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Jasna Jeličić Radonić and Miroslav Katić. *Faros – osnivanje grčkog grada – I, (Pharos - the foundation of the ancient city – I)*. pp. 233, c. 160 illustrations. 2015. Split: Književni krug Split and Odsjek za povijest umjetnosti Filozofskog fakulteta Sveučilišta u Splitu. ISBN 978-953-163-419-9 (Književni krug Split / Literary Circle at Split) and (Filozofski fakultet u Splitu / Faculty of Philosophy at Split) ISBN 978-953—7395-85-8 paperback 200,00 kunas (c. € 27)

This review would not have been necessary if this book did not have an extensive English summary, almost as long as the entire text in Croatian (the book's cover lacks the English title).¹

The aim of Jeličić-Radonić and Katić's book is to introduce us to ten years (1994–2004) of archaeological excavations conducted at Stari Grad on the Adriatic island of Hvar in Croatia, where the remains of Pharos (a Greek *polis* founded in the early 4th century BC) are located. Their book has fourteen unnumbered chapters as well as an Introduction, Conclusions, a Bibliography and an Index. I will only mention here some of the problems found in this book, whose publication has long been announced and awaited.

It should be pointed out that some chapters had already been published as separate articles, which is not unusual. What raises eyebrows is that the authors have not mentioned any recent studies that have appeared since those works were published. Such studies deal with various problems concerning the Greeks' emigration outside their homeland, their relations with the native populations, and the

presence of native pottery within Greek *apoikiai*.² They also include the recent numismatic analysis of Paros' coins,³ and criticism of the claim made by the authors that a purple workshop existed at Pharos.⁴ It is interesting that the authors do not even refer to the book by Marie-Christine Hellmann '*L'architecture grecque, I-III*', published in Paris in 2002, 2006 and 2010 and comprising 1008 pages (!).⁵ The authors have assembled most of their already published papers and turned them into a book without trying to update their bibliography. They have not used the new results of other studies and integrated them into a coherent monograph.

It is thus difficult to assess the value of this book, although one reviewer has described it as a scientific work. Namely, one cannot say that it is a scientific publication, not even a work of popular science, i.e. a book written for those who are not professional archaeologists, ancient historians, classicists or ancient art historians. The impression is that the authors' aim is to impose an unconditional acceptance of what they say, with a strong bias towards the superiority of the Greeks, and without mentioning other interpretations of the circumstances that are connected with the foundation of the Parian settlement on Hvar.⁶

At the end of the Introduction (pp. 9 and 159) the authors state the aim of their publication: '*...in this place the results of research that relate to the foundation of the Greek city of Pharos in the 4 century BC, when the Greek colonists from the Aegean island of Paros founded their polis (385/4 BC) will be discussed.*' And in footnote 8 (the English section of this book has no footnotes!) they state that '*due to the vast amount of material....the pottery finds will be published in a separate publication: Pharos – the foundation of the ancient city II*'⁷ (my translation). This is standard practice. However, some chapters, such as 'Chthonic deities on the Pharian coins', 'Traces of ceramic production',⁸ or 'Purple workshop' are not related to the foundation

² I.e. Carter 2006; Yntema 2011; Handberg and Jakobsen 2011; Vlassopoulos 2013.

³ Tully 2013.

⁴ Popović 2010b: 145.

⁵ One could say that the authors sent their book to press before the publications I have mentioned became available. Yet this is not the case, because their bibliography (pp. 207–217) includes works published in 2015.

⁶ Kirigin 2004, 82–88; 2006, 64–67.

⁷ Such a publication was already in print: '...see KATIĆ (The Pre-Grecian Settlement and the Beginning of Greek Colonization in Stari Grad on Hvar, in print' (Jeličić-Radonić 2005, p. 316 and footnote 3). The authors do not mention where the other finds such as the coins, the metal and bone objects, the faunal remains, and so on, will be published.

⁸ It is noteworthy that a discussion of the Pharian pottery production is announced for the second volume, see p. 9, footnote 8.

