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ARCHAEOLOGICAL RECORDING: FORM AND CONTENT, THEORY AND PRACTICE

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Resumen

En este artículo se ofrecen algunas perspectivas relacionadas con la teoría y la práctica del registro arqueológico. Se comienza investigando la aplicabilidad de conceptos de la Antropología Lingüística, de la Ontología Social y de la Filosofía del Lenguaje en el proceso de creación de la documentación escrita de la excavación. Luego, se presenta brevemente una serie de estudios de casos que ilustran los procesos de registro de Sardis (Turquía), Buenos Aires y cuenca del río Limay (Argentina), Waddi Rayyan (Jordania), sitios excavados por la Israeli Antiquities Authority (Israel), Lago Vouliagmeni (Grecia), trabajos de la Escuela de campo de la Universidad de Birmingham (Reino Unido), Combe Capelle (Francia) y los sitios Mimbres (Nuevo México, Estados Unidos). En este sentido, se aborda el uso de SIG y de escaneo laser con fines de registro. Se concluye con una reflexión acerca de si se podría registrar correctamente un sitio hipotético que cayese completamente fuera de las expectativas y de la experiencia de los arqueólogos, y en tal caso, cómo.

Abstract

In this article, I offer a number of perspectives on the theory and practice of archaeological recording. I start out by investigating the applicability of concepts from linguistic anthropology, social ontology and the philosophy of language to the process of creating the written documentation of the excavation. I then briefly present an array of case studies illustrating the process of recording at Sardis (Turkey), Buenos Aires and the basin of the Limay river (Argentina), Waddi Rayyan (Jordan), sites excavated by the Israeli Antiquities Authority (Israel), Lake Vouliagmeni (Greece), University of Birmingham's field schools (UK), Combe Capelle (France), and Mimbres sites (New Mexico, United States). In connection with this, I broach upon the use of GIS and laser scanning in recording. I conclude by pondering on how and if one could properly record a hypothetical site that would fall completely outside the archaeologist's expectations and experience.

Palabras clave: registro arqueológico, hojas de contexto (locus), antropología.

Keywords: archaeological recording, context (locus) sheets, anthropology.

1. Introduction

Archaeological recording has moved ever closer to the scholarly spotlight in the past 40 years or so (since the invention of context sheets), although it is still far from receiving the same attention as, for example, artefactual studies. A great number of recording manuals have now become available, such as Aufrecht (1992), Buccellati (1978), Bats *et al*. (1986), Badoni and Giove (1984), Dever and Darrell Lance (1978), Herr and Christopherson (1998), Hachmann (1969), O'Connell (1981), and Westman (1994)[1]. The theoretical debate is however lagging behind, despite salient contributions offered by Andrews *et al*. (2000), Hodder (1999), and Lucas (2001). In the present article, I do not presume to offer a background discussion of why it is necessary to record the dig, nor of what types of information basic pre-printed recording sheets include. I rather attempt to account for the variety of approaches that can be used today to tackle the issue of recording, and to report on the diversity of modern recording systems.

2. New theoretical perspectives on archaeological recording

Recording and linguistic anthropology

The perception of archaeological recording as being a mechanical transposition in words and images of what one finds while excavating has long been contested by researchers emphasizing that recording is theory-laden (Hodder 1999, Lucas 2001). A striking parallel to this process comes from the field of linguistic anthropology and has not been, to my knowledge, remarked upon yet, although a few studies of archaeological

[1] See review in Pavel (2010).

recording have come close to it from divergent directions of analysis (Goodwin 2006, Rossini 2008, Yarrow 2008).

In linguistic anthropology, tapes and videos of informants, be they speaking languages threatened by extinction, or simply being investigated for a better understanding of the workings of language in a given setting, need to be transcribed before any analysis can be performed and published. Linguistic anthropologists work with these transcripts, rather than the tapes themselves, just as archaeologists who put together a site monograph work with the excavation diaries, databases, and drawings, not with the weathered profiles from last summer.[2] Just as no two archaeologists will draw a given profile in the same way, no two anthropologist will offer the same transcript of a given tape. In linguistic anthropology, such transcripts used to be considered self-evident, or "transparent", but lately they have come also to be understood as theory-laden. Where researchers simply used to say they worked from neutral "verbatim" or "full transcription", now, since Ochs (1979), there is a proliferation of opinions suggesting that transcription is, in fact, already a form of analysis (Davidson 2009), that, as any form of recording, it structures data (Lapadat and Lindsay 1999) and indeed, as Ochs (1979:44) initially proposed, that transcription is a selective process reflecting theoretical goals and definitions. It is remarkable how close this debate is to the discussion around the objectivity of archaeological recording.

Transcription is a situated practice, always within an intellectual tradition, part of broader social practices and contingent on time and cultural context. Evidently those transcribing tapes while studying diglossia, autism, child verbal behaviour, expression of irony or cultural stigma would look for various specific clues in their informants' verbal and non-verbal behaviour. Ochs (1979) has, for example, noted that the use of standard ortho-

[2] This is not to say, as Dibble *et al.* (2003: 81) do, that "field forms, notes and databases… provide the documentation needed to interpret the context of archaeological data recovered". These forms are produced during what is already an interpretative process, and later are certainly further interpreted at other levels of analysis.

graphy in the transcriptions of children's utterances forces meaning upon what may be just phonological manipulation (sound play), taking them to be pieces of information. This in turn stems from an assumption that language is designed to express ideas. The decision to foreground verbal over non-verbal behaviour (or the other way round) in a video tape transcript also carries considerable theoretical implications. The way police and court officials transcribe investigations is not uncommonly pervaded by a sense of where the authority resides[3]. Another example is the anthropological study of how laughter is produced and expressed (Jefferson 1985). Also, the simple decision to transcribe two speakers on columns, one being to the left (and therefore, in English and many Indo-European writing systems, dominant, Bucholtz 2000), rather than one under the other, introduces other assumptions. A case of particular relevance for archaeologists is how certain transcription strategies may result in stereotyping speakers (Jefferson 1996). Archaeologists are well aware that some recording strategies, combined with the tedium of hard, at times unrewarding work, can make all contexts (loci) look alike.

Transcription is confronted with interpretational issues, but also representational issues: not only what is transcribed, but how it is transcribed, involves decisions about form and content which are inevitably based on theoretical assumptions (Ochs 1979) and often conditioning one another (Bucholtz 2000). There is no mechanical application of notation symbols to the sounds of language. Powell (2002: 59 and 122 sqq.) goes so far as to say that writing does not represent speech, since speech is a wave and there are no discrete sounds in the way there are discrete letters. Powell rightly highlights here the conventional aspect of writing and the fact that putting down in writing what one hears is not a straightforward affair, but the result of interpreting, discerning, and decision-making. As Davidson 2009 puts it, it is impossible to record all features of speech and interaction. It is hard to reach a consensus as to what should be

[3] See Bucholtz 2000:1442 on how "transcripts operate politically".

required to be designated by graphic symbols in the transcript: intonation units; end of intonation unit; falling intonation; fall-rise intonation; rising intonation; break in the intonational unit; self-interruption; break in the word, sound abruptly cut off; emphatic stress or increased amplitude; pause of 0.5 seconds or less; pause of 0.5 seconds or greater, measured by a stopwatch; exhalation (laughter, sigh); nonvocal noise; overlap beginning and end… (Bucholtz 2000). Ahearn 2011 details: "there is no perfect or final transcription of any linguistic interaction. Researchers must choose which features to include omit or highlight in their transcript depending on the focus of their analyses. Should they time pauses in tenths of a second? How if at all should they indicate overlapping speech or nonverbal gestures? What emphasis or intonation should be included in the transcript and with what symbols?". Should one record all stutters, pauses, intonation, the ers and the ums (Lapadat and Lindsay 1999)? Others such as Duranti 2006 (cf. Ochs 1979) suggest that the perfect transcript, one that records everything, would be technically and interpretatively difficult to work with, and that on practical and theoretical grounds it is best if selective. As Ochs (1979) urged, selectivity, not random, but clearly anchored in the hypotheses examined by the researcher, must be encouraged. In the same spirit, Ahearn (2011) spoke of transcripts as both incomplete and biased, and Psathas and Anderson (1990:77) of a "version of the data for particular analytic purposes". For Duranti (2006:309), transcripts only give us "a restricted, selected perspective—a stance, a point of view, often with an attitude" and he goes on to state that "transcripts have properties of models. It is such model-like properties that can allow us to argue through them and about them. It is their model-like properties that make them good to think with." Lucas (2001) has indeed spoken about the record as a model, and not a copy, of the archaeological site.

