

New Europe College Yearbook 2014-2015



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ISSN 1584-0298

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Novels:

Aproape a șaptea parte din lume, Humanitas 2010
Nicio clipă Portasar, Cartea Românească 2015

TRANSLATING OBJECTS INTO WORDS AND IMAGES: METHODOLOGICAL OBSERVATIONS ON THE BLENDING OF EXCAVATION AND TEXTUAL DATA

This article focuses on issues of knowledge migration across disciplinary borders, and represents an extended case study in “mode 2” knowledge production (Gibbons et al. 1994), which presupposes multi-disciplinary teams working to solve a specific research problem. Within this theoretical framework I am analyzing the metamorphoses of archaeological knowledge when incorporated into historical narratives and blended with historical knowledge, and when used, together with the latter, to produce so-called “realistic” visual models of ancient monuments. I attempt to redefine archaeological epistemology from two different, but converging perspectives, with similar underpinning issues of knowledge transfer and compatibility. The aim is to produce a theoretical device that may help to bridge the conceptual gap between different categories of data, between e.g. marble tesserae found in the trench, the talk of luxury in literary sources, and reconstructions of baths with opus sectile decoration. The urgency of the topic also comes from archaeology’s new approach to outreach, as well as from the heated debate around the impact of interdisciplinarity on archaeology today. Importantly, the two paradigms, textual and artefactual, must be analyzed in terms of commensurability. It is often assumed that they are directly comparable, where in fact they speak different languages. A middle-range theory is needed to overcome the issue of the perceived incommensurability between them. The main case study used is that of the ancient site of Troy.

1. The Problem

Tourists feel edified when they see in a museum room dedicated to Bronze Age Troy an array of loom-weights, clay hanging weights to keep

the threads taut, accompanied by captions reminding them of the scene in the Iliad where Helen is found weaving by the goddess Iris. There is a hidden dimension to this kind of “brilliant match” between archaeological and literary sources. Indeed, archaeologists read Homer long before they get to put the spade in the ground, and may be already convinced that there was a tradition of weaving at the site. Under the pressure of fieldwork, and of prestigious literary texts, is it possible that the artefacts called loom-weights somehow have more visibility for them, that they involuntarily look for them or privilege retrieval of fragmentary ones? And then historians, hearing about these archaeological discoveries, might in turn also become convinced Homer was vindicated, and Helen weaving was not just a rhetorical common-place for assigning gender roles...

2. Status Quaestionis

When creating a methodological framework for blending archaeological and literary data, one draws on a conspicuously fragmented body of literature. The very existence of a problem, namely that the integration of artefactual and documentary data is fraught with uncertainties (biases, circularity, self-fulfilling prophecies) was not duly acknowledged until comparatively recently. It has seemed for long self-understood that the much younger discipline, archaeology, should be a handmaiden to age-old history, and merely illustrate it (Hume 1964, Moreland 2006). A perception of archaeology as ancillary to history is in fact perhaps the most enduring misconception throughout the development of the discipline, and archaeologists repeatedly protested against this “tyranny of the historical record” (Champion 1990, Small 1999). It may be that revolutionary texts such as Rostovtzeff’s *Social and Economic History of the Roman Empire* (1926), which used, on a scale perhaps never encountered before, archaeological artefacts to illustrate a narrative still culled primarily from written sources, may have, as a side-effect, confined archaeology to playing this role in perpetuity. Indeed, the use of archaeology in a majority of ancient history books and journals still does not go substantially beyond this view, as Lloyd (1986), Martin (2008), and Hall (2013) have shown. Kemp (1984:21) pointed out that archaeology still supplies “garments, baskets, razors, sandals – all the props needed for the costume reconstruction of ancient life”. Among explanations for this we should count the fact that archaeologists are still “trapped by the

agenda set by historians" (Austin 1990), and in general that the questions asked of the material remains are in fact those normally asked of texts (Allison 2001:181). Classical archaeology's reluctance to indulge in theory-building regarding the relationship between "word" and "dirt", of course, did not help the situation.

It is Cambridge scholars who initiated a debate that was to bring about a loss of innocence in this respect, starting with Finley's (1971) disappointment in archaeology not having made much progress since Rostovtzeff. The former denounced the academic creed according to which statements in literary or documentary sources are to be accepted unless they can be disproved, and that material culture matters mainly in so far as it can support or falsify the literary tradition (Finley 1985, Hall 2013:207-9). In turn, A.M. Snodgrass (1987) exposed archaeologists' "positivist fallacy", a tendency to mechanically equate what appears to be significant in the archaeological record with what appears to be significant in the textual evidence. A layer of ash must be due to the Dorian invasion, some Cimmerian raid, or attack of the Goths. This is tantamount to making archaeological and historical prominence interchangeable. This fallacy is particularly easy to perpetrate given the fragmentary nature of both the archaeological record and of the historical sources.

The dissatisfaction with this state of affairs resulted in a desultory quest for solutions, although those found were rarely incorporated in actual research programmes. Summarizing an array of disparate suggestions in the literature, it could be said that the main solution, appeared to be a two-pronged approach. Firstly, one would make sure to analyze archaeological and historical sources independently (Leone 1988:29, Miller 1991, Andr n 1998, Storey 1999:232, Galloway 2006, Hurst 2010; Hall 2013:208). While this hopes to avoid circularity in argumentation (Kosso 2001, 81-90), the question as to what guidelines to follow in order to know when can the confrontation between the two lines of evidence begin remained unanswered. In a second phase, one would bring together the strands of evidence (pursued separately so far) and confront them. The priority for investigation would then become those aspects of the human past for which the archaeological and literary evidence contributed contradictory statements. Such contradictions have been in turn named inconsistencies (Allison 2001) contrasts (Andr n 1998), incongruities (Little 1992), ambiguities (Leone 1988), disjunctions (Carmack and Weeks 1981), deviations (Leone and Potter 1988), and dissonances (Hall 1999). Overall however, the insistence on degrees of independence between what,

