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External Review

Application of modern methods in landscape archaeology

Case study Ager Castronovano

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The dissertation here reviewed aims at promoting the use of novel technologies in landscape archaeology. The case study is represented by the *ager Castronovano*, north of Rome, in the municipality of Santa Marinella. As introduced by the candidate, the study relies heavily on previous archaeological works, both in terms of field survey and excavations of some of the sites within the region. New datasets were also produced during the dissertation.

A general overview of the structure of the dissertation from the table of contents seems to reveal a well-reasoned organization of the thesis; however, as one approaches the reading of the specific contents, several concerns and major issues arises.

There is a general confusion in the layout of the dissertation, especially for what concerns images and pictures. First of all, a reader has to wait until page 27 to see the first map showing the investigated area of the research; although some people and scholars can be familiar with the region, a dissertation focusing on topography should present immediately the geographical area of interest. The same can be said on the always mentioned map produced by Gianfrotta (1972) that is maybe well described, but not presented in its original form (only a re-elaborated version is present at page 14, fig. 10). Then, some of the pictures are included with the text which is a wise choice, especially if one wants to keep the reader focused, while a large number of them is at the end of the dissertation and not always with a clear justification.

Overall, English is of high quality if one escludes normal typos and a few misspelled words.

The theoretical framework is quite good, although the introduction to the methodology (2.3, p.7) could have been more sophisticated. It definitely needs some reworking, providing more references, and different approaches to the topic of landscape archaeology and the concept itself of landscape [can this be analyzed only by citing one scholar (Gojda)?].

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Chapter 3 presents some major issues. First off, the title suggests an overview on the state of the research of the territory here analyzed, but the text focuses on a much wider, although extremely scarce in contents, perspective on central Italy. One would expect a narrowing focus towards the *ager Castronovano*, which happens at the very end of the chapter, claiming that some of the cited works only partially covered the area (p. 26).

Speaking of references, here comes one serious problem. Although the use of statements like “in recent years” (p. 25), almost the totality of the cited works belongs to 40-50 years ago; a serious inclusion of much more updated bibliography is here needed. Quite shocking was the missing reference to the Albegna (not Albenga, which is in Liguria region!) valley results, published by Carandini and Cambi in 2002. Now, the volume is surely controversial for some of the results, and one can agree or disagree with the authors, but it cannot be eliminated. The Albegna Valley Project was a quintessential survey program, inspired by the South Etruria Project and has dominated (again, whether we agree or not with the results) the debates on the formation of Roman landscapes in Central Italy/Etruria in the last 20 years. Canceling it from the debate is barely impossible.

Thirdly, a large research project in Central Etruria was recently carried out and published: the Roman Peasant Project, where scholars from an international pool of universities were using novel technologies and field data to reconstruct Roman landscapes. No mention of this, again, was given in this chapter.

All the works produced for the South Etruria Project are missing, apart from what Goodchild did. How is this possible? Recent publications have tried to re-evaluate the results along the Tiber Valley (which one may guess could have some similarities with the *ager Castronovano*), with Helga di Giuseppe reassessing decades of landscape archaeology in this region. However, the chapter lacks any references to these and many other works.

Finally, one may wonder what was the purpose of this chapter. It starts as a summary of recent research for Central Italy, but the title directs the attention to the territory of Santa Marinella: in both cases, the chapter fails to deliver its original intents and needs a serious review (if not a complete rewriting) of its contents.

Chapter 4 presents the development and history of the site of *Castrum Novum*. Finally, as said before, the reader gets to see a map showing the location of the investigated area. Nonetheless, we are presented with a code (AC071) without having any explanation of its meaning, which will come only many pages later.

The very first sentence reads “*Castrum Novum* was founded on the place (p. 27)” without specifying on the place of what.

The description of a colony (The foundation of the colony was a codified administrative procedure approved by the Roman Senate etc. etc., pp. 27-28) is quite simplistic and needs some references (a new volume on Roman colonies was published in 2019, edited by De Giorgi and focusing on colonies in central Italy).

Citing Rutilius Namatianus is always tempting when dealing with Central, coastal Italy (p. 29). Although the candidate tries to contain the damages of mentioning him, especially in terms of understanding the late Roman countryside, a couple of extra sentences would have helped to better locate his work within the decaying Roman rural networks.

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The report on the excavations and the different structures is quite complete, although no plans are provided. An archaeological dissertation, describing and explaining urban structures needs to show evidence; it is not enough to properly cite references as the candidate did.