¹ The Croatian version of this review was published in *Vjesnik Arheološkog muzeja u Zagrebu* 49, 2016: 305–311.

of the Greek city. In addition, one would expect that *Pharos I* would include, as is customary, a history of the research and a discussion of what methodology was used during the excavations (the authors mention 'systematic archaeological excavations'), together with a detailed description of the architectural remains within stratigraphic contexts correlated with photographs and drawings. Unfortunately, none of the above can be found.

While perusing the book one notices that the illustrations, which are only within the Croatian text (some 160 of them), do not have numbers. This makes it difficult to connect them with the Croatian text, which is 156 pages long. Therefore one would expect that in the Croatian section of the book the reader would be given directions such as: see the photo on the left, or see the above photo, but this is not the case. As the English section (pp. 157-206) is without illustrations, it is hard or impossible to make a connection with illustrations only found in the Croatian section. Even though the illustrations have scant captions (in both languages), these do not adequately explain what they are about. Their meaning is clear to the authors, but it is not understandable at all to a reader who is learning about *Pharos* for the first time and has never visited the site. At the bottom of p. 35, for instance, there is a photo of a trench without a number or a letter (or anything), so one cannot find it on the excavation plans on p. 34, or on p. 40. The same is true for p. 39, where there are three photos that show walls that are not described in the captions beneath them and the reader cannot know where they are. Also, on p. 42 we hear of trench IA, but its position is not indicated in any of the plans. Throughout the narrative, we often hear about Remete garden, Remete house, and St John's church, but we are not informed about where they are in any of the published excavation plans.

A special problem is that walls are not marked (with numbers or letters, the usual way): hence we cannot know which wall is which (and there are many of them, as can be seen on plans on p. 34, 58, 70 and 93) on photos, ground plans, and profiles. Moreover, on profiles and views of different walls (pp. 38, 48, 52, 54 and 55) it is unclear whether the lowest line represents the bedrock or not. Only in one instance (p. 92) can we see the profile with stratigraphic layers! On it, the layers are marked with numbers from 1 to 4. But, there is not a single word in the text about this profile, there is no accompanying matrix, and we simply do not know where it is situated on any of the plans. If the authors wanted to give us compelling arguments and the possibility to check them, more such profiles would be needed in a publication like this one. It

would also have been critically important to publish here the documentation regarding the Illyrian huts (p. 33, 169) and those of the first Greek settlers (p. 35-36) found in layers 13-11 in trenches 'VII, IX and part of trench X (c 17,5 m²)' and the oft-mentioned profile of trench VII. These are crucial bodies of evidence about which we read something, but which we do not see illustrated. If the authors wanted to provide information about these dwellings, they could have presented their plans in this book instead of showing some ten panoramic views of Stari Grad and environs (without captions) that are decorative but not essential for a monograph of this kind. The c 17,5 m² area is a large space where in plan and profile one would see clearly the earliest huts, especially because the authors mention that this earliest layer is 80 cm thick (from a depth of 340 cm to 420 cm: see p. 33)! According to the authors, this layer is on virgin soil, at the depth of 416-420 cm. However, they do not state the distance of the virgin soil from the bedrock, which they do not mention at all anywhere.

The main excavation plan is on p. 34 and is shown in a rather small scale. On it the position of trenches I-XIII is marked, but an area nearly as large that was excavated next to them by the authors was left unmarked. In this unmarked area, many architectural remains have been found: parts of a street, walls, canals, wells, and fortifications, but it is unclear, and it has not been explained, why such areas have no marks while other areas do.⁹ On plans on pp. 40 and 58 (in the upper left corner of the page) three apses are drawn together with some other walls that are not shown on the plan on p. 34. There are no data that indicate what these walls are. How can a reader know what they represent?

In the Introduction, besides describing different educational activities, Jeličić-Radonić and Katić give a short review of their excavations from 1994 to 2004. But from it we do not know what excavations they conducted from 1997 up to 2003. The authors do not tell us where the data about the results of these campaigns can be found, or where they published their excavation reports.¹⁰

⁹ The authors do not mention the size of the area they have excavated.