since Hawkes (1954), was called text-free and text-aided archaeology (cf. Little 1992, and Young 1992 “text-misled”), did not really escape this reasoning loop. Subsequently, apart from the model above, a plethora of suggestions were published as to how to bridge the great divide between artefact and text, necessarily reflecting various habitus-bound research agendas. Occasionally, we find gut-feeling solutions, advocating a focus on a certain area of study, perceived as privileged for the deconstruction the dialogue between archaeological and literary data, such as the study of ancient materials and technologies (Martín-Torres 2008) or that of cult and religion (Dever 1991). The type of instinct needed to identify them is not further elucidated. On a more general level, Andrén (1998:146 sqq.) and Martín-Torres (2008) have argued that in order to sidestep a philosophically unrewarding study of concepts (artefacts, texts), we could analyze best-practice strategies, in other words how concentric contexts for each of the two are crystallized in practices. Others yet have found redemption in bringing into the limelight long-term quantifiable change, since that is precisely what texts cannot do (Hurst 2010), in using one category of evidence for deriving hypotheses to be tested within the other category (Little 1992), and in a sort of “formation process” analysis, geared towards understanding how texts and artefacts come to be and how they come to shape society (Galloway 2006, Moreland 2006, Martín-Torres 2008). Some of the solutions on the other hand further obscured the original problem, such as the idea that archaeology and history speak “different languages” (e.g. Ahlström 1991), when in fact the two types of evidence are rather incommensurable, in Kuhn’s sense.

A more metaphorically-minded cohort of archaeologists drifted away from the archaeological arena in search of solutions. For example, Martin (2008) proposed to achieve a proper “dovetailing of text and objects” by using cultural semiotics and thick-description ethnography, while Galloway (2006) resorted to Bruno Latour and Appadurai for actor-network theory and the social life of things to help construe the relationship between texts, artefacts, and society. Finally, where processual archaeologist spearheaded an increased, at times indeed total, independence from written sources (Arnold 1986), post-processual approaches then focused on hermeneutics (Leone 1988, Dyson 1995 on the archaeological site as an Urtext; also post-ironic perspectives in Isayev 2006).

The wide geographical and chronological coverage of the works tackling the subject must be underscored. If they did not build up to the critical mass needed for a problem specific methodology, they still testify to

tectonic movements through the whole of archaeology today. Such works deal with medieval archaeology (Austin 1990, Fehring 1991:229–237 focusing on Germany, Young 1992, and Tabaczyński 1993), prehistory (Bennet 1984 and Snodgrass 1985a for LBA Aegean), Classical (Storey 1999, Snodgrass 1985b, Lloyd 1986), Biblical (articles in Levy 2010 and Vikander 1991), and North American archaeology (Leone 1988). Granted, the debate is not so developed in areas where traditionally written texts have had a crushing impact (Egypt, Kemp 1984, Mesopotamia, Ellis 1983). As for Anatolia, it incited texts which, despite their fine scholarship on e.g. the Trojan War (Vanschoonwinkel 1998, Benzi 2009, Rose 2013), did not set methodology as their primary goal. Despite the substantial number of contributions then (although this is still small compared to most other provinces of theoretical archaeology), they are not yet articulated in a coherent whole. The concern remains that many only pay lip service to what promises to be a noble critical stance, and moreover, they reiterate a relatively small number of tenets – hence an overall sense of quixotic effort.

Currently, the most promising avenue of investigation, as reflected in the literature, seems to be to revisit the relevance of multidisciplinary for the issue at stake. The term multidisciplinary remains oddly uncomfortable when discussing archaeology and history, as if digging in the ground and reading Tacitus was in fact done with fundamentally similar methods and objectives (cf. Martínón-Torres 2008). As D. Austin (1990:13) put it, whenever archaeologists attempt to deliver an independent historical narrative, they are accused by historians of “at best, irrelevance or lack of scholarship, and at worst of uttering jargonistic claptrap”. Nevertheless, there are now enough voices pleading for the integrated production of historical knowledge, and even though the multi-disciplinary approach is “the most difficult, the most susceptible to superficiality, [...] it is still the most productive” (Carver 2002:490; Austin 1990). Blending archaeological, historical, and scientific knowledge is championed among others by Martínón-Torres (2008: 33) with the rationale that thereby the ability of the respective specialists to understand past societies is “multiplied exponentially”. Despite the reservations of Isayev (2006), the so-called mode 2 knowledge production (Gibbons 1994) is bound to offer the best understanding of the phenomenon. Some archaeological applications of it exist already, as in the multidisciplinary project (not without its own cautionary tales) described by Rankov (2004).

In conclusion, what Arnold (1986) noted in the eighties is still true today: researchers piously advocate the integration of the categories of

evidence, but a few are doing it and fewer are explaining how it should be done. The majority endorses (tacitly) the view that after all archaeological evidence is mute and cannot be understood without written texts, or a variant of this view (Allison 2001), and that the role of archaeology will never be more than marginal in the historical reconstruction (e.g. Lloyd 1986:42). Small (1995) deplored that the archaeological record is still seen as a “subordinate dynamic context for viewing textually-adduced reconstructions”, and sure enough some archaeologists still claim (notably Rainey 2001:148) that one ought not to use “subjectively interpreted archaeological data” to contradict written sources. The problem is not that the same archaeological data is invoked for antagonistic interpretations (Miller 1987) – plurality of interpretations has never been the problem – but that scholars proceed with a “forced harmonization” of archaeological and historical data (Ahlström 1991:119).

For a couple of decades prehistoric and Classical archaeology have been increasingly unhappy with this forced harmonization, but the situation remains that described by G.R. Storey at the turn of the millennium, namely that “a balanced, dependable method for integrating textual and archaeological data is still lacking” (Storey 1999:206).

3. (In)commensurable Paradigms

Matching excavation and textual data to produce historical reconstructions can easily result in circularity or in coalescing data that may be in fact incommensurable. This article stresses the future opportunities in the dialogue between the fields. As Stefan Hauser (2005, 94) put it, this collaboration between archaeology and history “is not a mere reconciliation of two accidentally and wrongfully separated components of classical and ancient studies, but an interdisciplinary negotiation between two theoretically and methodologically mature disciplines” (my tr.).

