A Comment on Hybrid Fields and Academic Gate-Keeping

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The workshop on Poros which forms the basis of this collection of papers was entitled ‘Archaeological ethnographies: charting a field, devising methodologies’. Both the workshop and the present volume that resulted from it constitute attempts to establish a new field, with its own methodologies and its own contested practices, at the interface of several existing disciplines and fields of research. My comment takes the workshop on Poros and its results as a starting point but intends to raise some relevant wider issues concerning the dynamics of academic practice.

Archaeological ethnographies investigate local perceptions and ideas about archaeological sites and are located somewhere in the borderlands between archaeology and social/cultural anthropology. Although part of the ‘ethnographic turn’ in archaeology (Castañeda, 2008 and this volume), they were said to be distinct from now established research areas such as, among others, community archaeology, public archaeology, and the ethnographies of archaeological practice and heritage. In their introduction to this volume, Yannis Hamilakis and Aris Anagnostopoulos claim that the ethnographic turn, of which their own archaeological ethnographies form a part, should not be viewed ‘as merely the emergence of a range of new practices that can be appended to a conventional archaeological ontology and epistemology’. Instead, they argue that its true task is ‘to dislodge the certainties of archaeology, its belief in its authority, its naturalization by its practitioners as the sole and exclusive agent for the production of discourses and practices about ancient things’ and thus to produce ‘possibilities for new practices’.

It did, however, become apparent during the workshop that no matter how unconventional and innovative everybody wished to be, most of those present also felt very strong ties to their home disciplines of social/cultural anthropology or archaeology and their respective repertoires of methods and approaches. The research discussed was often understood as being either anthropological or archaeological, depending on the underlying questions and the adopted methods. This perceived need to place scholarly work into predefined disciplines corresponds to Ioanna Antoniadou’s (this volume) experience that the ethnographic fieldwork she conducted as an archaeologist on the contemporary looting of antiquities was widely deemed to be ‘non-archaeological’ and ‘inappropriate’ for an archaeologist to engage in. Whereas archaeologists are expected to study the past and what remains from it, social/cultural anthropologists correspondingly study people in the present. However, as

this volume illustrates profusely, the two fields interlink when archaeological research is placed into its present context or draws inspiration from contemporary practices and when anthropological research engages with heritage or history. Indeed, one and the same project can employ specialists in both fields, together engaging in archaeological ethnography as a 'collective practice' (Hamilakis and Anagnostopoulos, this volume). But you cannot, it seems, be a competent specialist in both social/cultural anthropology and archaeology at the same time, nor indeed an academic specialist in a third field such as the socio-politics of the past that overlaps with both of them. As Eleftheria Deltsou argues (this volume), one of the questions in the room during the workshop was whether archaeological ethnographies should be seen primarily as an addition to archaeological or anthropological interests, while in reality it is precisely the blurring of these boundaries that has created a new and rich field of investigation and academic practice. I suggest that the reason for these conventional intellectual allegiances is an all too narrow and restrictive definition of academic practice, hidden behind a seemingly uncontroversial notion of academic competence.

Academics are generally assumed to be knowledgeable in one distinct discipline that is defined by a set of essential questions, methods and approaches. An archaeologist, for example, asks about material culture and chronologies, has got to know digging and dating methods and needs to be able to position him- or herself somewhere on the spectrum of cultural historical, Marxist, processual and postprocessual approaches. Each discipline has an academic canon that all of its students must acquire. This canon is maintained not only by partly standardized curricula (although they may vary between countries) but also by academic gate-keepers that function as peer-reviewers, for example for evaluating candidates for scholarships, manuscripts submitted to journals, or grant applications to major funding bodies. Although such reviewers can also offer useful commentary and constructive criticism, the reason why their existence is institutionalized (unlike that of other colleagues who may offer commentary and advice) is to maintain an assumed body of academic standards in a given discipline. The peers consulted often expect that a topic be investigated in a certain way and with reference to a certain body of widely known past work. They may also presume that certain issues are investigated by archaeologists and others by anthropologists, and that there are theoretical and methodological cores and margins for each academic discipline. For example, a few years ago I applied for a job at the University of Oslo. The external evaluators who assessed all candidates dismissed much of my own research as dealing with 'marginal' issues. My research did quite simply not correspond to their expectations of what is central to the discipline of archaeology. By the same token, Håkon Glørstad has shown that for Norwegian archaeologists to succeed they are expected to have published work on chronological issues (Glørstad 2006). Whether as a deliberate strategy or as the result of their academic education, some academics will adapt their own research topics to canonical requirements. Such behaviour is rational and understandable but it can also prevent academic innovation.