¹⁰ E.g., in *Obavijesti hrvatskog arheološkog društva* (Reports of the Croatian Archaeological Society). In their published report (Jeličić-Radonić and Katić 2009), the authors state that they conducted rescue excavations in 2003 and 2004 within the Remete house and that they made an inventory of 253 artifacts from those excavations. However, they do not mention where these artefacts are, nor whose inventory numbers they carry. Later on in the report they mention that they did conservation work on Greek and Roman architecture within Remete house garden. It is interesting that in *Pharos I* the authors do not quote their own published report and also the paper by Sara Popović in the same volume of the journal *Hrvatski arheološki godišnjak*

In the first chapter titled 'Historical sources about the foundation of the Parian colony' not only historical sources but also epigraphical ones are discussed. The authors wish to convince us that there is no more need to rack our brains about the centuries old dilemma: did the eparch of Dionysios I come to help the newly founded Parian settlement in the deep Stari Grad bay from Lissos (Lezhë in Albania) or from Issa (the nearby island of Vis)? They just assume that he came from Issa (a Syracusan settlement) and claim that this assumption is supported by the opinions of B. Kuntić-Makvić¹¹ and P. J. Stylianou.¹² However, neither of these historians are fully explicit regarding this issue, especially Stylianou.¹³ Jeličić-Radonić and Katić's theory is that Issa is closer to Pharos than Lissos (the theory was promoted in the 18th century). What is worrisome is that they do not discuss the arguments and reservations of other ancient historians that have examined Diodorus' passages related to the foundation of Issa.¹⁴ Yet, regardless of that, we still have no archaeological evidence which could confirm that a Greek civil or military settlement existed on the island of Vis at the beginning of the 4th century BC (i. e. prior to the foundation of Pharos).

In this connection, the authors state (pp. 13 and 161): 'Diodorus says that the Parian settlement was founded along the shore, from where the indigenous population of the local settlement was expelled'. Their translation of Diodorus (p. 12, not in the English section) goes like this: 'During that time Parians that have settled Pharos have by themselves founded and fortified their city next to the sea, and the barbarians that have previously lived **here**¹⁵ (my bold) they left unharmed to live in some fortification that was very inaccessible[sic]'. However, a literal translation of the original text by Diodorus (XV, 14, 1) reads as follows: 'At that time inhabiting

Pharos the Parians have left the previously inhabited barbarians to be in peace settled at some exceedingly fortified place, while they themselves founded a city by the sea and built a wall about it'.¹⁶

Regardless of different translations of this sentence in Croatian or in English, we can only say that, according to Diodorus, the Parians left the natives to live where they had lived at some inaccessible height and that they built and walled a city next to the sea. Regarding the conflict Diodorus clearly states that it occurred after the Greeks had founded their city. However, the authors make changes to Diodorus' text by claiming that (p. 33-43, 169-172) they have found, at the site of Remete garden, the Illyrian settlement that was destroyed and burned by the Greek conquerors on several occasions. I will summarise here what the authors want us to believe:

1. p. 33, 169. The Illyrian settlement is at the depth of 340-420 cm (black layer no. 13), below which is virgin soil at a depth of 416-420 cm. The Illyrian dwellings (plural) are made of 'wattle and daub'. Local pottery was found. Remains of wooden posts of 20 to 12 cm in diameter were cut into a loose red soil at a depth of 326-336 cm, and on it Illyrian pottery was found.¹⁷
2. p. 33, 169. The Greeks demolished the Illyrian settlement (layers 13-11a) as evident in trenches VII, IX and in a portion of trench X (cca 17,5 m2).
3. p. 33, 169. After the withdrawal the native settlement was rebuilt at the same place, which is confirmed by remains of huts and by daub 'that was not combusted in the fire'.
4. p. 33. The Illyrians retreated to the nearby small hillfort of Glavica, which had been built to protect the settlement by the sea.
5. p. 33, 169. The return was fatal. In the new Greek attack the local Illyrian settlement was totally destroyed. Greek bronze arrowheads were found. A similar settlement lies near the town of Hvar (on the opposite shore of the island).
6. p. 35, 170. The Illyrian settlement suffered several times and was finally burned by the Greeks that

(Popović 2009) which is directly related to the paper by Jeličić-Radonić and Katić and is related to the same area! But what they did from 1997 to 2003 remains unknown.