Chapter 6 is the main catalog of the dissertation. It seems like only a portion of a much more “complex database” is here presented. At p. 45, finally, one discovers the meaning of the codes that were already present and in use in chapter 4.

The list of categories presents some issues. For instance, why is “settlement” under housing? Following the provided description “A settlement conventionally includes its constructed facilities such as the roads, enclosures, field systems, boundary banks and ditches, ponds, manor houses, moats and churches” one wonders if boundary banks that are part of a settlement could be housing places, or ponds or ditches, or churches. There is a long debate around categorizing sites, especially if the intent of the research is to reconstruct landscapes in the *long durée*; here the candidate shows some of the scholarship, but again avoids recent research (one for all, the publication of the results of the Roman Peasant Project where the problem of identifying sites in landscape archaeology is a crucial one).

Moreover, are farmsteads only known for the medieval period? The provided definition sounds like the most common definition of a Roman villa, rather than a medieval agricultural facility. It is also known that farms and farmsteads were present in Roman times and molded the landscape as much as *villae* and other infrastructures.

Then comes the definition(s) of *villae*. Here the issues are massive, especially as the brief descriptions of the different kinds of rural complexes do not do justice of decades and decades of debates around them. General “publications” (p. 46) are apparently responsible for the chaos, but the candidate doesn’t provide the sources (are these local publications? Are these major works on Roman villas? Or the publications mentioned at the beginning of the paragraph?); it reads also that some efforts were made (by whom?) to describe Republican villas as smaller (in comparison with?), modest and dissimilar (to?) but if one thinks, just as a pure example, about the construction of the Republican *villae* at Settefinestre or at le Colonne everything can be said but that they were small or modest. The issue is not even resolved at pp. 170-173 where the terminology is a little bit more elaborated, but far away from completeness that is expected from a Ph.D. dissertation. Besides, is it possible nowadays to speak about *villae* without mentioning a single work by Annalisa Marzano or Andrea Carandini? Or the Oxford Roman Economy Project results?

All these categories, whether appropriate or not, lack a fundamental factor: there is no way to weight the reliability of the interpretation in the database. If, and it won’t be excusable, this was inserted in the more complete database, it should have been shown and, actually, it is not. As archaeology may be extremely subjective, the interpretation of a site depends on a number of factors that must be listed (how is a scatter of materials recognized as a villa rather than a village?). Without any possibility of balancing the real data (scatter of materials, pottery, ruins, anomaly etc, etc) with the subjective interpretation provided by the researcher, any hypothesis on the settlement network can hardly be reliable.

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Dating can be also a problem. The candidate states that for known sites the chronology was taken from the original references. Was this taken for granted? Was the pottery/material culture assemblage reassessed especially for quite old publications?

As one reads the catalog, interesting data can be retrieved. For instance, AC052 (p. 85) is described to be an Imperial villa based on building stones (rubble?) and fragments of roof tiles. How is this identified as a Roman villa? How is this dated to the Imperial period? The same could be true for AC054, a Roman villa with a picture showing no traces of the old site (105A), while the text refers to kitchenware and plaster (not shown anywhere) and no bibliography. AC109 states that a farmstead has a core structure dated to the 13th century AD, but then it is cataloged as a postmedieval feature (so post 1453 or 1492 AD).

Chapter 7 tries to summarize the data retrieved and collected during the research; however, given the previous issues on the raw data it may be hard to fully evaluate the results.

At least for the Roman period, some annotations can be made. At p. 169 it states that “the first inhabitants (of two *castra*) were poor Romans” but this is based on which evidence?

Yet for the Roman period, how is it possible that there are no villages? And if so, is there a reason behind this choice? No sanctuaries whatsoever? Is the Roman countryside formed only by *villae* and *castra*? No isolated farms?

This passage is quite confusing “*The difference between scattered pre-Roman settlements, strict Roman urbanism and the urban landscape is evident. The topography pattern clearly shows that Etruscans and Romans are two different types of communities. The Etruscan settlement structure created a so-called “leopard-skin” pattern. The Roman community, on the other hand, respected the Latin Right building rules, which fixed the regular structure (Stek 2014, 90). One of Cato’s passages could also explain the intensive transformation of the landscape. It first obliged the owner to deal with the planting of crops and their productivity; only later, with the construction of the villa. The apparent reference to the transformative and conservative capacity of the structure and instrumentum of the villa was implicit in this concept of proportionality*” (p. 210). Even if one wants to agree with such a statement, where is the evidence to support it? The so-called “Romanization” process is here just mentioned without the insights due to a landscape analysis of this type of transformations (no references to the massive works of Nicola Terrenato, including his latest volume).