As an extended case study in “mode 2 knowledge production”, this article will showcase the complex interplay between archaeological and historical data. Compared to the sophistication with which ancient historians discuss literary data, their approach to archaeological reports in order to harvest usable knowledge is often perfunctory and reductionistic (Carver 2002, Moreland 2006). Indeed, a whole catalogue of judgment errors with respect to the simultaneous use of archaeological and historical

data could and should be compiled, and this could be the basis for establishing a set of guidelines for the integration of such data.

Above all, archaeologists and historians must address the need to epistemologically legitimize the corroboration of archaeological and literary data. Partial solutions must be sought at the level of theory formation. The research hypothesis here is that contrasting, rather than merging strands of evidence is the more productive approach. It is not sound method to employ texts to understand what archaeological finds mean, at the very moment when one is using artefacts to understand what the texts are referring to. The idea that archaeology simply fills in the gaps in the historical record remains superficial. The building blocks of archaeological narratives ought not to be expressed in terms borrowed from a textual paradigm. The solution to the incommensurability of archaeological and historical data is twofold.

Firstly, one can begin to match two different categories of evidence, and evaluate possible discrepancies, only when satisfactory, if perhaps incomplete, explanations can be given of either category solely on its own terms. These discrepancies between archaeological and historical data can then be used to prioritize research topics. Curiously, there is no in depth discussion as to how such “contrasts”, to use Andrén’s terminology, can be used as markers and catalysts of further rapprochement between archaeological and historical narratives.

Secondly, the solution resides in a change of approach to historical reconstruction. Instead of merging these categories of evidence in the fabric of an omniscient narrative, one would be best advised to constantly tell two intertwining stories, an archaeological and a historical one, each one using the other as foil. One would make sure to analyze archaeological and historical sources independently (Leone 1988:29, Miller 1991, Andrén 1998, Storey 1999:232, Galloway 2006, Hurst 2010). This is not to say that inscriptions found by archaeologists should not be used by historians. The point is that a purely archaeological reconstruction of occupation phases at a site is fundamentally a stranger to the same reconstruction culled from written sources. The objective here is tantamount to a reconceptualization of historical reconstructions, emphasizing the role of archaeology in holistic historic narratives, as well as the kudos and caveats of visual models of past monuments (v. *infra*).

I argue that history and archaeology can be re-cast as “science in the context of application” (Carrier and Nordmann 2011), where objectives are formulated from the outset within a dialogue between

scientific stakeholders. In so doing, one should investigate not only how society-specific knowledge claims are raised, but for whom knowledge is in fact produced. The main hypothesis here is that the study of the collaboration between historians and archaeologists, often joined, e.g. during experimental archaeology projects, by scientists is a multidisciplinary effort best understood as “mode 2” knowledge production (Gibbons 1994). Researchers negotiate and produce knowledge in a non-hierarchical manner. None of the disciplines dictates the framework. Quality control is a function of disciplinary rigor combined with social accountability, consensus formation, and constant negotiation of the results (Nowotny *et al.* 2001, 2003). Reflexivity is a characteristic of this process since the result is not scientific truth, but rather socially-viable knowledge: not objectivity as a view from nowhere, but a network of views managed by knowledge brokers. The literature offers various examples of case studies of multi-disciplinarity involving also scientists and not just historians and archaeologists, from re-building a Roman bath (2003) to the re-building of a Greek trireme (Rankov 2004). A secondary hypothesis of interest is that knowledge is actually shaped by the process of knowledge dissemination. Outreach will have to be considered as one of the new important resources for the creation of archaeological knowledge, articulating an epistemological interface between archaeology and history. In the future, teams of archaeologists could envisage a pilot programme with the avowed goal of critically monitoring for pressure from literary sources during fieldwork; in this direction, anthropologists have supplied a starting point in the “ethnography of the dig”.

4. Case Study: Troy and the Trojan War: Between the Tyranny of the Text and the Archaeological Bias

The central case study for creating a methodological framework for the corroboration of archaeological and textual data will be in this article the site of Troy (Hisarlik) sung by Homer and excavated since the 1870s. The question I ask is how can scholars make use, in an epistemologically legitimate way, of textual data to obtain and interpret excavation data, and conversely, how can they make use of archaeological information to contextualize literary sources. This is important because, as outlined above these two sets of data are, at best, conflicting, and at worst, belong to incommensurable paradigms. For major sites such as Troy, literary

information (in this case mainly the Homeric epics) appears to actually bend archaeological data in a force field, while the biases which have traditionally characterized the archaeologists' work seem more acute than elsewhere. To locate Troy in Finland, or, as done by Schrott in 2008, to make Homer a eunuch scribe writing about the Assyrians, cannot be explained simply by the so-called underdetermination of theory by data in social sciences in general. Such attempts must be seen as pertaining to fringe archaeology. But even academic archaeology, in the city on the Dardanelles, suffers under "the tyranny of the text" (Small 1999). Without the knowledge of, and reverence for, Homer's text, Schliemann, Blegen, and even Korfmann in the past decades would never have argued, solely on the basis of material remains, that the site was burnt and sacked by the Mycenaeans. Indeed, they would have never been interested in excavating the site.

As already seen, the traditional approach was to posit that archaeology is ancillary to history: history gives us the framework, archaeology fills in the details. Not only does this result in unacceptable biases, but, as is well known, the archaeology of provinces and the results of regional surveys are often at odds with texts produced by the elites of the center. Historical sources on provinces, interfaces and "contested peripheries", caught between larger states (such as Troy and the Aegean-Anatolian Interface between the Mycenaeans and the Hittites) are the locus for the creation of otherness and therefore notoriously distorted. Troy is by no means unique in this respect. Another example among many is the site of Gordion in Central Anatolia, whose chronology was previously built around classical and Neo-Assyrian literary sources, and made to depend on events such as the reign of Midas and the invasion of the Cimmerians. Recent excavations in Gordion, however, have determined that the Gordion Iron Age was in fact up to two hundred years earlier than originally thought (Rose and Darbyshire 2013).