¹¹ Kuntić-Makvić 1995, 33-36.

¹² Stylianou 1998, 191-197.)

¹³ Stylianou 1998, 195-196. See the review of Stylianou's book by J. Cargill published in *American Journal of Philology* 121/3, 2000: 483-487 where he mentions Stylianou's statements about Diodorus being a 'second-rate epitomator' (p. 1), and his work 'a work of compilation, and hastily and incompetently carried out' (p. 132), which is a pity as Stylianou says he uses first rate sources which are lost, especially Ephorus who, according to Stylianou, wrote between 330 and 320 BC (p. 110) and writes 'most of the errors to be found in Diodorus' account were produced by inept abbreviation of his mostly reliable main source' (p. 124).

¹⁴ To mention only recent, non-Croatian references, see Vanotti 1991; Fraser 1993; Anello 1999, 122; Alfieri Tonini 2002, 211-216; Lombardo 2002, 431-2, 440; Wilkes 1992, 114-115.

¹⁵ By here they mean at Stari Grad. In another place (Kuntić Makvić 1995, 33 i bilj.2) instead of 'here' stands 'there': 'At that time the Parians that have inhabited Pharos and have by themselves founded and fortified a city by the sea, have left the barbarians that have lived there (my bold) to live unharmed at some fortification that was very inaccessible' i. e. somewhere else on the island and not at Stari Grad (Pharos).

¹⁶ I am grateful to Milenko Lončaru from the University of Zadar who has made this translation at my request. It is baffling that the English version of this passage in Jeličić-Radonić and Katić's book (p. 160) has the English version from the 1954 Loeb edition (similar to Lončar's), which differs from the Croatian translation provided by the authors (p. 11). Why have one translation for Croatian readers, and another and a different one for English readers, one wonders?

¹⁷ According to the authors 'These vessels (are) revealing the conservativeness of the indigenous population' (p. 33 and 169)

were 'painstakingly conquering autochthonous land' (my translation).

7. p. 33, 169. '...the foundation of the Parian colony demanded several actions and fierce conflicts **over a longer period of time**' (my bold and translation).

8. p. 36, 37, 170. The 'first dwellings of the Parians'. Surviving natives retreated to the hillfort (no. 4 above). The Greek settlement is built of wooden materials (layer 10) that 'contains mainly fragments of Greek ceramics (fine black glazed vessels such as bowls with a single handle, of a Corinthian black-glazed cantharus, a black gloss scyphus, parts of a black gloss lucerna, fragments of mortars, amphorae and kitchenpots) with the occasional fragment of a vessel of local production, and two examples of bronze coins of Paros' were found.

9. pp. 36-38. After the attack of the Illyrian coalition, Dionysius I's eparch and the Parians defeated the Illyrians.¹⁸

10. pp. 35-36. The natives led by the Iadassinoi (a Liburnian community in northern Dalmatia) severely attacked the 'just founded Greek colony'. Confirmation in Diodorus (XV, 14,2). Layer 10 represents the 'totally destroyed Greek settlement'. This layer overlies layer 11a, which represents the burned Illyrian settlement.

11. pp. 37-38, 170-172. The Greeks built their first houses made of stone in trenches VII, IX and X, (layer 5) 'above several earlier layers that represent the Illyrian settlement that was destroyed several times together with the first Greek settlement'.

12. pp. 45-52, 67-69. After some time, i. e. 'before the mid-4th century BC' a new layout of the town and the east fortification wall with a gate defended by towers appear. Because of this, the first Greek houses made of stone were demolished and new Hellenistic housing blocks were constructed. Here we learn that Hellenistic blocks date before 350 BC!