For Bucholtz (2000) transcribing is simply creation of text. She continues (1441-1442):

"*All transcripts take sides, enabling certain interpretations, advancing particular interests, favoring specific speakers, and so*

on. The choices made in transcription link the transcript to the context in which it is intended to be read. Embedded in the details of transcription are indications of purpose, audience, and the position of the transcriber toward the text. Transcripts thus testify to the circumstances of their creation and intended use."

For a field archaeologist, the paradigm in which linguistic anthropologists analyze transcription issues is bound to evoke common dilemmas in archaeological work and suggest that the analysis of the intricacies of recording and its impact on the intellectual modeling and archiving of the site can benefit from a more anthropological approach.

Recording and social ontology

Contemporary philosophy, and particularly social ontology, is becoming increasingly preoccupied with recording. It would be useful to introduce here the concept of "documentalità" as the structural human need to leave traces and record traces proposed by Maurizio Ferraris (Coralini 2009; Ferraris 2009a). Documentality is a theory of documents as the highest form of social objects (Ferraris 2007). Derridian in his centrality of writing, the Italian philosopher proposes an ontology of social reality where social objects, particularly documents, are fundamental. In fact, in an article from 2007, he boldly stated that, because nothing social exists outside the text, papers, archives, and documents constitute the fundamental elements of the social world. For Ferraris (2008:109), we live in a society of recording, not a society of communication. Recording, or registration, is the key condition for the creation of social objects, which are, he seems to imply (Ferraris 2007), instrumental in the becoming of human beings through socialization.

For Ferraris (2009b), documents, as social objects, are powerful interfaces between an individual and society. Any social act inscribed on a support (from paper to memory) is a social object. Social objects cannot exist outside records. Most importantly, Ferraris distinguishes between weak documents, recordings of facts, and strong documents, inscriptions of social actions; both categories create the sphere of documentality.

Weak documents can be unintentional, e.g. simply a clue found by the forensic investigator or a symptom of a disease in a clinical file, or intentional, such as the clinical file or the forensic file. While weak documents are mere records, often not of a public or intentional nature, strong documents are inscriptions of acts which pertain to the sphere of institutional objects, such as paper money, a wedding certificate, or, for that matter, art. While a weak document is a proof, a strong document is an act, often of a performative nature, belonging to a superior ontological category (Ferraris, it must be said, does not employ the concept of performativity). He illustrates the distinction between weak and strong documents by hinting that a fingerprint on a safe left behind by a burglar is a weak document, while a fingerprint included in the signatory page of an identity card is a strong document.

Going back to field work, I would propose that it is the excavator's social obligation to produce *strong* documents. The record of archaeological discoveries (artefacts and their complex soil matrix) left behind by an extinct society can result either in weak documents or in strong documents. Often these discoveries are under-recorded, that is, only a weak document is produced, which at its best lists details which might become clues for the investigation. The field documentation must be the inscription of the act of supreme curiosity and striving for knowledge, not simply counts and measurements.

Retrieved archaeological remains only become social objects when they are incorporated, by word or image, into strong documents. It is in that moment that they acquire considerable import for society. By producing the record, and on its basis, that second tier of recording which is the publication, archaeology asserts itself socially as a legitimate human science. The demise from trying to understand the social and mental structures behind physical evidence in the act of recording will often preclude this understanding in post-excavation stages as well (Andrews 2000). Quite simply, if one records archaeological evidence in a weak document, it will always remain a weak document. Archaeology should arguably restitute to us societies, not material

debris measured to the millimeter. It seems more appropriate to strive for a recording that is informed by understanding and interpretation, a recording that neither defers interpretation nor bans theory and hypotheses in the name of a total record or total objectivity, because none is scientifically achievable or indeed profitable. Archaeological recording is not about producing paperwork, but social objects. In so doing we are not only salvaging an extinct society, but we are also enriching our own social world. If, as Ferraris has it, recording is the act *par excellence* of creating social objects, archaeologists have to assume higher imperatives in their fieldwork. Recording is not ancillary for "archaeologizing", it is a constitutive part of it. Excavators cannot possibly aim to produce weak documents, meaningless logs of material distributions, but strong documents instead, pertaining to the inner workings of an extinct society and sanctioned by a mandate from our own society.

Recording and the philosophy of language

As John Austin (1962) has argued, some speech acts are performative, and not simply constatative, since they do something, rather than describe something. For Austin, a performative utterance is not truth-evaluable, but merely "felicitous" or "infelicitous" depending upon whether or not it has been performed in the proper manner and circumstances to ensure its success. Utterances such as "the court is now in session", "war is declared", "I apologize", "I now pronounce you man and wife", by being uttered, do something (Austin 1962:5). I contend that the description of archaeological deposits is, except in extreme cases, a performative utterance. Archaeological recording is performative in the sense that, by the very act of recording, one transforms some characteristics of archaeological discoveries into surviving archetypes at the expense of all unrecorded characteristics. Archaeology is history based on material remains, true, but this should not obnubilate the truth that all archaeological interpretation is also rooted in a linguistic reality. Archaeology transforms material remains in words and then builds narratives, explanations, and interpretations with them. This makes ar-

chaeology assume, on the one hand, the interpretative limitations exposed by Quine in his theory of the indeterminacy of translation (Quine 1960, 1968), which states that no science can take words as raw data, since words are not a measurable feature of the world. There is, for Quine, no single correct way to objectively interpret a sentence. No analysis of verbal behaviour can yield the truth. On the other hand, as de Kerckhove put it (de Kerckhove 1997:194, discussion in Brill 2000), "epistemology has become ever more the science of how media construct the reality of the things that we know". The way we formalize and archive our impressions (in words as well as in images) ultimately structure our knowledge. Not only the researchers that re-assess our excavations on the ground of our record, but we ourselves grasp the discovery in its verbal avatar. This means that in our interpretational endeavour, we deal with the verbalisable, recordable aspect of the discovery, not with its physical embodiment (Lucas 2001 for a different view). My point here is that the recording of deposits literally transforms the deposits in their description, it transports their concreteness into words and images. Whatever has not been selected by the describer to be a part of a description effectively ceases to characterize the deposits. Indeed, it ceases to exist altogether once the deposit, after recording, is removed. Even the few deposits that are not removed but left *in situ*, whether open to the public or not, only exist to the scientific community as their published description, or, even more indirectly, as the conclusions drawn on the basis of those descriptions. Moreover, once the field season is over and the team is piecing together the final reports, they generally resort only to their own descriptions of the deposits. Now, all descriptive statements are partial in that they do not attempt to offer a full description of the thing being described; they are only concerned with a selection of the characteristics. This selection depends, among others, on the focus of research and the various technical limitations of the investigation. However, archaeological description proceeds at the expense of things left undescribed, obliterating all properties of deposits, features, structures that are not mentioned prior to their destruction. Archaeological

recording thereby brings into existence novel objects, creating a Potemkin village of knowledge there where the material remains have been annihilated. Recording, then, does not merely describe the archaeological discovery, but brings it into existence. Archaeological description makes knowledge possible by performative utterances, by mediating between subject (archaeologist) and object (archaeological discovery), by reconstructing the object as a verbal edifice which is intelligible and scientifically profitable. While performative utterances as defined in the philosophy of language cannot be false (but see Searle 1989), they can be, as we have seen infelicitous or "unsuccessful", if for instance the person declaring war is not entitled to do so, if an apology is not needed nor asked for etc. On the contrary, the constatative utterance, "this is bus 32" can be false. The bus arguably existed before, and continues to exist after the utterance. Quite the opposite, the profile cut by the archaeologist has never existed before, and minutes after being recorded will begin to erode into illegibility. In short, the archaeologists' quest while recording could be thus defined as the production of felicitous performative utterances.