From an archaeological point of view, Troy must be liberated from the obligation to answer Homeric questions. Troy's importance in modern research is given not by its role in Homer, but quite simply by its uninterrupted bi-millenary Bronze Age sequence, fundamental for reconstructing the life of settlements in the Interface. Archaeologists are made to approach Troy with questions which are external to the site: has there been a Trojan war as described by Homer? But prehistoric archaeology's answer to this question is, by and large: it doesn't matter. What matters is to give answers to more valuable related questions

whether site-specific (what were the funerary rites of the Trojans), or more general (why did the Hittites not attempt to expand further West into the Aegean-Anatolian Interface, and what drove the Mycenaeans' expansion there?). However, while we cannot look at Troy through Homeric glasses, we should also not shun a periodical confrontation between our archaeological models and information from literary sources. Ideally, one would also be able to see how archaeological evidence bears on Homeric issues, but without making this an overwhelming priority. The problem is that whenever Homer describes events, our assumptions begin to be rooted in both literary and material sources, with the danger of circularity.

Tentatively, improvements in how we produce this final narrative at the junction between archaeological and historical data can probably be brought about by exposing archaeologists to theories of scientific reasoning, including bias literature and epistemic rationality studies. Also, studies of "bounded rationality" suggest that our heuristics "in the field" must be frugal in what they take into account, fast in their operation, and fit to reality (Gigerenzer and Goldstein 1996). Field archaeologists, even more than other social scientists, are indeed quite prone to use "fast and frugal heuristics", often under the guise of biases. Biases, in turn, can be defined as recurring cognitive errors with epistemic value, cognitive adaptations for decision making (Tweney and Chitwood 1995). In the paradigm of fast and frugal heuristics, biases are perceived as having the potential, under certain circumstances, to make one a more effective decision maker. Such biases are exercised both under the pressure of actual excavation, "at the trowel's edge", and, much less justifiably, in the publication of final results. Among a score of examples: the clustering illusion, accounting for the perception of patterns where in fact the data has been randomly generated (Pavel 2011). If we have a better understanding of how archaeologists think, we should be better equipped to make sense of their data and integrate it with literary data. Designing the much-needed methodology of this encounter, in fact a real data clash, archaeological vs. literary, is again hampered by the problem described above: the unsound method of employing texts to understand what archaeological finds mean, at the very moment when one is using the artefacts to understand what the texts are referring to. And how can we make sure that our biases are at least not cumulative? Clearly, evidence of one type might be put to better use in constructing modes to be tested within the confines of the other type.

The Destruction Layers in Troy

How does one treat archaeologically a war which has been dubbed “la guerre fantôme” (Benzi 2009)? If no real event should actually be imagined behind Homer’s war stories (as wanted by Carpenter 1946, Finley 1964, Hertel 2008), then archaeology must “limit” itself to describing the very general context of war techniques, psychology etc. in the Aegean and W Anatolian world, with further SE European and Near Eastern ramifications in the four centuries that separate Homer from the war he purports to describe, since all and none may have played a part in his description. Homer’s story of the Trojan War may incorporate echoes of any of the fighting done around Troy and anywhere on the coast between Mycenaeans (together with some of the Sea Peoples?) and the Arzawans or Hittites or their (former) vassals, including the conquest of Mycenaean Miletus by Mursili II. But if Homer was speaking of a real war fought in a real city, and if that war left recognizable traces in the archaeological record, then archaeology should be able to identify them as destruction layers.

In the present state of our knowledge, Troy exhibits a number of destructions from the end of the 14th c. to the end of the 10th which would be potential candidates for such a Trojan war (for an excellent introduction on the archaeology of the Trojan war, Vanschoonwinkel 1998, Wiener 2007, and Benzi 2009). I will present here the situation for the Late Bronze Age.

Troy VIh, the largest Trojan citadel, came to an end in a severe destruction (archaeological indications and interpretation in Blegen, Caskey and Rawson 1953, 89-92, 98, 262, 329-332; 1963, 143-145, 147; Hiller 1991; Mountjoy 1999a). The upper part of the fortification wall as well as the superstructures of the large houses that stood inside the fortress collapsed, and damage to towers VIh and VIi, as well as subsidence of the fortification wall is noted. No house appears to have burned and no human victims have been found.

The dating of this destruction is based on Mycenaean pottery, the chronology of which is constantly being refined. The most recent thorough reassessment was done by P. Mountjoy (1999a), who dates this destruction to the end of LHIIIA2, around 1300 (a little earlier for Benzi 2002, 352). Blegen, Caskey and Rawson (1953) and Mountjoy (1999a) are definitely for the earthquake interpretation, and so was Korfmann (occasionally with some reserve, e.g. 1995). They often use the fact that Troy is not

impoverished after this destruction and its culture remains the same in the ensuing phase VIIa, when also relationships with the Mycenaeans are maintained, as an argument against the interpretation of the destruction as a result of war (with the Achaeans). For Rose (1998) however there is no uniform agreement whether this was due to an earthquake, an invasion or both. Other researchers see Troy VIIh as destroyed by war (Allen 1994, Bryce 2005), either by the Achaeans (Mellink 1986, 100) or by the Hittites and their allies (Basedow 2007).

Troy VIIa came to an end with another important destruction following a period of “tell-tale” changes in the life of the settlement (archaeological indications and interpretation in Blegen *et al.* 1958, 10-13, 51; Blegen 1963, 147-164; Hiller 1991; Korfmann 1996, 2002, 2003; Vanschoonwinkel 1998).

Before describing the destruction layer, it is appropriate to note in the archaeological record some signs of a potential threat or “emergency”. These may include the substantial additions to the fortification wall, especially by the East gate. Small houses are now crowded closely together abutting the inside face of the wall, where streets used to run before. Unprecedented in VIIa is that almost every house (e.g. 730 and 731) had numerous *pithoi* sunk to their full height (1.75 - 2m) in the floors, with the rim covered with stone slabs. They maximized the storage capacity to the point of honeycombing the floors on which people walked. It would thus appear that the acropolis was obliged to shelter a larger population than in Troy VI. The excavators of 1932-38 argued that in this period the quantity of imported Mycenaean material plummeted. In addition to these findings by Blegen’s team, the excavations after 1988 showed that the SW gate VIU, found in 1995, the largest gate in the wall of Troy VI, was blocked before the destruction, at the same time when a little street running up the East gate VIS is dismantled, and when a mudbrick bastion is reinforcing the area around the NE bastion.