According to this it follows that from the first Greek wooden houses to the first houses made of stone and to the new, now urbanistically planned buildings with a fortification that obliterates the first stone-built houses, no more than 30 years have elapsed! If the Greeks had to undergo such torture to survive, far away from their homeland (some 700 NM), what resources did they have? The Syracusans could not help them much, because they were at war with the Carthaginians from 383 until 375 BC.

¹⁸ According to the authors (p. 36-37, 170) 'At that moment the opposition of the autochthonous inhabitants was totally defeated as they were in terms of military technology and material culture less developed and easily vulnerable'.

In a chapter titled 'Survey of research into Pharos' (pp. 25-31 and 166-169) one notices that the authors do not mention, nor use, nor do they critically acknowledge, the research of M. Nikolanci, J. Barbir, M. Zaninović, and N. Petrić, let alone the research by the international projects 'Hvar – archaeology of the Mediterranean landscape' and 'Adriatic island project'.¹⁹ This shows that the authors either do not know of their existence, or have just ignored them, which is unconscionable by academic standards. What is more annoying is their total lack of awareness of the papers by Sara Popović²⁰ that are directly connected in time and space with what the authors are presenting in this book. The papers by Popović must have been known to the authors, as the first one (2009) was published in the same journal (*Hrvatski arheološki godišnjak/Croatian archaeological yearbook*) where the authors also published their findings (see footnote 10 here), and the other one (2010a) appeared in the same publication to which one of the authors contributed a paper.²¹ One of the main results of Popović's work is the analysis of the eastern fortification wall. The authors claim that the wall is the original wall of the Greek city from the 4th century BC. Based upon the results of the excavations by Sara Popović, the eastern fortification wall (marked as no. 1 on fig. 9, p. 142 in her 2010b report) leans upon wall 3, which cannot be characterized as a fortification but is obviously of Greek origin. In the authors' view, the east fortification wall of Pharos was built 'before the mid-4th century BC' (p. 69 and 179). Therefore wall 3 should be even earlier. Although the authors draw that wall (no. 3 in Popović) on plans found on pp. 34, 40 and 58, they make no mention of it at all. Wall 3 is built in the same way as the wall of what the authors describe as the first residential block (the one that has an added threshold as seen on their photo on p. 73) which they connect with the building of the east fortification wall. But wall 3 disproves this. It is logical to conclude that the east fortification wall was built later, most probably at the end of the 3rd century BC. Thus, the notion that the eastern fortification wall was built in the mid-4th century BC is not a 'definitive' (p. 5) fact as the authors would like it to be.

According to the documentation of the cistern with channels going in and out²² situated west of Remete

¹⁹ Forenbaher et al. 1994; Gaffney et al. 1997; Kirigin 2004, 37-38, 66-73, 82-88; 2006, 23-26, 48-58, 64-67.

²⁰ Popović 2009; 2010a; 2010b.

²¹ Jeličić Radonić 2010. As an example of the fact that this omission must have been deliberate the authors in the Bibliography (p. 215) mention a paper of Sara Popović that was published in 2012 and deals with the quarries in the Stari Grad bay (Popović 2012).

²² Popović 2010b, 142-143, fig. 10-11, 15 and 16; the authors, pp. 86-89.

house it is evident that the cistern does not have any connection with their second residential block (p. 40 and 93). Namely, as Popović²³ has shown, no walls that incorporate the cistern with or within the second residential block to the north were found to the north of it (although this area was excavated all the way to bedrock). It is more likely that the cistern was a public utility, not a private installation within a house. Next to that on p. 93 the authors provide a reconstruction of the second residential block without any proofs or indications about the existence of the south and west wall of that block, not even the northwest corner... What is more questionable is that the second residential block serves for them as a module for the rest of the blocks (p. 109), for which there is no material evidence. Afterwards they mention that the residential block is 100 Attic feet long, i. e. 29.84 m, and 75 Attic feet wide, i.e. 21.78 m. But the Attic foot is 29.57 cm,²⁴ which indicates that their residential block measures 29.57 by 22.17 m. Because of this it is impossible to understand whether the builders of Pharos used the Attic foot or some other Greek foot, for instance the one that was used for the division of the chora of Pharos, which is equal to 30.26 cm.²⁵