Academics have internalized disciplinary canons and the need for gate-keeping to an extent that they surprisingly often agree on standard methodological and theoretical criteria, even though they may occasionally differ as to whether or not a specific paper or proposal may fulfil sufficiently many of them. This practice can be

justified in terms of an academic division of labour, as Deltsou suggests and Castañeda implies (both this volume). There is a widespread concern that archaeologists should not do 'second-rate anthropology', and vice versa. This became evident recently during the 2009 Conference of the Association of Social Anthropologists held in Bristol and devoted to 'anthropological and archaeological imaginations: past, present and future'. Although members of both disciplines were genuinely interested in each other's work, had respect for everybody's achievements and appreciated opinions on topics of mutual interest, there was no sense of the emergence of a substantial joint (post-disciplinary) discourse where disciplinary affiliation no longer mattered. Indeed, when it transpired during questions that assumptions made about contrasting methods and approaches in the two disciplines were mistaken or outdated, this caused more embarrassed consternation than a sincere willingness to overcome the nearly impenetrable disciplinary boundaries that were the cause. I am not inclined to agree with those who assume or imply that academics are technicians trained in applying a set of distinct methods and techniques to certain canonical questions. The terms archaeology and anthropology are not protected trademarks and neither one's academic practice is tightly regulated by law, nor should they be. I think instead that exclusive and narrowly defined academic fields of competence serve nothing but the perpetuation of the academic disciplines themselves — and indeed of academic discipline in the singular, constraining renewal and suffocating intellectual creativity.

A recent critique of my work can illustrate further what I mean. Kristian Kristiansen (2008: 489) argued that 'Holtorf has been ideologically consumed by the popular culture he set out to analyse, perhaps because he is an amateur in the field, and therefore lacks the critical and methodological distance that another sociologist would have possessed'. Needless to say I dispute both his analysis and his judgement. But what matters in the present context is that Kristiansen, in order to object to my conclusions which he disagreed with, chose to question my academic credentials and expertise. As I see it now, also intriguing was my own reaction, considering it best to insist that 'I adopted an anthropological approach and an ethnographical methodology in the research underlying my books ... [and that] I was well qualified for such an analysis given that Ethnologie (the German equivalent to social anthropology) was one of two subsidiary subjects in my initial *Magister* exam based on five years of study' (Holtorf 2008: 491). I wrote this even though I do not really believe in the division of the world into disciplinary territories whose sovereignty is policed by disciplinary gate-keepers like Kristiansen. I deemed that any other argument would have compromised my own academic integrity, as it might have been widely seen as a tacit acceptance of Kristiansen's critique and thus of his accusation of my own incompetence. In reality, I do not think at all that it is a sign of incompetence when a researcher tackles a trans-disciplinary topic in a creative way. Indeed, I agree entirely with what Kristiansen has written elsewhere (2005), namely that established academic boundaries can legitimately be challenged in critical discussion and that innovative research environments are precisely characterized by novel approaches that challenge established disciplinary boundaries, theories and practices.

A helpful way of engaging with all these issues in more depth is a look at different models for the dynamics of the sciences and humanities, inspired by papers by Michel Callon (1995), Thomas Gerholm (1990) and Paul Peter and Jerry Olson (1983).

TABLE 1
FOUR MODELS FOR THE DYNAMICS OF THE SCIENCES AND HUMANITIES (INSPIRED BY CALLON 1995,
GERHOLM 1990, AND PETER & OLSON 1983)

Model	Nature of advancement	Academic competence	Dynamics	Hybrid or new practices
1: Rational knowledge	Knowledge growth	Ways of interrogating, studying and evaluating	Open disagreement among peers	A threat to a functional system?
2: Socio-cultural practices	Continuity of academic culture	Tacit knowledge	Ordinary social practice	A disruption of normal practice?
3: Marketing	Popularity of academic publications and theories	Recognizing existing demand	Researchers seeking status	A winning strategy?
4: Extended translation	Development of actant networks	Power to make allies	Competing networks	An opportunity to strengthen a network?

Although the discussion that follows is brief and somewhat provisional, I hope that it can throw additional light on the project of establishing ‘archaeological ethnographies’ as a new academic field. The point is to demonstrate that there are several models that can account equally well for the dynamics of academic disciplines (Table 1). The four models I will be distinguishing are ways of understanding academic practice and are neither reflections of the academics’ own conscious goals and intentions nor normative instructions for how to work academically. Yet they do place existing academic rules and norms into wider social contexts, effectively relativizing the unquestioned validity of any one perspective. It will become clear that there is no necessity to embrace an academic gate-keeping perspective that enforces standard ways of interrogating, studying and evaluating aspects of the world in order to guarantee knowledge growth and academic progress of a particular kind. Academic competence can mean very different things. I will discuss several alternative ways to the traditional one implied throughout this volume. Each of them allows charting a new field like ‘archaeological ethnographies’ in a different way and gives researchers different roles.