As to the destruction proper, Blegen mentions great masses of stones and crude brick along with burned debris found in a destruction deposit up to 1.5m high. A man, considered a war victim, was covered by debris on the western slope of the hill outside the acropolis wall, and the bones of another individual appeared to be found in house 700 as well as outside in the street, with other victims being suggested by a lower jaw bone in house 741, skull fragments in street 711, etc. Blegen found almost no weapons in the citadel (the bronze arrowhead in street 710 was Finley’s

object of ridicule), but Korfmann's excavations have added to this picture more victims, for example the hasty burial of a girl with burnt feet. Also, the so-called *Terrassenhaus* outside the citadel, the only house of VIIa which could be almost completely excavated, was destroyed at the end of this period. All its rooms burnt in a "devastating fire". The excavators found arrow points and spearheads (Koppenhöfer 1997 dates them rather in VIIb1), and many sling stones in almost all rooms. But most importantly, in the burnt layer on the street South of the *Terrassenhaus* 180 sling stones were found in piles, of which one pile of 121 ("deposits of unused weapons", Korfmann 2002, 216; problems with the interpretation, Benzi 2002, 354-5, Hertel 2008, 199 n. 23, Vanschoonwinkel 1998, 244 n. 93, the latter noting that the Mycenaean arrowhead found by Blegen is attested in Pylos, "mais aussi, exceptionnellement, à Alishar [Hüyük]").

Mountjoy (1999b) reanalyzed Blegen's Mycenaean pottery, which had not been reexamined since 1958, and dated the destruction of VIIa to "late LHIIIB and probably in transitional LHIIIB2-IIIC early, but not later", around 1190/1180 (similarly Mellink 1986, 94, Vanschoonwinkel 1998, 243: second quarter of the 12th c.). While the interpretation as war is upheld by a majority of researchers, Blegen (1958; 1963) was decidedly for the Trojan War. But the fact that the invaders could have been Mycenaean is neither confirmed nor contradicted in any way by the archaeological record. No Mycenaean weapons have ever been found at Troy. In fact, the closest location with Mycenaean weapons South of Troy is Pergamon (Niemeier 2006, 54). Mountjoy (1999b) and Mellink (1986) attribute the VIIa destruction to the Sea Peoples (along the same lines, Finley 1964, 4-6, further discussion in Vanschoonwinkel 1998, 250).

A similar analysis can be done for the three partial destruction layers of Troy VIIb (Pavel 2014).

It should however be borne in mind that we lack general criteria to determine when a destruction will appear serious enough in the archaeological record to be considered a lost war, and equally one cannot import criteria from somewhere else, e.g. expect to see Troy's VIIa record replicate exactly the destruction of Beycesultan in mid-12th c. or the Achaemenid destruction of Sardis in 546. Some of the destruction layers in Troy presented above are not reported from large enough (by archaeological standards) areas of the site, most of them cannot be safely (also by archaeological standards) associated with war, and none of them can be, on archaeological evidence alone, suggested to be the result of war with the Mycenaean. In fact, for at least one of them, the Trojans

seem to be something other than the Anatolian people Homer is talking about. Thus, for all we know, one of the Trojan Wars may have been fought between the Sea Peoples and the Anatolian Trojans, or between Achaeans and Thracian immigrants.

It is only for the general public, that the most important thing in the Iliad is the Trojan War, or what is left of the real Trojan War in the Iliad. For the historian, almost every word in Homer, even the emotions of his gods and goddesses, is something that is “left”, only from different historical strata. Homer was not a Dadaist poet or a Cubist painter. There was no intrinsic merit for Homer or his audiences in lack of logic or random invention, as there may have been much later for only a handful of ancient writers such as Lucian. Deformations must certainly be reckoned with in the Iliad, but only meaningful ones underpinned by a rhetoric or ideological rationale. In that sense, the problem becomes one of identifying what triggers these deformations (primarily the task of the historian, Petre 1982), and being able to map, with a reasonable chance of success, what material culture elements stem from which time period and geographical area (as shown by the work of Lorimer as well as of Matz and Bucholtz). Most probably, an analysis of the Trojan war must begin to take shape at the intersection of the two.

5. Visual Models in Archaeology – From Piranesi to Google Earth

The analysis of the interplay between archaeological and historical sources must be complemented by a discussion of how digital models of ancient sites draw on both categories of sources. A good example of this could be the media hype surrounding the 2001 Troy exhibit “Dream and Reality” in Stuttgart, Germany (Troia-Ausstellung, Traum und Wirklichkeit), which was both a success to judge by the number of visitors (almost a million) and an occasion for controversy and scandal. M. Korfmann, the excavator of Troy and initiator of the exhibition, was at that time accused to have presented reconstructions that were not entirely backed by excavation results, and to have gone beyond the liberties any reconstructed model was entitled to. In the present article I will however not be dwelling on the details of this controversy, but rather present the larger theoretical framework for such analyses.

Behind the popularity of digital models of ancient monuments there is more than just an interest in the past and a fancy for elegant design.