I will just turn to one more inconsistency. It is about the coins whose photos are seen on pp. 37, 83, 118-121, 123, 125-127, 136, 139 i 155 – all together 51 of them. Only for 18 of them (listed on p. 127) bearing the legend 'Early issues of Pharian mint' are there specific catalogue entries (pp. 128-130). There is no mention in the catalogue of the layers from which the coins come. Two of the coins are said to have been found in trench A, but we cannot read anything about this trench in the book. Moreover, two of the coins do not belong to the Pharian, but to the Parian mint (nos. 10 and 11). To the Parian mint also belongs coin no. 12, which does not come from Remete garden but from the collection of Zagreb's Archaeological Museum (inv. no. 3078) that was published by P. Visonà;²⁶ his paper is not mentioned in this book.

I have to ask: why do the other 33 coins (the photos of some of them are illustrated twice without explanation) not have catalogue entries? Why are they shown here? Although the same catalogue had been published previously,²⁷ the authors do not mention this in their coin entries for this book.

On p. 155 there is a fine photo of a silver coin of Pharos (Zeus/goat with legend ΦΑΡΙ in the exergue

(the space underneath the goat), not in the egzarch (Greek ἑξαρχος), as the authors write in several instances (pp. 123, 125 and 128): an ἑξαρχος was a governor of a particular territory in the Byzantine Empire. In the chapter titled 'Early issues of Pharian mint' (pp. 117-130, 190-195) the authors do not say a word about the coin types on these silver issues of Pharos, although they discuss the same types found on Pharos' bronze coins.

I can also add that while most coins mentioned in the text, e.g. on p. 40 (inv. no. N 614), p. 42 (inv. no. N 614 i 647), p. 43 (inv. no. N 684), p. 53 (inv. no. N 440), p. 75 (inv. no. N 645) and so on, have no catalogue entries, some of them do: for instance: p. 82 (inv. nos. N 223, 224, 380, 381, 486, 492, 493). But when the latter ones are mentioned, it is not stated that they are catalogued in the book: the reader has to find this out for him/herself.

What is important when bronze coins of Paros type with Demeter/goat are discussed is that, according to the analysis of J. Tully,²⁸ there is no evidence that such coins were minted in the early 4th century BC. Instead these coins were minted in the early Hellenistic period. If our authors could provide evidence of their claim that the coins of Paros found in trench A in Remete garden (whose location is unknown to us) are datable to the early 4th century BC, this would be an important contribution. It would prove that the Parian mint was issuing coins at the beginning rather than at the end of the 4th century BC!

In other chapters of this book there are many questionable issues that are outside the scope of a journal review. One gets the impression that the authors do not need evidence to reach the conclusions that they present. Writing a book about this book would be a solution, but it is not feasible under current circumstances.

After the remarks that I have made here one could ask: what is the purpose of this book? To me it creates more problems than it solves. Maybe the second volume that has been announced – *Pharos II* – will eliminate all the uncertainties and doubts that I have expressed.

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Alfieri Tonini, T. 2002. Diodoro e la colonizzazione adriatica di Siracusa, in L. Braccisi i M. Luni

²³ Popović 2010b, figs. 15 and 16.

²⁴ Oxford Classical dictionary, Oxford 1996, 942-943 s.v. Measures.

²⁵ Stančić and Slapšak 1988.

²⁶ Visonà 1993.

²⁷ Dukat and Jeličić-Radonić 2012.

²⁸ Tully 2013.