3. Practices of archaeological recording

Data collection

Clarke (1973:17) has once defined archaeology as "the discipline with the theory and practice for the recovery of unobservable hominid behaviour patterns from indirect traces in bad samples". What is then the adequate way to record "indirect traces in bad samples"?

Data collection (how and how much to collect) is one of the fundamental problems in field archaeology. I define data collection here as a formalized process of gathering information on a support. One can understand some things about a site without ever writing a word about its excavation, but how this works (or rather does not) will not detain us here. I am concerned instead with the underpinnings of data collection, which also have implications in the objectivity/subjectivity and description/interpretation debates. In the optimistic opinions of the brave early

archaeologists such as Pitt-Rivers, everything on an excavation, "however small and however common" must be recorded, for "there is no knowing what may hereafter be found to be most interesting" (Pitt-Rivers 1887: xvii). It is true that he was speaking about artefacts there, in the context of typological variability. However, his insistence that excavators must reduce their own personal equation to a minimum in order to record as much as possible, and as objectively as possible, represents an early statement of this policy of full data recovery. In this are rooted all approaches which recommend all-encompassing observation, on the basis of which one can eventually develop theory. Some 20 years before Pitt-Rivers, an opposite opinion had been expressed – by Darwin: "all observation must be for or against some view if it is to be of any service"; somewhere else he writes, "I have an old belief that a good observer really means a good theorist" (letters from 18 Sept. 1861 and from 22 Nov. 1860 respectively, *apud* Medawar 1969: 11, note 6). This is the encapsulation of a theoretical stance wherein data are always constituted within a theoretical framework, and observation is dependent on pre-understanding. The total data collection approach is typical of logical empiricism; "selective" data collection is now typical of post-processual approaches.

In his reflections on scientific methodology, P. B. Medawar (1969: 28) notes that "innocent, unbiased observation is a myth" and that "our observations no longer range over the universe of observables: they are confined to those that have a bearing on the hypothesis under investigation" (id.: 51). J. Hill (1972) vehemently combats the idea that one might collect all data, which he calls the "vacuum cleaner approach" (67). He argues that one cannot "go into the field with an open, unbiased mind and collect a large body of «basic data» suitable for a wide variety of subsequent analyses. In fact, there is an infinite amount of potential data, and choices of what to observe must be (and always are) made in the light of a priori ideas" (id:63). Hill sharply observes that any site poses an infinite number of problems to be solved, and that collecting "all" data in the hope that they will then spontaneously organize themselves to answer these problems is

an illusion. In his words "we simply cannot collect everything that might be relevant to something" (Hill 1972:71). It seems accepted now that data is hardly ever acquired in modern science by observations that are devoid of particular questions, hypotheses, or theories (Pavel 2011). However, as Hirst proposes, even if we cannot collect data that might solve all the possible problems, we should strive as much as possible to anticipate questions that other archaeologists, with different experiences and agendas, might want to ask of this site. The most meticulous recorders of all, Palaeolithic archaeologists, agree it is impossible to record, for example, all aspects of the provenience of an object (Dibble *et al*. 2005). They offer a lucid evaluation of the situation (317-18): even though sites are destroyed by excavation, archaeologists must balance recovery of information with the expense of that recovery. These expenses include not only the fieldwork, but also the costs of processing, analysis, and curation of the objects and data. The solution is to concentrate on collecting data relevant to the research design of the project, while adhering to accepted standards of recovery for materials that might be of interest to other researchers.

Consequently, we should always ensure, regardless of the goals of our investigation, that a minimum of essential data is gathered; but the definition of this minimum is not straightforward either. Ideally we would also be able to tailor our observation methodology, and the recording system that expresses it, not only to our own, but also to our peers' research agenda. All others being equal, a recording system that allows more research objectives to be fulfilled is better. This functional aspect of recording is often lost from sight. We record as a natural consequence of the interpretation process, and not just in order to preserve, or more precisely, there is no true preservation of a site without understanding. It is indeed unethical to excavate and not interpret. As Higginbotham (1985:13) warned, "if, in the interpretation of a site, a post-hole remains a post-hole rather than a gate post, a verandah post, fence post, or corner post of a barn, then the archaeologist has not done his or her job." One does not preserve a series of post holes, but a whole cultural construct

that makes sense of them and the others vestiges at the site. Clearly, the archaeologist's two obligations, to record and publish are interrelated, and bad recording often results in no publication (Hirst 1976). Different purposes require adjustments of the recording scheme. Hachmann (1969) has expressed concerns that the adoption of a pre-existing system without the fine-tuning required by the new site's personality is dangerous. Recently Yarrow (2008) has argued that the categories of prompts and boxes on the context sheets express certain concerns and judgments about the site where they are used and tend in turn to reinforce these judgments and reify the site as a cultural object. These pre-printed sheets tend to restrict description and (even worse) interpretation to the categories that they have been designed to include. But our recording system must not be a Procrustean bed for archaeological data, or a set of pigeonholes in which we force the endless variety of evidence. As opposed to context sheets, site diaries are by nature not standardized, or less so; but their use poses problems too. For Susan Hirst, notebooks, at their worst, are just "messy and meaningless collection of inconsistent jottings" and even at their best, "the data is at the mercy of the archaeologist" (Hirst 1976:17). But Herculaneum is a different example: Nicolas Monteix, who is preparing an edition of Maiuri's diary from the Herculaneum excavations, has noted that the site as seen today is as much a creation of Maiuri's as it is a creation of Vesuvius' eruption (Monteix 2009). In Maiuri's publication of the site (1958), some of the houses are described as restored, not as discovered and recorded in the diaries.[4] Are diaries then made to be modified, overcome, overruled? H. L. Dibble and his team had to work hard from notebooks that were less than encouraging, as the reproduction of one page in the article shows, studying the excavations of none other than François Bordes at Combe Grenal (Dibble *et al*. 2009). Ultimately, their research is a good case study of how difficult it is to analyze

[4] Striking examples are the Casa di Nettuno e Anfitrite, wherein objects found elsewhere have been exhibited, and Casa a Graticcio, where the balcony overhanging the street was omitted from the reconstruction because it did not fit Maiuri's ideal vision of the site.

a collection of artefacts produced by some other team's excavation; of how tantalizing this becomes if the respective excavation has not produced adequate records for it; and ultimately of how controversial it is to continue excavating when material from older excavations is not published or not even washed (*cf.* Dibble *et al.* 2005). True, some of Bordes's original records have been mixed or misplaced in the years *after* the excavation, but this, on the other hand, is the usual fate of excavations whose publications is long delayed.

Unfortunately, few excavations publish any information regarding their recording systems. If this must be taken to mean "why, we record just as everyone else does", then it shows little awareness of the variety of recording systems in use. Other reasons, such as lack of editorial space are equally non valid, since a (concise) description of the recording system must be planned for in advance and be reserved some room in the final publiccation. Finally, it is understandable why excavations where recording has been done pretty much at random oust from their reports any mention, let alone a dedicated section, of methodology and recording (which generally go hand in hand); but this cannot be in the least condoned.