Then “*In the late Republic period and the early Imperial period, agriculture formed the backbone of the Roman economy*” (p. 210) What about the mid-Republican period? And the early Republican period? Was it different? If so, how? If there was no difference, why stating the late Republican period in comparison with the late Imperial period? Is the 10% drop in the agricultural workforce (within some 4-5 centuries of difference!) relevant to the discussion?

At p. 214 the analysis provided that “*the small peasants had 12,5 hectares at their disposal. They probably had another source of livelihoods, such as grazing or fishing. In Suburbium, wealthier landowners owned around 60 hectares (240 iugera)*”. I guess the natural question is who are these small peasants? How is the analysis run? Does the analysis take into account possible unrecognized sites due to the fallacies of the used categories?

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For the Imperial period, p. 214, it states that a crisis dominated the society in the 1st century AD (but started 3 centuries before). What is this crisis? How can be Domitian's wine edict of AD 92 earlier than the 1st century AD (being in the 1st century AD)? The entire paragraph on the Imperial (but also the Republican) period needs a severe reassessment with a much more recent bibliography on the agricultural economy.

Is Fig. 54E related to the Etruscan period (as stated in the caption) or to the Roman period (as stated in the key)?

At p. 215 it reads "*The Latifundia stretched over 100 hectares. These areas largely corresponded to the possibilities of ploughing. A pair of oxen managed to plough 25 hectares, two pairs of oxen managed 50 hectares. The most impoverished peasants who owned 12 hectares either had no animals or hired them. The model showed that the area was divided into plots unsymmetrically, and there were differences. In the lowlands near the colonies, there were located slightly smaller plots.*" Any reference to support these hypotheses?

Large confusion comes soon after. So, the first 2 centuries AD saw an agricultural crisis, according to the candidate. *Villae* survived well (p. 215), as "*Furthermore, starting from the first two centuries AD, the landscape of the villas was well structured and did not undergo significant changes*" but at the same time "*The original Republican settlement network remained the fundamental pillar, but there were several changes in the landscape*". So, there is an economic crisis (agriculture) but the main component of this economic system (*villae*) all survived well (how?) and with no significant changes from the Republican settlement network that, however, had several changes.

The section on the late Roman period is extremely vague and resents a number of historical biases. The territory here analyzed is immediately north of Rome, in a strategic position along with terrestrial and maritime routes and, apparently, no archaeological data is available from the 3rd to the 5th century AD. It reads like the presence of Christianity was at the base of the decline that started with the establishment of *dioceses*, although a much more profound analysis could have been provided, trying to place the territory in the wider context of the Mediterranean and the Italian peninsula (none of the big social/economic/political models for Late Antiquity are even mentioned to try to understand the changes of the later Roman Empire).

As for the Middle Ages, it is worth remembering that the Greco-Gothic War and the arrival of the Lombards happened both in the 6th century AD (not in the 5th!) - p. 220.

The overall impression on the dissertation is that it has some potential to present some new data, but large confusion on techniques of landscape archaeology and limited knowledge of recent debates on, at least, the Roman countryside in central Italy represent major issues.

It is unclear how the use of novel technologies (namely the composition of a DTM, analysis of historical maps and reassessment of previously collected data) brought new data on the general knowledge of this region. The establishment of a GIS platform (digital maps+database) and the acquisition of new possible sites through the analysis of aerial pictures and limited surveys should have been followed by a wider project of archaeological research on the territory that is, at this stage, not carried out. In order to better read and assess a multi-phased landscape, such as the one here studied, the candidate should have also promoted serious and planned field surveys to integrate and verify her hypotheses.

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For all these reasons, I **do not recommend** to defend the dissertation as it stands but to provide significant changes before resubmitting it.

Finally, the candidate's list of publications seems quite adequate to the stage of her career, showing different research interests; no previous works are, however, related to the main topic of the dissertation.

If you have any further questions, please feel free to email me at as424@buffalo.edu

Yours sincerely,

A handwritten signature in black ink, appearing to be 'AS', with a long horizontal stroke extending to the right.

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