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Diana Rodríguez Pérez (ed.). *Greek Art in Context: Archaeological and Art Historical Perspectives*. pp. xxiii + 282. 59 illustrations. 2017. Abingdon: Routledge. ISBN 978-1724-574-5 5 hardback £84.

As the editor observes, 'context' has become a buzzword (p.1). Why? The editor has concluded, following the 2014 Edinburgh conference designed 'to discuss the issue [of context] and clarify its terms', from which this book derives, that 'How each of us understands the term depends on one's field of specialization and on the nature of the material under study' (p.12). That seems to me nonsense. What context is relevant depends on the question being answered. It is not whether something is a pot or a statue, or whether I am an historian or an archaeologist or an art historian that matters, it is what I want to know. If your question is about pot-painters' hands, then the appropriate context is other pots displaying similar graphic habits; if your question is about pots representing peltasts, then the appropriate context is other pots with pictures of peltasts (or from which peltasts are surprisingly absent); if your question is about pots in (Athenian or Etruscan) graves, then the appropriate context is the assemblages found in (Athenian or Etruscan) graves. The growth of interest in 'original' contexts of deposition was not a consequence of a growth in the number of specialists in excavation, but of the growth of interest in questions about first social and economic, and later, religious life. The more recent growth of interest in the museum context is not because of a growth in the number of curators, but because there is more interest in questions about how modern display contexts influence people's assumptions about antiquity. And so on. It is not in fact the case that 'contextualization' has become more important, or new that 'considering 'things' in context' is 'the ultimate scientific approach' (p.1): everyone who has ever written about anything or anyone has done so by putting them into a context. The more vocal insistence by archaeologists on looking at things 'in context' has been part of a move to change the questions asked, and to insist that the most fundamental questions are the questions which relate to the context in which an archaeological object has been found.

The contents page reveals the importance of the question fighting to get out. For although the introduction lists as types of context 'The find-spot', 'Secondary find-spots', 'The Museum as context' and 'Texts as contexts', the volume itself is organised into sections named 'Location and the find-spot', 'Experiencing material culture', 'Historical and

artistic contexts', and 'Recontextualizations'. What is 'experiencing material culture' doing here? How is this not a matter of 'historical and artistic context'? Here, for a moment, the question of 'How was X experienced?', a question that turns out to involve different contexts depending on exactly what X is, has taken precedence over starting from a context and then searching for what questions can be answered on the basis of that context.

'Experiencing material culture' starts with a paper by Bonna Wescoat and Rebecca Levitan in which they seek, in the face of recent denials from Clemente Marconi, to answer the question of whether the Parthenon frieze could be seen when in situ, and whether its relief and/or colour helped its visibility. To answer that question required putting the frieze into its original viewing context, something impossible on the Athenian acropolis but possible on the full-sized replica Parthenon in Nashville. The answers are clear. Although different viewers were more or less able to distinguish detail, both colour and relief aided visibility and very few had difficulty distinguishing the figures when coloured. The vast majority also found themselves wanting to move with the figures on the frieze, and that the columns helped the viewer to focus. In terms of the modern debate, this is pretty decisive support for those against whom Marconi was arguing.

Winfred van de Put isn't really asking a question about experience, he just happens to offer different experience as an answer. He describes how he became aware that the iconography on sixth-century lekythoi from Attica was different from the iconography on sixth-century lekythoi from Athens. Unfortunately, we are given a great deal of abstract discussion of methodology, in which van de Put initially seeks the moral high ground for questions arising from archaeological context above questions arising from other contexts, only finally to concede that there is no objective context, but we are given no data to illustrate his claim about the difference between Athens and Attica, other than the mention of a single lekythos; the suggestion that this might correlate with different experiences is not developed.

Katerina Volioti asks how repetition of similar iconography in Athenian black-figure pottery was experienced by the viewer. She first shows examples of repetitive iconography from the Haimonian group of black-figure cups and lekythoi and then suggests that repetition aids 'processing fluency' as well as creating a 'brand'. Effectively she shows that there is a context, the theories of modern cognitive psychology, which will enable us to make sense of