Even the more methodologically aware excavations, which describe for example what they mean by locus or feature, generally leave aside any information about their notebooks and context sheets, and not because of lack of space, but because of perceived lack of relevance. This is proven by the fact that some excavations using e.g. Museum of London MoLAS system (Westman 1994) do not feel the need to simply state "we use the MoLAS recording system, as published by Westman". This does not take up too much space, and effectively inserts that excavation's approach in a paradigm that immediately allows to reader to understand it better. Finally, of the very few excavations which present their context sheets, just a precious few present them as they should be theoretically filled in, together with examples of how they actually have been completed in the field. It is only this comparison that gives one true insight into how recording was done. Some of Carandini's Italian context sheets

(Rossini 2008) have become public in a typewritten form which neatly prepared the field originals for this particular purpose. Reference collections of recording forms would be useful for archaeologists who want to find inspiration for the recording of their new site, and then, if they cannot present their system in detail, can simply mention in their publication that they used e.g. French context sheets from Bibracte (Paris 2004) and feature sheets from Lattes (Bats *et al*. 1986). I will present in short a few case studies here, as a continuation of the other sixty or so discussed and illustrated by the present author in a 2010 monograph. Undeniably, what data is collected (and how) is in a direct correlation with what is understood and published about a site.

Case studies

1. Sardis

A detailed recording manual has been designed for the Archaeological Exploration of Sardis by director Nicholas Cahill, who found inspiration in the system used in Tel Anafa by S. Herbert. At Sardis (according to Cahill 2010, an internal unpublished document) the lot is the smallest unit of stratigraphy, e.g., a single layer of soil, where soil has no pedogenetic connotation, but simply means sediment. Features such as a pit or a wall with a blocked door may consist of several lots, but a plain wall is both a feature and a lot and is labeled as "Lot [number] [name of category]" such as Lot 1 Wall. Interestingly, in the case of the wall with the blocked door, not only the wall and the blocking are lots, but also the "door", and therefore negative units and interfaces are also recognized at Sardis. Soils layers are allotted numbers only after the excavation is over. Only features, immediately identifiable as such, are given numbers on the spot. If 20 such features have been identified in a trench, then the topsoil will become Lot 21 and so on. Material is stored by baskets, rather than by lots[5]. Baskets are sub-units of lots. As Cahill justly

[5] In the Athenian Agora excavations, which use a similar recording system, baskets are not combined into lots until the very end of the season, when a thorough analysis of the pottery can be done.

remarks, lots are units of stratigraphy, and baskets are units of excavation. This means that within any clearly recognizable lot (the equivalent of what is called a context in the MoLAS and other systems[6]), the excavators will dig and record separately a number of baskets, generally as spits in the absence of visible stratigraphy or when the soil changes are so slight that, in and of themselves, they are not enough to warrant the assignment of a new lot number. Lot numbers are meted out for quite clear changes in sediment, baskets for very minor/irrelevant/no changes. Cahill (2010:2) explains: "a fill may include different kinds of earth, reflecting separate cartloads or dumps of fill; although the cartloads may have different colors and textures, those differences are insignificant since the whole fill represents one deposit". The use of baskets as arbitrary units of excavation within stratigraphic units (lots), in order to avoid any contamination and to enhance control, is highly commendable. Should a putative disturbance finally be identified, then at least some baskets of the original lot may still be left uncontaminated and ready for analysis; in other words, this keeps the one rotten apple from spoiling the whole barrel. Baskets are then lotted together if dates of the finds, or micromorphological analyses, do not seem to indicate the presence of a disturbance unidentified by the excavators. Each basket is described in the notebook on a basket stamp, comprising: basket, lot, coordinates, begin, end level, under/over basket, pottery fieldbook (which number, and page where the pottery is recorded), catalogue objects, description. (A comparable, more elaborate stamp is used in Corinth). Ideally, excavators would always identify stratigraphic units correctly, with their chronologic and/or functional relevance to the site's history (for example not missing the pit that takes late material down to early layers), and then the basket system would be unnecessary. It is on the other hand to be noted that

[6] A stratigraphic unit or context is defined as any archaeological deposit resulting from human or geological activity that has distinguishable physical characteristics and which can be interpreted as functionally or chronologically relevant to the history of the site. The definition is extended to include masonry as well as the interfaces created by removal of such units (Pavel 2010).

the exaggerated splitting of stratigraphic units in numerous smaller units in order to minimize the risk of overlooking a disturbance can slow down or even compromise the excavation, interpretation and recording processes.

The use of "baskets" probably originated with Badè's excavations at Tell en Nasbeh in the 20s and 30s, but the concept did not quite take on its present significance until Dever's excavations in Tell Gezer (1966-1972) and Aharoni's excavations in Beer-Sheba (1969-1974). In recent years it was still widely used in the Near East, especially on Israeli sites, such as Tel es Safi, Tel Dor, Tel Batash, Tel Halif. Occasionally the name may vary, but the idea remains the same ("pail" instead of basket in Tell el Hesi in the Palestine or in Tell Madaba, Jordan). Excavations in Greece also employ this concept, whether in the same manner as in Sardis (e.g. in the Athenian Agora), or with different assumptions (e.g. in Corinth, where the basket is the main unit of stratigraphy and is often called, as in the excavations in Knossos, with its local name, *zembil*[7]).

But why would one not simply call both baskets and lots by one single name, since each of them is, after all, an independent entity, a separate unit? The reason for this is that using just one name for both lots and baskets obscures the fact that two different thought processes are behind the attribution of these categories, not just the perception of a variation in size, with the

[7] For comparison, in Corinth (Sanders *et al*. 2002), "lots" are the baskets that have not been thrown away as irrelevant (storage room is always limited) and receive their own separate running numbers plus the year of the campaign. The lot form indicates the provenance of the shards in the lot, grid, a description of the stratum of provenance, the numbers of the lots above and below, the original and saved weight (the saved weight is generally around a tenth – which is more or less the percentage of diagnostic shards), a detailed description of pottery forms and dates, broken down to fine ware, coarse ware and cooking ware, together with coins and other finds. As opposed to Athenian Agora (and Sardis) practices, in Corinth lots cannot consist of several baskets combined. This is because, as the *Behälter* in Troy, the Corinthian *baskets* already have the stratigraphic significance of a context (or lot in Sardis). If a deposit yields more pottery than one basket can carry, the basket number stays the same with the addition of "b"/ "c" etc. for any new *zembil*; they all carry wooden basket tickets bearing their numbers.

latter being a subdivision of the former. Lots are "authentic" units, and come into play when the archaeologist identifies clear cultural, or more rarely, geological changes in the stratigraphy. Baskets are "artificial" units, and one resorts to them generally in the absence of any perceived change in soil or artefacts, proceeding in cautious spits. The highest returns of the basket system are of course on sites where pottery is relatively securely dated. In geographic areas less or poorly excavated, with no reliable master sequence of ceramic types, it will be stratigraphic control that helps one know which vessels are early and which are late, rather than the pottery being used to check the accuracy of the excavation.[8] It should be stressed here that the name basket, although not as abstract as the terms *context, unit, locus, layer,* or *stratum*, which generally describe the unit upon discovery, does not apply merely to artefacts retrieved from a unit, but to the unit itself, as recognized in situ.

Regarding the excavation diaries, Cahill rightly advises that they ought not to be recopied in an attempt to make them clearer. This often results in the author changing the original details, and "encourages over-interpretation and reworking of primary first hand observations which should be left for the record" (Cahill 2010:7). The notebooks feature excavation accounts on the right hand page, leaving the left hand pages for plans, sketches, sections, object drawing, photos, records of objects discarded during excavation, references, and additions. The day plans, 1:25, are the "clearest record of the day's excavation" and, as in Peter Biehl's excavations in Çatalhöyük, use computer printouts so that certain features do not need to be drawn/recopied every day. On such plans one enters daily the locations of the day's baskets and lots excavated. The first pages of the notebooks are reserved for

[8] It must be said though that even in the absence of any absolute chronological clues, a pottery specialist might still be able to suggest that the assemblages from two baskets seem different by judging from the types present and their frequencies, especially in the case when such differences are consistently reported from e.g. upper as opposed to lower baskets of a certain stratum encountered across the site in several trenches.

indexes and lists of lots and baskets and features with descriptt-tion.

