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Mapping Hellenistic and Roman Egypt: Comment

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This session on mapping Hellenistic and Roman Egypt grew out of a suggestion by Heinz Heinen, who pointed out to me – after seeing some of the first proofs of maps for the *Atlas of the Greek and Roman World* – that the work on the *Atlas* offered an opportune occasion for taking a look at the problems and opportunities of the cartography of Egypt in the papyrological millennium. As a result of this group of papers, I am torn between consternation at the thought that the work of our map compilers is becoming out-of-date before it appears and cheerfulness at the thought that eventually an electronic version of the *Atlas* will be constantly enriched by new discoveries. For despite all of the good reasons to be concerned about the accelerating threats that development in Egypt poses to the survival of the archaeological record, it is hard to escape the impression that our means of increasing cartographic knowledge are growing at a still faster rate.

Mapping has various purposes, which are by no means incompatible with one another. During the past century the most widely pursued aim has no doubt been the identification of ancient toponyms surviving in the texts with actual archaeological sites. Modern historical currents have widened that interest to include settlement patterns and the mapping of infrastructure, particularly of course in the case of Egypt the human intervention in the distribution and use of water. All of these aims of mapping show clear gains from the approaches represented in this session. I will single out a few points for comment, both looking for the central methodological points of the papers and singling out some of the problems and limitations that have emerged.

Perhaps the single most important common theme is the use of various sorts of non-invasive archaeology. At the very remotest level we have the use of satellite imagery, particularly using non-visible parts of the spectrum, to discern from a distance patterns not able to be seen at close range. But this is just one technology in support of the larger enterprise of survey archaeology, which has for quite some time been a fashionable, if controversial, area of contemporary archaeological practice. It becomes clear from several of our papers just how incomplete our knowledge has been even in areas extremely familiar and long explored. Who would have guessed that our grip on the Fayum, of all places, was so feeble? And yet we have seen two striking demonstrations of how much more we could learn, from the intensive regional survey Dominic Rathbone has been carrying out and Paola Davoli's reopening, with dramatic and convincing results, of the question of the existence of ancient sites consistent with a Graeco-Roman level of Lake Moeris not greatly different from that today. In the valley, the survey of which Renate Müller-Wollermann was a part has shown that areas which we know well from the papyri were until recently hardly studied at all on the ground. The eastern desert has, as we know, been an area of greatly expanding archaeological activity in the last two decades; from Steven Sidebotham's report we see how many sites not discovered or accurately reported by earlier travelers can be identified and placed precisely thanks to the satellite-based positioning systems that have gone from novelties to everyday gadgets within the past decade.

The other major emergent point seems to me to be that we are far from finishing the philologically-based part of our work, a side of things unique in its richness in Egypt. Rathbone's project has shown that going back to the papyri with new questions drawn from survey work can produce a far richer result than either methodology can offer in isolation. Other recent scholarship has shown that much can be learned by constructing schematic maps of the ancient toponyms known from texts, using the information about administrative districts and connections of one place with another. In this category may be mentioned work on the Oxyrhynchite by Julian Krüger (*Oxyrhynchos in der Kaiserzeit*, 1990) and more recently Jane Rowlandson (*Landowners and Tenants in Roman Egypt*, 1996), and that on the Hermopolite by Jennifer Sheridan in *Columbia Papyri IX* (1998). Falivene's book on the Herakleopolite (1998) also makes a substantial contribution in bringing together different types of evidence in a detailed study of a single region.

A somewhat different, but also extremely interesting, perspective is opened up by Müller-Wollermann's discussion of the use of modern place-names for the identification of ancient sites. I shall return in a moment to some of the difficulties involved in this enterprise, but at the least it is clear that a systematic sifting of the numerous toponyms of modern Egypt is worthwhile and still to be undertaken for many regions.

The underlying assumption of all of these papers, even if not said in so many words, is that the use of methods from multiple disciplines to approach a single problem is in fact successful. Of course interdisciplinary research is often praised, but it is less often practiced.

Now, I want to look at what seem to me some of the limitations of the methods we have been discussing. The single most significant, I believe, is the poor chronological specificity, or resolution, of survey archaeology. This point has often been made in debates about the value of such surveys, but it is worth bearing in mind. The collection of surface pottery may at worst give an erroneous sense that a site was not occupied during periods not represented in the surface finds. At best, even if all periods are represented, one tends to wind up with chronological indications like "Roman", which is all well and good but not really very informative. Obviously it is better to know that we have sites of the Roman period in particular places than not to know anything at all, but we should keep in mind that great care must be used particularly in reaching any negative conclusions.

A different problem, but also of resolution, is the relative coarseness of the radar technology in remote sensing, with a 30 meter resolution. Compared to what is now possible in visible spectrum satellite imaging, that is very rough indeed. As with survey archaeology, it can tell us that something is there, but many times little or nothing about what that something is. Again, we are grateful for all new information, but we should not mistake it for being more specific than it is.

On the philological side, Müller-Wollermann has herself pointed to some of the remaining difficulties, particularly in uncertainties about how Egyptian sounds were translated into Arabic representation, something no doubt to some degree dependent on vocalizations in local dialects of Egyptian – in turn itself a subject about which we know little or nothing in many regions. We may hope that further discoveries of Coptic papyri from areas previously not well represented will enable us to get a better sense of the vowel sounds in use in particular areas, which may have diverged from official spellings. It is also well to keep in mind the existence of homonyms, both ancient and modern, and above all the fact that place-names can migrate as villages move. In such cases, the distance moved may not be enormous, and we may still have at least an approximate sense of where the ancient village was located, but again the precision of our knowledge leaves something to be desired.

Finally, it is worth redressing what may seem to be a bit of an imbalance in all of this by pointing out that more intensive investigation of individual sites still has an important role to play. This point emerged from Rathbone's paper, as he showed how the detailed surveying, rather than mere recording, of the sites in his area has helped to bring out a clearer sense of the settlement history of the Polemon district. It is also strongly borne out in Davoli's evocation of Qaret el-Rusas, where despite the damage done by immersion of the site there would still be much to be learned by excavation. And we have only to think of the difference between the Eastern Desert sites that have been identified and drawn and those that have been excavated, yielding texts with their peerless ability to give precision and names to the remains. Despite the difficulties involved in excavation, I think that carrying out a large number of very small-scale exploratory excavations on sites identified through survey ought to get serious consideration. As we are constantly reminded, agricultural and residential development are threatening many of the sites of the Hellenistic and Roman periods, and only a few will ever be able to be dug in extenso. Quite a few more, however, might get enough investigation for chronology and character to be made much clearer than survey alone will ever permit. If it is a measure of a successful group of papers that they send us away with more work to do, our contributors have been a remarkable success and deserve our warmest thanks.