicating that lab procedures attribute lower confidence in the date (e.g. experimental pre-treatment); (2) Sample number from site in brackets; (3) software code used: R_Date(1707,26), indicating the use of a conventional radiocarbon age and its value with 1 SD error. Underneath the title line, the calibrated age ranges at both 68% and 95% are given in numbers. As the date shows a bimodal distribution (two peaks) which are not overlapping, two age ranges have to be given, both showing the probability associated with them. Graph and calibration done using OxCal 4.2 (Bronk Ramsey 2009).

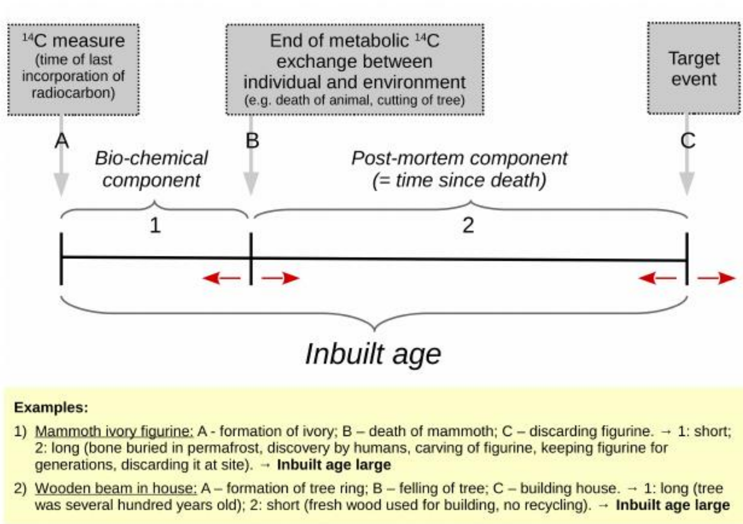
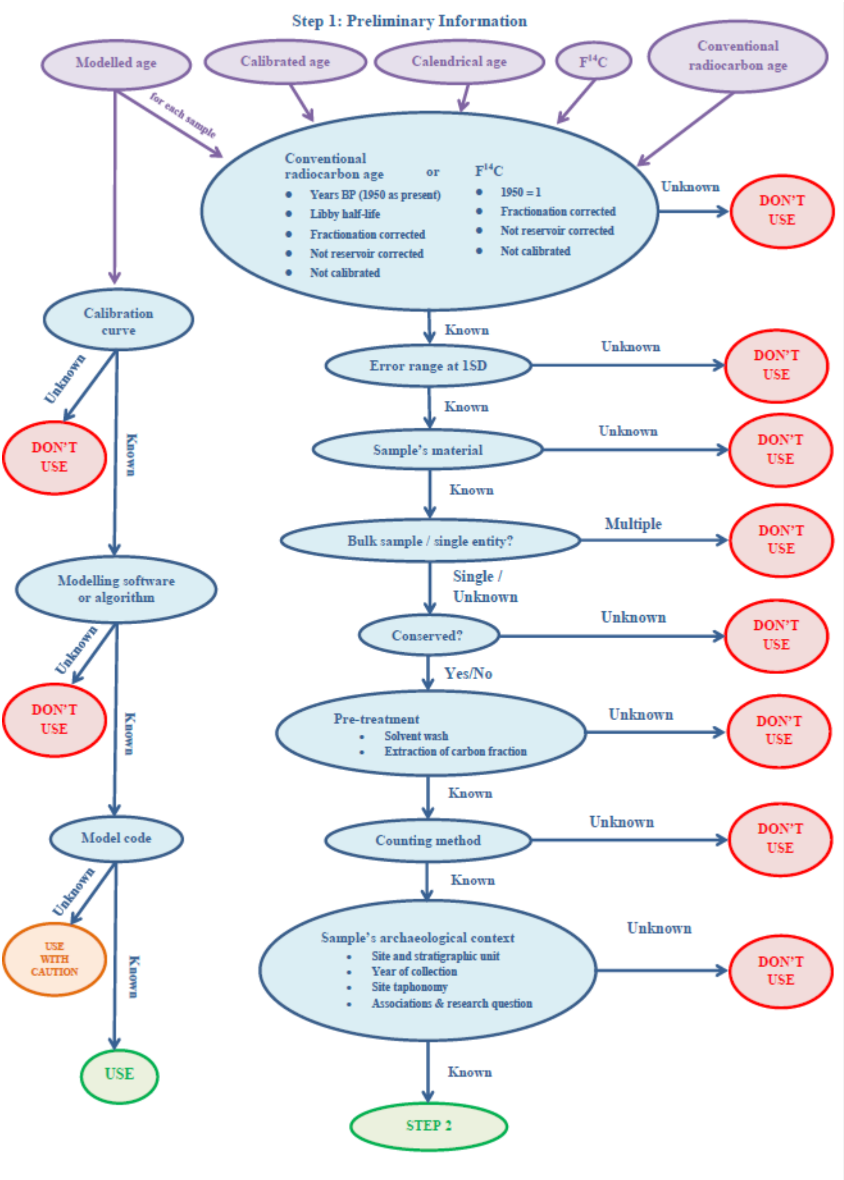
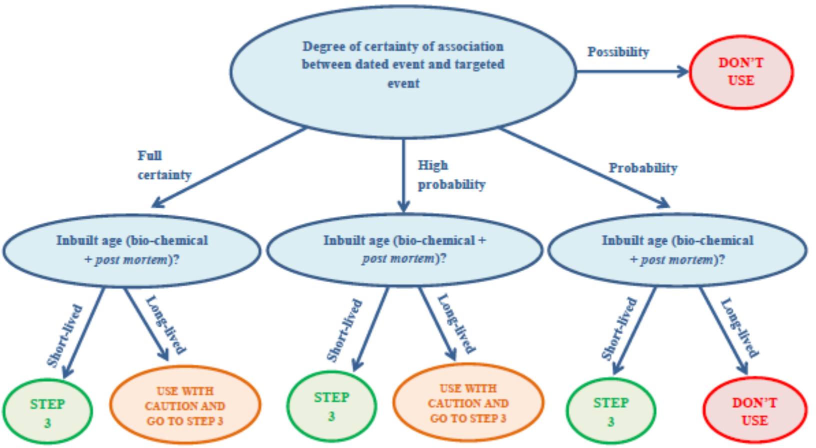