2. Buenos Aires and Río Limay area

The excavations in the area General La Madrid in Buenos Aires have developed in the late 80s a system of pre-printed loose sheets (*hojas sueltas/ formularios /registros preimpresos y móviles*) to record archaeological excavations ([Crivelli Montero] 1989). The "grid sheet", *registro (hoja) de cuadrícula de excavación*, is dedicated to a *capa arqueológica* (stratigraphic unit), defined as "a discrete concentration of archaeological evidence, of limited vertical and horizontal expansion". It records site, *capa*, grid, geologic layer, archaeological layer, levels, and stratigraphic relationships between *capas* (*sobre, bajo, corta a, cortada por, rellena a, rellenada por, igual a*). All important finds (*hallazgos*) are described, either as artefacts or ecofacts (these in a section headed by a triangle, suggesting that a tridimensional positioning is required) or as structures (under a rectangle). If the *capa* is part of a structure, the number of the structure sheet (burial sheet, fire installation sheet, general structure sheet) will be indicated. The most interesting structure sheet is the *registro de estructura de combustión*, a fire installation card. It is unusual that all stratigraphic relationships are entered here again in the same terms (above, below, cuts, is cut by etc.), which is generally not done for a structure, although the Italian ICCD system (Badoni and Giove 1984) does contrast relative stratigraphic positions of features. Under "composition" one lists all evidence for combustion (animal, vegetal carbon; ashes; burnt stone, baked sediment; soot, rubefied surface). The type of structure (built or not) is recorded together with form and profile. Associated artefacts, ecofacts and structures are mentioned. There is a prompt for lenses of sediment separating the results of combustion and entries for evidence of oven maintenance (emptying) as well as patterns of ash dispersion outside the oven. Apart from this sheet, with parallels in Çatalhöyük, the General La Madrid project tried to implement a card for "articulated faunal remains", inspired from the Miqne system, as well as burial sheets and profile

stratigraphy sheets, which appear to have been taken over with a few modifications from Heizer (1953) and Frédéric (1967). These sheets, following the early suggestion of Hirst (1976), were to be printed on paper of various colours.

In a substantial revision of this proposal, Crivelli Montero and Fernández (2005) have offered perhaps the most important contribution to recording systems ever published in Spanish. This system is designed for the excavations in the basin of the Limay River, and, as the authors rightly point out, one could not have simply imported pre-printed sheets used in urban or in Andean archaeology to use on sites such as those in the Río Limay area. As in the Buenos Aires area, here the prehistoric population also consisted of mobile hunter-gatherers.

For Crivelli Montero and Fernández (2005), a recording system must be exhaustive, coherent, economic, objective, secure, clear and flexible. The authors distinguish among field records between chronological registers (such as photo lists, with successive annotations), and systematic registers (such as context sheets, with detailed information on a single topic). As opposed to Crivelli Montero's 1989 proposal, in the Río Limay system the stratigraphic unit is not the *capa* anymore, but the *estrato*, which can be e.g. a layer of ash or sterile sand, a lens of vegetal residue, a cut, etc. Pre-printed sheets are again recommended, as the unique diary does not encourage labor division. The forms ought always to be photocopied and one copy be kept safe in a different place. It is again suggested that the forms be printed on paper with at least edges of different colours, as this helps both in the field and in the archive (495-496). To prevent the allocation by mistake of the same number to different *estratos*, the sheets are taken out in the field already numbered (508). The *hoja de estrato* is a central piece of the written documentation and the gist of it must also be recorded in the square diaries. It is dedicated to both positive and negative layers, represented on the sheet by the symbols "+" and "-". An interesting prompt (*casillero*) receives the *numeración provisional o de campo* (p) or *numeración definitiva o de gabinete* (d). This does not refer to a re-numbering in post excavation work of stratigraphic units as identified in the

field, but simply "to the page number, which may be different in the daily bundle of papers carried by the square supervisor and in the final compilation of the complete excavation record" (Crivelli Montero, pers. comm.) Stratigraphic relationships are scrupulously presented, a minor change from the 1989 version being the replacement of *sobre, bajo* with *cubre a,* respectively *cubierto por*. The 2005 article also recommends the use of "contemporaneous" as yet another possible stratigraphic relationship, but this is not adopted in the pre-printed sheets actually used in the field (or at least not in their 2009 revision, the one I had access to through the authors' kind help). The *estrato* sheet includes interesting boxes for formation processes (as a working hypothesis), for organic content, and for how recognizable the deposit interface is – clear, diffuse, *concordante* (no unconformity between the two layers) or truncated. A mini field guide to recognizing clay, sand and silt in the field is preprinted on the sheet; thus when wet, clay adheres to and stains fingers, and in dry state, it is lumpy; silt is soapy and does not adhere to fingers (when wet), but (when dry) is floury; sand is granular (wet) and abrasive (dry)[9]. Colour is indicated in both wet and dry states. Some possible interpretations are also pre-printed: natural accumulation; human frequentation; floor; ashes/campfire; well/pit; animal hole; fill of animal hole; unintentional fill of well or pit; inhumation; silo; other. The number of bags of artefacts retrie-

[9] Such standardized descriptions are well known from the MoLAS system. At Wadi Rayyan (Autori varii 2005) soil consistency is defined as follows. For coarse sands and gravels: indurated (broken only with a sharp pick blow, even when soaked), strongly cemented (cannot be broken with hands), weakly cemented (pick removes sediment in lumps which can be broken with hands), compact (needs mattock for excavations, loose (can be easily excavated with hoe and trowel); for fine sands, silts and clay: hard (brittle or very tough) stiff (cannot be moulded with fingers) soft (easily molded with fingers) very soft (exudes between fingers when squeezed), friable (non-plastic, crumbles in fingers). The system of the University of Birmingham (Hirst 1976) describes "soil structure" as: crumb, cloddy, blocky, prismatic, laminated. Cloddy is defined as "larger subangular aggregates, often breaking down into crumb; divided into small cloddy 5-25 mm and large cloddy 25-50mm; crumb is" small, porous, rounded or subangular aggregates, usually 3-6mm in diameter.

ved is not handwritten, but circled from among pre-printed numbers. On the "artificial layer form" one records the spit taken within a given unit of stratigraphy. The presence of a prompt where the recorder notes what *estratos* have been encountered in that spit appears to be there for emergency cases; otherwise, as the article advocates, spits will only be used in the absence of visible stratigraphic change. This spit taken through a given layer is called a level.

The fire installation forms from 1989 were not used anymore, but fire installations, as strata, received a *hoja de estrato*. Articulated faunal remains are now recorded with unique identification numbers and recorded tridimensional. The authors considerably augmented the burial sheet. This is four pages long, seems to be inspired by the Tell Hesi forms (O'Connell 1981) and uses the terminology from Sprague's work in the late sixties (now revised in Sprague 2005). The unusual length of this sheet apparently contradicts the "economy" principle, which asks for the recording system to cut the red tape, but is justified by the particular importance and frequency of burials in the Limay area. The position of the body shall be described with reference to itself, not to the grave, square, or cardinal direction (arm "to the face", not "to the North"). Remarkably, the level of the water table is noted. Sprague's endless variations are adapted to the more common occurrences in the area under investigation. Thus the receptacle of the remains is indicated as animal hide, stone mound, basket, pot, cist, or wooden coffin; the disposal of the dead can be: reduction of remains, cremation, exposure, mechanical defleshing, other, unknown; final disposal: surface disposal, inhumation; localization of disposal area: isolated, in a group, delimited, not delimited, graveyard, ossuary, garbage dump; body preparation: stained with ochre, painted, dressed.