Model 1 is the traditional model emphasizing rational knowledge as the main product and the most significant parameter of all academic disciplines. According to this model, rational knowledge grows over time as the sciences and humanities proceed, mainly thanks to distinct disciplinary ways of interrogating, studying and evaluating chosen parts of the world. Open disagreement among academic peers leads to continuously improved theories and models. As I read them, practically all papers in this volume subscribe to this model, offering contributions to the growth of academic knowledge at the interface of archaeology and anthropology. This is the case even though the knowledge proposed originates from very different perspectives, including political, historical, feminist, and visual ones. The various authors agree that archaeological ethnographies in one way or another have important contributions to make to academic knowledge. The latent unease that occasionally came to the fore during the workshop, concerning the legitimacy of hybrid fields and the significance of disciplinary homes, reflected a certain anxiety not to endanger an existing system of academia that is perceived as essentially functioning well — whatever minor adjustments or new fields of study may be proposed. According to this model, there certainly is a role for academic gate-keepers. Although in many cases they will be open-minded and helpful, they may also pass harsher judgements.

Model 2 considers academic disciplines as cultures (or sub-cultures), with their distinct norms, beliefs, and traditions. How to behave as a researcher is governed by tacit knowledge transmitted informally from one generation of academics to the next. Within each culture academic practice proceeds much like ordinary social practice as it appears in other cultures and sub-cultures in society. None of the present authors subscribes explicitly to this model. But with the possible exception of the innovative photo-essay by Hamilakis, Anagnostopoulos and Ifantidis, all papers reflect this model as they follow to a large degree the normal expectations of what academic papers are supposed to contain and how they are supposed to look. Their authors can legitimately expect to harvest academic rewards in the form of recognition in future discussions and esteem from publishing in a peer-reviewed special issue of an international journal. To the extent that archaeological ethnographies disrupt established academic norms and traditions they threaten the affected culture's future and may bring external gate-keepers onto the scene that seek to preserve as much as possible. As I indicated, I do not think that the present papers, with one possible exception, disrupt academic cultures in this way. They can, rather, be interpreted as a continuation of already established practices in the relevant academic cultures and from this perspective may be less innovative than perhaps intended.

Model 3 is again a model that none of the papers explicitly subscribes to although it might describe very well why their authors wrote them and why this journal published them. Here, academia is about the marketing of ideas. According to this model, researchers are motivated by a desire to increase their academic status. To that end they are seeking to publish works and theories that correspond to a perceived existing demand among both their peers and their students. To come up with a new idea, or — as in the present case — further develop an existing field at the interface of two academic disciplines and associated with a catchy label ('archaeological ethnographies'), might be just the right kind of product at the right time in order to succeed on a competitive market. Predicting which theories are in demand is never easy, but occasionally it is a good idea to jump on an already winning bandwagon — which is what the 'ethnographic turn' of archaeology could be just now. According to this model, academic entrepreneurs are needed who can read the signs of the times, and not academic gate-keepers who act too conservatively by working for a demand that no longer exists.

Finally, model 4 interprets academia as part of a larger process of extended translation. In this view, academic disciplines contribute to competing actant networks. What is required first and foremost of an individual researcher is the ability to add new allies to a given network. The concept of archaeological ethnographies might be conceived as being a new actant and potential ally which those present at the workshop had already recognized as a valuable addition to their existing networks. Once again, none of the papers make explicit reference to such networks, but their content (including the affiliations of their authors and all their citations and references) shows that they use the new concept precisely for an extension of existing networks that translate academic competence, cited authors, theoretical concepts, empirical examples, funded projects, etc. into powerful machineries. Here, academic gate-keepers are hostile agents who seek to support some actant networks while undermining others to which they may not be tightly connected

themselves. I understand now that this is what Kristiansen practised in his critique of my work. Another example are peers insisting, as they often do, that the author must absolutely discuss a particular other work known to the reviewer, in order for the paper under review to be acceptable: a work is only considered as being good enough when it forms part of the actant network to which the reviewer belongs.¹

I firmly believe that academic discussions, including the one contained in this volume, should more often argue from other academic perspectives than those emphasizing the advancement of rational knowledge as represented in my first model. This would make it easier to challenge academic practices and preconceived assumptions that are inherently conservative and hinder academic innovation while at the same time provide an alternative perspectives on academic progress. Rethinking academic competence along the lines suggested by models 2–4 is a useful exercise for coping better with any challenge to a researcher's expertise explicitly based on the first model. It teaches us that there are different ways to understand academic practice than the dominant traditional one, and that there is not necessarily a need for scholars to worry too much about reshuffled disciplinary boundaries and emerging hybrid fields.

This volume demonstrates elegantly that archaeological ethnographies are well placed to investigate further the relations between archaeologists and their disciplinary practices, knowledge and arguments on the one hand, and the full social and public contexts of archaeology on the other hand. As one likely outcome we can indeed expect that some of the certainties of archaeology will be dislodged as new academic gates will be opened. Let us step through them together!

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Notes

¹ All I said can of course be applied to this very comment, too. It can be read as my own contribution to improving on the rational academic ways of knowing the world. Maybe in some way I am also trying to defend an academic culture I belong to against perceived threats. Certainly, I am marketing ideas hoping for demand among

academic customers. Arguably, I am also seeking to extend an existing actant network describing the character and role of academic archaeology, to which some of my work and ideas already belong (e.g. Holtorf 2005), so that the achievements of the present volume are somehow connected to it.

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