Fig 5 Inbuilt age: Schematic representation of the time between the radiocarbon measure and the event to be dated, showing the (1) bio-chemical and (2) *post-mortem* component resulting in the overall inbuilt age.

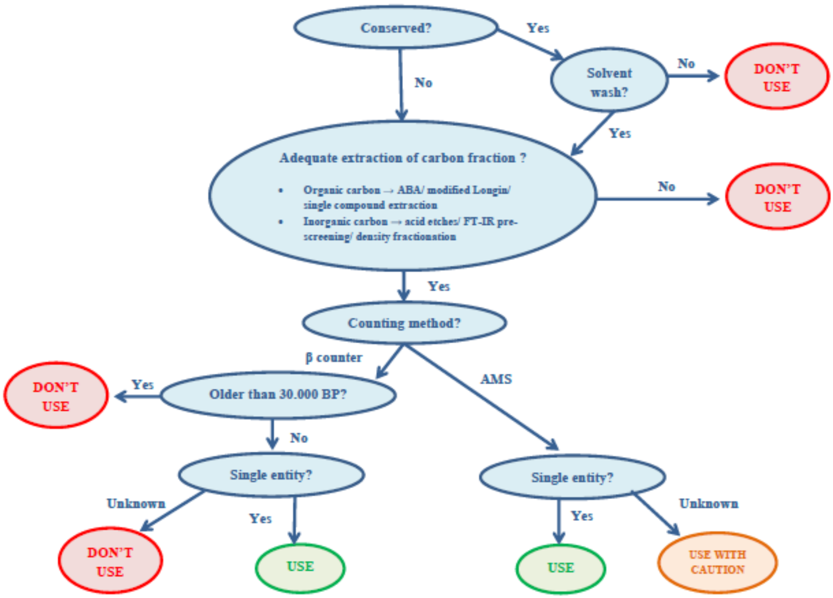


Step 1: Preliminary Information
Modelled age
Calibrated age
Calendrical age
F14C
Conventional radiocarbon age
for each sample
Conventional radiocarbon age
Years BP (1950 as present)
Libby half-life
Fractionation corrected
Not reservoir corrected
Not calibrated
or
F14C
1950 = 1
Fractionation corrected
Not reservoir corrected
Not calibrated
Unknown
DON'T USE
Known
Error range at 1SD
Unknown
DON'T USE
Known
Sample's material
Unknown
DON'T USE
Known
Bulk sample / single entity?
Multiple
DON'T USE
Single / Unknown
Conserved?
Unknown
DON'T USE
Yes/No
Pre-treatment
Solvent wash
Extraction of carbon fraction
Unknown
DON'T USE
Known
Counting method
Unknown
DON'T USE
Known
Sample's archaeological context
Site and stratigraphic unit
Year of collection
Site taphonomy
Associations & research question
Unknown
DON'T USE
Known
STEP 2
Calibration curve
Unknown
DON'T USE
Known
Modelling software or algorithm
Unknown
DON'T USE
Known
Model code
Unknown
USE WITH CAUTION
Known
USE

Step 2: Validity Assessment



Step 3: Measurement and Treatment Assessment



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