As scientists, we are indeed dealing rather in the past-as-reconstructed, than in that utter stranger, the past-in-itself. But science-aided (virtual) experiences are today marketed as the only ones which can impart the feeling of authenticity. The displaced materiality of the models is promoted to be the only one capable of doing justice to the mathematical manifold that nature has become. This is rooted both in Husserl's phenomenological analysis of the crisis of science and in the neopositivist conception of science, and is allegedly pushed to the extreme by a compulsive drive to show off computer capabilities. The current conception of virtual reality has as a result a disembodied viewer who has become ubiquitous and omniscient. This seems a vindication of Husserl's conception of the model achieving a higher ontological status than the reality (the *Lebenswelt*). At the same time it paradoxically illustrates another philosophical stance, Baudrillard's view that reconstruction is a simulacrum, "a truth concealing that there is none", reflecting the "characteristic hysteria of our time: the hysteria of production and reproduction of the real" (1988, 166, 180). Digital models in archaeology may have become Baudrillard's (id. 166) "maps that precede the territory", in a world of scientific illustrations where original monument and its reconstruction are mutually constitutive. In an age of simulation, witnessing the liquidation of all referentials, models have become more real than real, and indeed their hyperreality is the only reality that past monuments can enjoy. In the recent philosophy of science, N. Gray further argues that virtual reconstructions are generally perceived as erring on either side of reality, either almost but not quite real, or, on the contrary, more concentrated than real (i.e., perceiving either the model's "deficiency" or its "intensity", Gray, 1995 (with n. 1, p. 347).

Key in the discussion of visual reconstruction in archaeology today is the notion of realism. The dominant view in archaeology today is that digital models "bring the past back to life". The proponents of this view would be of course otherwise bound to dismiss as naïve any historian who set about showing history "how it really was". What are the underpinnings of this generous naivety when it comes to digital reconstructions? Models have become indeed increasingly convincing over a very short while since the advent of computers, and it is easy to forget that the same reservations that applied to the historical narrative should now be applicable to such visual reconstruction. No epistemological vaccine was ready in time to protect against the sudden rise of astonishing and compelling computer models. The graphic pizzazz of such models downplays or completely conceals that any number of educated guesses or pro domo choices go

into such models (Kantner 2000), and that visual reconstruction is not the restitution of the past, but a present theory of the past (Moser and Gamble 1997). Moreover, if the public is expecting to be walked through the past, they will always be disappointed by the model's inability to ever breathe actual life into the model. It has been pointed out that the authenticity of a model is not about verisimilitude, but rather about process, biography and embeddedness (Gillings 2005, also Mesick 2013, 66). In fact, the realism of such reconstructions is judged against photography, so in fact it should be called photorealism, reifying vision as the means to evaluate the world (Gillings 2005). Architects such as J. Pallasmaa have criticized the hegemony of vision in the appreciation of built (reconstructed) structures. Realism as a criterion for evaluating success should be discarded if understood as mere visual, photorealistic (as opposed to plurisensorial, contextual, and functional) agreement between original and reconstruction, without considering "the life for which the original was intended" (Yegül 1976, 171-172)). One of the ensuing paradoxes is that the more realistic the model, the less it helps towards research questions (cf. Kantner 2000, 52). Moreover, accuracy, verisimilitude, or realism can in fact only be assessed as a function of purpose, and often this involves the consideration of the intended audience. Is the reconstruction focusing on the building's earthquake resistance, or its daylighting analysis; accompanying a traveling exhibition; being used in undergraduate courses; or currying the favor of excavation sponsors? Between consumption, teaching, and research, it is goals which restrict the infinite task of reconstructing, and modelers must choose how their restrictive interpretation will operate. The aspiration to be realistic has also resulted in models using real photos of dramatic skies over the digital Roman Forum model, or real water videos in Hadrian's pool, while at the same time having the viewer fly through the model on a Wimbledon serve trajectory, certainly unlike anyone's experience of these sites, now or then.

Barthes argued in his "L'effet de réel" (1968) that the use of very concrete details can well remain a mere rhetoric device, driven not by the need for accuracy, but by the art of persuasion. Audiences are actually used as arenas to build disciplinary prestige, with less regard for knowledge formation. C. Mesick further argued that models ought to self-sabotage their in-your-face realism, suggesting they could signal to the viewer that they are just conjectures, by means of "angled contours of the landscape", "deliberately [...] garish colors", or "obviously «fake» textures onto roofs" (2013, 81). The intentional introduction of conspicuously non-realistic elements in

archaeological reconstruction is an interesting avenue of research. The seductive realism of images of the past may also give ideology better tools for manipulation (Smiles 2013); data is often scant and/or ambiguous and deductive reasoning can only take the archaeologist so far.

As Yegül put it, to rebuild all ancient buildings to their last roof tile, would be very disappointing, if not totally irresponsible (and financially impossible). But while the question whether to restore and/or reconstruct even individual monuments is very difficult, no harm is done in creating an academic model of it. In an ideal world, it would be mandatory for any archaeological publication to include a visual reconstruction of the site's architecture, indeed alternative reconstructions of the same monument. Not in order to wow the public, let alone to encourage handsome reconstructions piggybacked on poor data (cf. Favro 2013, 164), but simply because reconstructions (visual, as well as narrative) are the crucial test for the archaeologist's understanding of the site and therefore a direct fosterer of knowledge. The costs in money and time are probably the cause why alternative reconstructions are not offered. At best, it is suggested (Kantner, 2000, 52) that all reconstructions be accompanied by written text and description of the original archaeological material. The very definition of academic reconstructions presupposes, apart from the authors being qualified experts, the full disclosure of the metadata on which the work is based. Alternative reconstructions are, to be fair, recommended in the 2007 ICOMOS "Ename" charter for the Interpretation and Presentation of Cultural Heritage Sites (par. 2.4; language such as "the most probable reconstruction" was significantly left out from a previous draft, and the idea was altogether absent from the 1964 Venice charter). The authors of such a model would be perhaps best advised to present two reconstructions, at the opposite ends of what they consider the range of the possible.

Another counterproductive trend is correlating increased realism with decreased human presence. Realistic appears to have to mean dehumanized, cold, scientific, and numb: building models, streets in digital cities are generally shown with no people, little if any vegetation, no graffiti, construction materials with no signs of age and wear (Kantner 2000, Favro 2010, 32, n.5). While this is likely due to the additional computational difficulties rather than being a statement about society, it is bound to permeate public opinion and, in time, give new generations a quite eerie impression of how ancient places must have looked like. Experiential depth of these models, in sum, remains very shallow (Favro 2013, 168, Gillings 2005). The object's aura, lost, in Benjamin's view,

during (mass) reproduction, seems to have a correspondent in the loss of experiential depth from original to digital model. An additional problem is that models make a point to be rich in details, clear and easy to view – that is, after all, what makes them valuable, according to the common creed. But what if the original monument was dark, elusive, intentionally confusing, labyrinthine, awe-inspiring? What if the visitor was supposed to feel lost, or dwarfed (as already understood by Kantner 2000, 50). Instead, the modern visitor of virtual ancient environments is in control, a domineering consumer of science as entertainment.