A special mention deserves the excellent form for C14 samples, based on the form provided by the LATYR, a C14 lab operating in Argentina.

3. Wadi Rayyan

University of Sydney's Wadi Rayyan archaeological Project (2003 - ongoing), directed by Jaimie Lovell, adapted for their recording the MoLAS system and the procedures of the Liverpool Museum Field Archaeology Section. Their draft manual for the first season of excavations at el Khawarij, in Jordan (Autori varii, 2005, internal unpublished document) groups contexts into three categories, cut, fill/deposit, structure, with sub-categories, e.g. for cuts: pit, post hole, cup hole, vat, etc. The description has three lists of parameters, for cut, fill/deposit and structure, with those not applicable being crossed out. Structures are described in terms of shape, orientation, dimensions, components, other features. As this project investigated primarily the development of tree crops (particularly olives) in the late prehistoric and proto-historic periods in the southern Levant, they made extensive use of archaeo-botanical samples (some 20 l were taken from most sediments), and that is why the sample section of the forms was expanded. It also lists find types (ceramics, lithics, faunal, ground stone, shell, worked bone, metal, other) and special find numbers. Contamination is listed as: modern, root, animal, other. An interesting prompt is that for the clarity of the interfaces, be it doubtful, merging, or clear. Context numbers are entered again on the back of the sheet, and this proves helpful when the sheets are photocopied. Also overleaf there is room for a sketch, for the interpretation and the matrix. While the matrix is not preprinted as empty boxes, the Wadi Rayyan sheets are unique to my knowledge in that a bar scale is preprinted (with the actual scale, e.g., 1:25, added in handwriting).

4. The Israel Antiquities Authority system

A full set of pre-printed sheets, primarily for rescue excavations, has been designed by the Israel Antiquities Authority, the governmental authority regulating excavations and the regime of antiquities in Israel. These forms draw heavily on the system used in Beer-Sheba by Y. Aharoni (1973). Locus cards are dedicated to stratigraphic units (contexts in MoLAS terminology). These cards are the only ones where among the heading infor-

mation (site, supervisor, and the IAA logo – quite prominently) one adds the excavation license number (upper left corner, a top priority position corresponding to the right corner of European and American sheets). Locus and type are also in the upper left corner. The season year is somewhat inadvertently mixed in between square and area. The locus "type" is simply one word (e.g. destruction, as in the Sussita excavations). These loci are assembled in "units" and then in "complexes": a locus is a floor, the unit is the room, and the complex is the building. The status of the locus is excavated or unexcavated; if the excavation of the locus is done, one also ticks "finished"; dates when opened and closed are also entered. There are boxes for period, stratum and phase representing ever finer datation of the locus. Period is the most inclusive one and often the only one filled in on site. Stratum is generally in the Near East a distinct level of human activity on a site, and it usually refers to a level that is seen in many places on the tell (what is called a "horizon" in Dor or an isochrone surface in Tell ed-Der), but in Beersheba it is used to mean phase, while in Tel Dor the sequence is phase, stage and stratum. The largest space on the IAA form is for a sketch, again of almost the complexity of a daily top plan. On the back of the locus form, the locus number and the type are entered again, which is an excellent redundancy device, useful when – as it generally happens – the forms are photocopied. A list of baskets follows, this time organized from right to left: basket number, level, "+-R", description (as in Beersheba, "+", the most common, hinting that some finds from this basket were taken to registration, "–" meaning the entire basket was discarded and "R" indicating the contents were kept for restoration.) IAA Wall cards indicate among the usual information, the adjoining floors (useful prompt also present on Tell Safi forms) and also mention inner surface, outer surface, core, coating, entrances/windows, secondary use, and "connected elements".

In Israel many excavations have experimented with the Aharoni system, and the result was an increased diversification of pre-printed sheets. This is also illustrated by the excavations recently directed by Yosef Garfinkel in Khirbet Qeiyafa. On the

locus forms from 2008, possible locus interpretation are pre-printed: top soil/surface; fill/occupational debris; pit; floor surface/earth; floor/stone paving; wall socle (stone); superstructure (mudbrick); street surface; hearth/oven; other (specify). The basket form has a number of interesting prompts. Thus, the Munsell colour is entered for the soil in both dry and wet states. For each basket, the "density of cultural material" is filled in, from none to high; basket and locus have the same relationship as basket and lot in Sardis, *v. supra*. In Khirbet, each basket is further split into buckets (the number of buckets is entered).

5. Lake Vouliagmeni

In his discussion of the pottery recording system from the excavations at Lake Vouliagmeni (Greece), as presented by Sedgwick *et al.* (1980), N. David (1982) argued that any recording system must 1. effectively help fulfill the specific aim of the study; 2. maximize the productivity of personnel; 3. minimize human error; and 4. allow others to subsequently restudy the data. These requirements are also applicable to recording stratigraphic units, not only artefacts. In their response, the team from Lake Vouliagmeni further elaborated on the traits of recording systems (Fossey *et al*. 1982). They state that the paramount duty of archaeologists, at least of those not working under the constraints of rescue archaeology, is to "retain as full and detailed a record as possible of all aspects of an excavation and of all material resulting therefrom" (247). They define recording systems as an organized method for gathering information, and propose a number of criteria for evaluating it. Efficiency comes first and is defined as a ratio of the amount of detail gathered (quantity of information per recording unit) and the speed of processing (time spent per recording unit); a system is efficient when it has a high rate of acquisition of information, that is, when it helps to retain much information in little time. Accuracy in turn is achieved when the error rate is low, and the amount of redundancy built in the system is appropriate. Objectivity deals with the minimization/exclusion of deliberate human bias in the recording of data. Fossey *et al.* (1982) also briefly discuss the criteria "spe-

cificity" (retaining that information which directly pertains to the study) and "generality" (retaining additional information that might turn out later to be germane to the study). They point out that some of these are conflicting requirements, as there is always a trade off between, for example, speed and level of detail, between the specificity and the generality of information acquired. One of David's objections, pertaining to his first requirement, was that the research objectives of Fossey's team were not clearly described, and therefore it was difficult to assess whether their recording system was adequate. In response, Fossey *et al.* (1982) affirm that the aim of their excavation was "to record as much information as possible" (242). This statement, also present in Hirst (1976), encapsulates a common problem with recording. Indeed, archaeologists should not dig in order to record, just as they do not live in order to breathe. Webster (1963) had explicitly warned against recording becoming a goal in itself.

6. Hirst's system (University of Birmingham)

Hirst proposed that the archaeologists record layers on white A5 cards, positive features (defined as those having layers build up around them, such as walls) on pink cards, and negative features (those which cut away through layers, such as graves) on blue cards. The layer cards open with "how defined" (in the sense of how was the layer distinguished from the surrounding matrix) and move on directly to interpretation. The presence of the interpretation prompt as first priority on the sheet, in Hirst's view, also has the advantage of forcing the supervisors to think carefully before allocating a layer number, discouraging them from using many numbers indiscriminately. Lucas *et al.* (2003) have argued, along the lines set out by Hodder (1999), that interpretation should be acknowledged by the archaeologist as preeminent in recording, rather than being made to "ensue" from a description made by the very same archaeologist whose interpretative abilities had been put on hold, so to speak. But Hirst makes a fair case in suggesting that the recorders should always makes sure they understand what they are recording, in other words, they do not imagine they will make sense of the site at the

end of the campaign by browsing through the paperwork. On the pre-printed sheets, associated layers are indicated (above, below, adjacent to), as well as associated feature (fills, seals, is cut by), followed by soil structure. The "clarity of horizon" asks for how clear the demarcation between this layer and the one above respectively below was – sharp: change in soil properties occurs within 25mm; fairly sharp: within 25-75mm; gradual, over more than 75 mm. On the back, general finds and recorded (small) finds are entered. Negative feature cards mention associated layers (cut into, sealed by, filled with) and associated features (associated with, cuts, cut by). Positive feature cards indicate associated layers (lies on, sealed by, associated with) and associated features (contemporary with, primary to, secondary to – the latter two concepts unfortunately left unexplained in the manual).