In conclusion of this section, if he could watch this 3D digital model of Ancient Rome, produced by international experts, previously on Google Earth, now here <http://vimeo.com/32038695> (last accessed Aug. 26, 2015), Flavio Biondo, the Renaissance father of archaeological topography, would be amazed; Piranesi, probably, disappointed. The vagaries of assessing the success of a model point towards disciplinary ruptures between archaeology and history, and their societal insertion, but also to the opportunities for reconciliation in the future. Analyzing the production of models in the framework of mode 2 knowledge production has the advantage to emphasize the potential of non-hierarchical interpretative decisions, of reflexivity, and of the conception of scientific truth as also having a strong social dimension.

6. Methodological suggestions

A number of methodological suggestions can be put forward. The departure point should be the typical mistake of seeing archaeology's potential as a way of proving or refuting textual evidence (Hall 2013:210). Archaeology must be juxtaposed to texts as a fully viable alternative, structured along independent force lines, and only partly translatable into the textual paradigm. A key idea is the use of contradictions (disjunctions, etc., v. supra) between archaeological and historical data to design research questions and delineate areas of promising further investigation. It is perhaps not intrinsically justified to state that a discrepancy between what literary sources and excavations teach us necessarily warrants a priority investigation into that particular aspect. But for the construction of research question and topics, the search for harmonizing inconsistent data is a valid and pragmatic approach. The identification by zooarchaeologists of some *sus* bones in Islamic settlement areas (residual?) certainly deserves

further study, even though the study of eating habits per se does not contribute more to our understanding of a past society than the study of funerary ritual, built space etc. It becomes apparent that such topics are more important because, apart from giving insight into past societies just like the study of any other topic, they have the additional benefit of fine-tuning our archaeological and historical methods. This is an aspect that has not received the attention it deserves. It concerns the fact that certain research topics can also serve to “sharpen the tools” by which we do research, by leading to the identification of factual errors and reasoning fallacies, and by promoting re-assessment of the theoretical platform from which we conduct such operations. Ultimately, I propose that the great research potential offered by a contradiction between historical and archaeological data resides not in the possibility of some spectacular discovery about a past society, but in the opportunity it offers to investigate how the hermeneutic spiral unfolds between the study of material culture and the study of literary sources. In other words, when it is proposed (e.g. by Carmack and Weeks 1981) to see such a contradiction (called a “disjunction” or an “ambiguity”) as one of the most meaningful issues archaeologists can focus on when exploring past societies, it should be emphasized that it is in fact key to exploring archaeology itself, especially in relationship with history. Finally, the problem itself of the “contradictions” between archaeological and literary data is insufficiently theorized. Inherent in the idea of a contradiction is the belief of commensurability, the conviction that we are able to establish instances where the archaeological and historical discourses have the same referent. But since assertions based on either archaeological or literary evidence are always qualified, and in the absence of middle-range theories, can we ever point to an actual antagonism between these data categories? How exactly are contradictions constructed in an interdisciplinary dialogue? Researchers must be reasonably sure they brought the data to a common denominator. For this very reason, we ignore the fact that most discrepancies between archaeological data and historical sources are before our eyes and we ignore them because we explained them away in our quest for presenting a consistent interpretation. Once ensconced into persuasive narratives, most of these contradictions will never be revealed again for what they are.

Archaeologists must also advocate the obligation to entertain multiple interpretations at the same time when merging archaeological and historical data and when creating visual models. Clearly this is not a

comfortable thing to do. Juggling with alternative interpretations requires not only scholarly effort, but a kind of flexibility that many years of specialization tend to stifle. Even researchers who start off by considering several hypotheses end up soon committing to one of them, and namely even before a critical mass of evidence, pointing in this or that direction, had been reached.

Another suggestion is to reconsider the knowledge production “mode”. Mode 2 knowledge production (Gibbons et al. 1994, Nowotny et al. 2001, 2003) brings with it great advantages, such as the specificity of the problem and the diversity of approaches brought forward to tackle it. There are also a few hitches with mode 2, which may be outlined. Mode 2 is a short-time intervention which may be unable to establish branch cohesion and common epistemological routines. From the point of view of the sociology of science it is impossible to conceive of “outbedded” science, science without social continuity. Mode 2 has an intense, but short-term social correlative, and best practices are slow to emerge. The solution to this may be to have a network of mode 2 think tanks, working on specific questions, with scholar mobility and inter-core communication. In the absence of such a network and such a rhythm, best practices will not be established. Institutionalized interdisciplinary dialogue takes time to learn. Another concern is that research funds are driven by applicability, and while mode 2 caters to applicability, it still struggles with wider generality. In other words, we aim to produce socially-viable knowledge, but the warning is that this, while indeed socially “viable”, ought not to be “short-lived” knowledge. Again, imagining networks of diverse interdisciplinary think tanks may ensure that the results from one are fed back in the cycle. This will make not only for social permanence but will give true meaning to the idea of consensus formation and negotiation of results.

In turn, archaeological outreach has changed dramatically over the past few decades. Outreach often constitutes now a separate branch of major archaeological projects, governed by ever more specific and independent laws concerning information processing, packaging, and presentation. The way this leads to the production of new archaeological knowledge is understudied. An explanation for this may be the fact that many archaeologists are still too concerned of asserting their scholarly status to allow themselves to become unequivocally involved with “simplifying” outreach activities. But outreach can be seen, rather than as a downgrade, as a fertile arena for producing archaeological interpretations. Firstly, because of the results, secondly because of the methods deployed.

Outreach has translation as the avowed central part of its methodology, the translation of excavation and literary data into a product with the largest impact on the general public. The experience of outreach experts is paramount for advances in this field. Outreach specialists are much aware that they are translating knowledge from a paradigm to another, whereas archaeologists use historians' data (and the other way around) as if this was a self-understood process requiring no conversion.

Additional work is needed to map out the periods and type of sites for which merging archaeological and literary information works best. It would seem only logical that archaeological evidence is most needed where literary sources offer only meager coverage of a specific historical period. There is however an embedded historiocentrism in this suggestion. In fact archaeological evidence is needed as badly, if for different purposes, there where texts are abundant and (apparently) trustworthy. Indeed, it can be counterintuitively suggested that we need archaeology especially where texts offer a mass of information that is bulky enough for us to winnow out inconsistencies, judged by internal (not archaeological) *Quellenforschung* criteria, and thereby also create the possibility to compare them with the archaeological narrative. Clearly, however, there is a point after which the benefits of archaeology in addition to texts become minimal, such as in the case of post-medieval archaeology. On the other hand, where prehistory and history merge, and texts are scant and political, the idea of merging the two categories is almost meaningless. Importantly, an exclusive focus on periods that are both well excavated and well documented by historical sources (such as Roman Italy, or the Greek Aegean) risks not only to perpetuate a knowledge paradigm that is both Eurocentric and conservative, but also to obliterate the process through which blending the two categories of data is done. It is likely where texts are more lacunose and archaeology has not already tackled most key sites that the way this works can be observed more successfully. From this point of view, epistemological advances are therefore to be expected rather from the archaeology of fringe areas, interfaces, provinces, and melting pots.

Related to this, it is to be desired that archaeological and historical information be clustered around fundamental issues of general import. Specific questions, such as whether Alexander the Great was buried in Babylon or if the *depas amphikypellon* pottery type was used by the heroes of the Trojan war, risk to result in circular answers. Merging archaeological and literary evidence functions best when the historian strives for regional syntheses and wide-ranging evaluations. That way patterns emerge from

both the historical and archaeological sources, and they can be overlapped or confronted. The drawback here is that syntheses appear to be very unappealing to academic authors, although they are supposed to be the ultimate goal – in archaeology even more so than in history. Archaeologists are often hard to convince to publish their excavations of a villa in the countryside, let alone produce an analysis of a whole province.

An important avenue of research, anticipated by Little (1992), is the use of one category of evidence (e.g. textual) to derive hypotheses to be tested within the other category (e.g. archaeological). Subjects such as the trade between the Roman Empire and India, or of cultural influences between Greeks and the Phrygians, lend themselves well to such an operation. However designing hypotheses to be tested is not a straightforward process. It is perhaps not the falsifiability of hypotheses that matters, but that they be carefully selected to be refutation-effective or confirmation-effective, according to the scenario at hand.

Finally, it is important that both archaeologists and historians understand that the others' conclusions are just as provisory as theirs. It is understandable that a historian would want a clean-cut judgment from the conclusion of an archaeology book on her topic. To be able to put it to use, all "probably", "possibly", "partly", "apparently" will as a rule be removed from the conclusion. In a recent book J. Hall (2013:209) spoke in this respect of "unidimensionality". While there is a continuum of knowledge between archaeology and history, the danger remains that the choice between conflicting interpretations is made by an archaeologist based on a historical interpretation, itself perhaps privileged by the historian because of an archaeological tentative conclusion. Interdisciplinarity does not mean picking up a book from within another discipline and quoting its conclusions. It is rather a fine understanding of the workings of the other discipline, of its accomplishments and doubts, and of its potential to reach back and borrow in turn from one's discipline. How archaeology and history can work together on a safe epistemological basis is what is at stake now and this paper tried to offer a few avenues of investigation. I am certainly not advocating herein that archaeology should constantly strive to prove literary sources wrong, and thus assert its epistemological independence. And in this sense I will conclude with a cautionary tale. It is a scenario that later A. Snodgrass has popularized in the Anglo-Saxon world, and which in turn is attributed by Ducrey to Christiane Dunant. It gives archaeological "proof" that Switzerland was actually not neutral during the second world war: "[u]n archéologue de l'an cinq mille après

J.-C., fouillant la ville de Genève, tombe sur les ruines du Grand-Théâtre de Genève, détruit par un incendie le 1er mai 1951. Fouillant un autre secteur situé à 250 mètres de là à vol d'oiseau, il tombe sur une seconde couche d'incendie, non moins importante: il s'agit des vestiges du Bâtiment Electoral, anéanti par un autre incendie, le 4 août 1964. Une conclusion possible serait la suivante: contrairement à ce qu'apprennent les sources littéraires, la ville de Genève a été bombardée au cours de la guerre mondiale de 1939 à 1945 et a été partiellement incendiée. Un archéologue plus audacieux encore pourrait estimer, sur la base des mêmes fouilles, que Genève, et qui sait la Suisse tout entière, a été directement touché par le conflit" (Ducrey (1977, 13).

7. Conclusion

I am interested in the transferability of knowledge from one theoretical framework to another. I feel this boils down to a multi-step negotiation of this knowledge between archaeological, written sources and visual reconstructions. I attempted to make some progress in deconstructing the two-way transfer of knowledge between history and archaeology, and in generating a model of how this is done now and how it should be done, in other words in producing the epistemic map of this transfer. The process of translating archaeological results in historical narratives and visual illustrations helps to filter out inconsistencies, fosters reflexivity and multivocality. Above all, this process, rather than creating mere copies or equivalents, actually generates archaeological knowledge. Moreover, the creation of historical narratives and visual models of sites and monuments does much more than communicate results. It is a heuristic device to test the archaeologist's understanding of a site and help them find out more about it. This is a proof that true interdisciplinarity helps to also reinforce disciplinary excellence and integrity, rather than posing the threat of relaxing disciplinary standards. It also stresses that historical reconstruction, including its visual avatars, can benefit enormously in the future from truly accepting what archaeology has to offer in terms of identifying and explaining the processes of interaction, acculturation as well as the creation and meaning of images.

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