7. Combe Capelle

The Middle Palaeolithic site of Combe Capelle (Southern France), excavated by H. Dibble, M. Lenoir and S. McPherron, used a recording system presented by Dibble *et al*. (2003). They state (81) that the "field documentation … is perhaps the most important part of an excavation project". While their stratigraphic units closely resemble MoLAS contexts, a feature is at Combe Capelle "a nonportable artefact that cannot be removed from the matrix without destroying its integrity". There is some redundancy in this definition since an artefact is "nonportable" precisely because it cannot be removed without destroying the matrix. Also, how much an object must be modified by humans to become an artefact is debatable, making Dibble's definition applicable e.g. to a layer of earth brought in to level a surface.[10]

The Combe Capelle project did not use pre-printed forms, and this turned out to be regrettable because the trench super-

[10] Another widely used definition recognizes a feature to be any cluster of contexts that build up a coherent morphological or functional whole (Pavel 2010). For Hirst (1976:16) however, essential in defining features are their vertical dimensions, which distinguish them from layers which are always horizontal (with the exception of cuts).

visor's notebooks eventually exhibited a vastly varied understanding of what was worth recording and in what amount of detail. However, the team managed to enter all data in databases and published them (including the photos) on CDs accompanying the monograph volumes. The authors warn though that the archival life of electronic recording tools is not known and that it is not safe to assume that they will endure the fast paced changes in hardware and software for longer than 10-15 years. For their future campaigns Dibble *et al*. propose the use of "excavation level" forms (context sheets) and of feature sheets. These excavation level forms (as on page 88) are dedicated to a "stratum" or "level", defined based on changes in texture, moisture content, etc. in the sediment, and are kept short, with more information (e.g. interpretation of the results) being entered as daily entries in the field notebook. One of their distinctive prompts is the one for notes entitled "methods, observations, problems".

8. Mimbres sites

In the United States, a recording tradition started by Heizer (1953) culminated in modern systems such as that used by the Crow Canyon Archaeological Center (Autori varii 2001). A comparable system was used by the Mimbres Archaeological Center and Steven LeBlanc in the 70s (e.g. LeBlanc 1974). In the Mimbres system, as exposed in LeBlanc (1976), an excavation unit can be a room, a kiva, or an arbitrary trench; but the "fundamental recording unit" is the "excavation volume" […], "any volume of earth that is excavated, noted, and labeled as an entity". They are recorded with the number of the unit (in this case, room), the number of the locus (major subdivision of the units, such as half of the room), and the number of the level (subdivision of the locus not further explained): Unit.level.locus (e.g. 51.1.1, 51.2.1, 51.3.1 etc.). It may be seen as counterintuitive to not use a gradation from largest to smallest, such as in the more logical succession unit, locus, level.[11] Moreover, pits or burials

[11] On the Aegyptian site of Marsa Matruh, "deposits" are numbered e.g. "2.3", where "2" indicates the layer is the second in depth from the surface down, and

(they might be the "features" mentioned being at page 163 as being "simply another volume of earth that one wants to keep as a separate entity") found within levels can for LeBlanc also be loci, and will also be recorded in the same tripartite form, unit-level-locus. This results in the confusing situation where a locus consists of levels, and a level consists of loci. Artefacts are recorded by a "unique designation" consisting of site name, followed by the aforementioned tripartite scheme and an "item number". LeBlanc's observations pertaining to the efficiency of labeling systems also apply to the recording of stratigraphy. He suggests that they be evaluated by four criteria: information content, accuracy, efficiency, and computer compatibility. He emphasizes that a mislabeled artefact is possibly worse than an unlabeled one, as field archaeologists can certainly testify with regard to stratigraphic units.[12]

GIS and laser scanning in Schwartzbach and Cremona

The use of **GIS** to bring together the drawing and photo archive and the finds and stratigraphic data is becoming increasingly common. On some excavations (Athenian Agora) recording is done directly in hand held computers. On many more,

"3" means that it is the third spit/basket in that layer (White 2002:19-29). In the 70s, on the excavations of the University of Birmingham, fill layers of e.g. pit 6 in area A were numbered A6a, A6b. A 3-D recorded find from A6a was A6a/1. A skeleton found in situ in grave fill A8a will also be a recorded find A8a/1 (Hirst 1976).

[12] The excavations of the ancient Borysthenes, directed by Dmitry Chistov from the Hermitage Museum on the Berezan Island (Northern coast of the Black Sea) have pioneered the use of context sheets in North Pontic archaeology, and hopefully others will follow suit. Pre-printed sheets are also being used for now on the Romanian Coast of the Black Sea in Histria. In Berezan, Chistov uses such pro forma complemented by site notebooks, and a computerized database for the recording of pottery. Such a sheet (a *spisok*, i.e. list in Russian) describes contexts in the MoLAS manner. Interpretation can be chosen between arbitrary level (*uslovnyi gorizont*, spits taken in the absence of visible stratigraphy), stratigraphic layer (*stratigraficheski vydelennyi sloi*), with special importance being given to floors/pavements and fills of cuts. Stratigraphic connections (cuts, cut by etc.) feature prominently and are accompanied by graphic symbols, thereby diminishing the possibility of confusions in the field.

context sheets are at the end of the day entered into GIS powered databases; both pre-printed sheets and database are designed to be compatible with each other. A remarkable example of this kind is that offered by Framework Archaeology, a joint venture agreement between Oxford Archaeology (OA) and Wessex Archaeology (WA), founded to provide archaeological services to the British Airports Authority. I present here in a nutshell a less well known example, that of the GIS "Tacito" produced by the EGLUE company in Bergamo and used to record the results of the excavation of an aristocratic residence razed to the ground in 69 C.E. by Vespasian's troops in Piazza Marconi in Cremona (Passi Pitcher *et al.* 2009). These excavations from 2002 and then from 2005 on have produced some 7000 context sheets and about 1.5 million finds. The context sheet incorporated in the GIS is called "scheda di unità stratigrafica". It acts as a pivot around which the find and image archives are organized. It has standardized drop down menus, based on the sheets used in the field. The unit is assigned a "definizione" which is an objective description such as *cut* or *layer*, a "categoria" dedicated to the nature of the unit, basically an interpretation such as pit or leveling, and a "tipo" pertaining to the morphology of the unit, such as the form of a cut or the coursing of a masonry block. The box for "interpretazione" expands on "categoria" adding details of date and comments on potential uncertainties in interpretation. This is the dynamic part of the *scheda*, with arboreal structures allowing one to refine the description, for example from "wall" to "partition wall" to "partition wall with irregular courses". The fixed part of the *scheda* has a number of drop down menus for selecting the formation processes (natural, anthropic), contamination, samples etc., as well as a series of fixed prompts that stay the same for every unit, including those for site, year, number, etc. (which can be called heading information). The stratigraphic relationships are presented as a table, although as yet no direct links to the surrounding contexts were technically feasible; however from the *scheda* one has direct access to the finds, drawings, photos, and videos pertaining to that particular unit. These stratigraphic units are organized in structures and these in turn in

complexes, such as walls and floors being grouped into rooms, and rooms further into buildings. It appears for example from Fig 6. on page 766 that the room is the *struttura* and the house is the *complesso*. The term "complex" is not often used in the interpretation of a site; M. Carver was using it in Shrewsbury (1972-1974), where the hierarchy of units was layer/feature/complex (Hirst 1976); for the IAA (*v. supra*) this hierarchy is: locus/unit/complex. In Eastern Europe, "complex" is generally used to denote an ensemble of artefacts coming from a stratigraphic unit, or, more rarely, an ensemble of units in a feature.

One of the first applications of **laser scanning** in archaeological recording is presented by Doneus and Neubauer (2005). Laser scanners are used to three dimensionally record for all deposits their top and bottom surfaces, which together form "the immaterial and complete surviving hull of any deposit" (194). One of the major advantages of this procedure is that cross sections through the site along any desired line can be automatically reconstructed by the computer. Indeed, surfaces of important features on any site should certainly be 3D recorded. Scanning the surfaces of all deposits seems nevertheless unnecessary and for many digs also unaffordable for now. First, Doneus and Neubauer claim that the upper surfaces of deposits, which have been at one time exposed, account for far more time in the history of a site than the deposits themselves. Many examples can contradict this. All trodden surfaces, apart from the most meticulously maintained, increase their thickness with use. Middens and garbage dumps in settlements are often composed of numerous lenses, exposed perhaps for no more than a few hours, but whose material results from daily or weekly habitation practices. Second, there is not much reason for the recording to be much more accurate than the actual digging. The scanners used in Schwartzenbach near Vienna were set for an accuracy of 1 cm. Other than some very sharply defined layers and features, many archaeological layers cannot be exposed with this accuracy, for both technical and interpretational reasons. Recording them with a superior accuracy may tend to reify them. For example the point cloud images produced by the laser scanners transform the

tool marks of archaeologists/workmen in intriguing patterns of cultural traces, which may considerably puzzle specialists at a later point. To mitigate this skepticism however, it should be said that, from the point of view of the psychology of fieldwork, superior standards of precision in one aspect of the investigation (recording) might help to improve standards in other aspects (excavating). On the other hand, super-recording might bring about human carelessness. The archaeologist, once backed by ultramodern technology, may feel covered against any mistakes, or less responsible with having strict control over the interpretation of the site, since that, given the "perfect" (?) record, can be done at any other moment by anyone else. This deferral of interpretation (Andrews 2000) seems to be the main negative side effect of improved technology in recording. Interestingly, and although technological advances will certainly change that, laser scanning in archaeology as described by Doneus and Neubauer gives reasonably good results when the surface "has minor differences in height or is evenly sloping up" (195); in other words, this revolutionary recording of complex 3D surfaces works best when they are 2D. More intriguing is the authors' pedantic observation that, as compared to the work done by a team of two people, the laser scanner collects 50 times more data in 20% of time (203). One wonders how this was measured. A single scanning results in 2 million data points with x, y, z coordinate, color value and signal amplitude. Archaeologists, however, do not collect cloud points. Also, they do much more than measure coordinates, and reducing their work of data collection to the numerological jargon of instruction manuals does not do any service to archaeology as a human science. This said, it is certain that, as the pioneer work of Doneus and Neubauer shows, laser scanning may have tremendous applications in recording of interfaces.

4. Conclusion

I would like to conclude this article with a brief discussion of an imaginary archaeological excavation, one where archaeologists would have to record a site impossible to predict and

perhaps impossible to understand. This is undoubtedly never the case in practice, although many excavations are so highly charged with national, religious or "just" academic significance as to put considerable pressure on the shoulders of archaeologists. It is certain that the excavators of the tomb of Saint Peter, Vatican or the cave of Saint John the Baptist, near Jerusalem, or the excavators of mass graves in Bosnia or in Nazi extermination camps, have had to deal both with their own emotions and media pressure. But even in the cases invoked here, archaeologists still maintain some degree of control, no matter how surprising the discoveries are. One recalls how in 1997 in the Lorraine (France), rescue archaeologist Jean-Pierre Legendre retrieved from more than 4m deep a Lancaster bombardier, disappeared while returning from a WW2 mission in the Ruhr, with machine guns, engines, fuselage and crew's silk maps (Olivier 2008: 95-96). I will be using however a much more radical example as a pretext for exploring the intimate mechanisms of recording. In Stanley Kubrick's classical movie "2001: A Space Odyssey" (1968), astronauts are investigating an artefact, a huge black monolith, found buried on the moon. It is first identified by a magnetic anomaly on the lunar surface, and Kubrick shows the astronaut holding something similar to an archaeological geomagnetic map. They call the artefact a "Tycho Magnetic Anomaly" One, in what is a first classificatory attempt. One of the astronauts explains: "we thought it might be the upper part of some buried structure, so we excavated it out on all sides, but unfortunately we didn't find anything else" and continues: "the evidence seems pretty conclusive that it hasn't been covered up by natural erosion or other forces… it seems to have been deliberately buried". (An exchange from the original 1965 script coauthored by Arthur C. Clarke and Kubrick has not been used in the movie: "Floyd: how can you tell it was deliberately buried? Michaels: by the deformation between the mother rock and the fill"). The time of burial is ascertained (it is not clear on what grounds) to have been four million years ago. Kubrick could of course toy with the idea of artefacts buried by extra-terrestrial life forms, and get an Oscar and three nominations; any archaeo-

logist referring to such a hypothetic and sensationalist situation will more likely receive a letter announcing their exclusion from their teaching or research institution. While the "Space Odyssey" has never been intended as a parable for the archaeological profession, there is food for thought in the aforementioned scenes for any practicing archaeologist. Not when it comes to the excavation techniques involved: excavating a structure on all sides will of course destroy all stratigraphic connections between it and the surrounding layers, and the movie seems to predict that, if anything, in the future archaeology will go back to the methods of the 19th century. Also speaking of a "deformation" between bedrock and fill is muddled terminology. There is one thing left however, and that is the parable of excavating a structure of utter importance, of which we have no idea in advance what it could be, and in what circumstances it was buried. It in fact raises the question of how many of us ever think, when going digging, that we are excavating something that humanity is crucially interested in, and something that defies all our preconceptions. I am here mainly interested in recording (which, as expected, is not even mentioned in the movie - here for once Kubrick got the spirit of excavation reports right). In connection to what could be the most important archaeological excavation ever attempted, as in Kubrick's movie, one would want to ensure that the record of the excavation is nothing short of perfect. But how do we record clues we have, this time more than ever, absolutely no idea about? How does one record something one cannot in the least make sense of? To put it bluntly, it would be interesting to ask archaeology undergraduates what kinds of context sheets they would use for the excavation of a 4 million old artefact on the moon. (Let them stop saying that archaeology does not prepare one for a real job!). Even if stratigraphic relationships will stay the same, can one record the traces left by utterly different technology put to use for utterly different purposes? Can we assume that the lunar monolith was buried there just the way the foundations of a heavily mortared Late Roman wall are buried in a foundation trench? Will our context sheets still retain prompts such as: is the deposit water sorted,

was any flint debitage retrieved, what is the percentage of misfired pottery? The only relevant parameter for understanding how the artefact was buried might be the magnetic field of every cubic centimeter of lunar sand, or, say, the reactivity of each grain to strontium. Traces of what fuel, or of what food, might we be prepared for, if such categories are at all applicable? The keys to the mystery are certainly there, but can we notice them? There are always millions of variables that might be recorded, and we only record those that we anticipate might be somehow meaningful (Hill 1972). If we cannot imagine the explanation, or at least approximate the paradigm that it is in, we cannot work toward it, we cannot gather the data from which, after the correct inductive and deductive manipulations, it might emerge. Therefore recording, just as any gathering of data, is correlated with pre-understanding, with hypotheses, and research objectives. And it should be done in such a manner as to serve our own, and ideally others', research objectives, making room for the unexpected and tying it into a profound understanding of the site.

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