

Simone Rambaldi

In 2009 I published a monograph entitled *L'edilizia pubblica nell'impero romano all'epoca dell'Anarchia Militare (235-284 d.C.)* (Ante Quem publishing, Bologna). In 2011, in the number 24.2 of the present journal (822-30), a long review signed by Hendrik W. Dey came out.

I am glad the reviewer dedicated to my text a detailed analysis, the result of a precise and pondered reading, and he chose such a prestigious place for presenting his discussion. But since the debate among scholars should be a continuous dialogue, I believe some explanations are necessary, in order to help a better understanding not only of my reasons, but perhaps also of Dey's reflections.

My monograph about the Military Anarchy was the outcome of an almost ten-year and much complicated work, first of all for the vastness of the considered subject: fifty years of public building in the entire Roman Empire, which I studied examining, on the basis of all the available sources, the interventions of new construction or restoration executed for any type of edifices. The achieved results led me to conclude that the documented building activity shows a substantial continuity of life in Roman cities. This is in line with the directions of many modern historians, who had pointed out that *Soldatenkaiser's* period was not characterized by the catastrophic crisis frequently enunciated in the past. The complexity of my study involved a series of problems and hard choices that have nagged me for a long time, both when it came to plan the research and during its execution. So much so that, with all humility, I held necessary to put before the text a methodological note, in which I endeavoured to explain how I proceeded and the grounds of my choices (pp. 18-24)¹. But the reviewer seems to bear in mind these preliminary remarks only in part, even if he mentions their existence (824), otherwise he would not have reason to raise some of his methodological objections. Dey, for example, is surprised I did not include imperial palaces and mausolea in the catalogue of building interventions of my volume (823-24): but I had explicitly said the exclusion of imperial palaces was a choice of mine (23), certainly not an oversight (elsewhere in the book, however, I remembered all the known data about them)². It is questionable, of course, like many choices are: I perfectly know that such edifices showed 'public' features differentiating them from normal 'private' edifices, but it was necessary to take sides. When one is working on an essay of this type needs to make even drastic decisions, otherwise one cannot go on. It can often be hard to understand which way is the best, but it is important to safeguard one's own intellectual honesty, in the awareness that other solutions are always possible. Since I wanted to catalogue only edifices having a completely public use and purpose, like baths, theaters, amphitheatres, but also structures like city walls (22-23), it seemed natural to me to exclude palaces and especially imperial mausolea. In that I was encouraged by the fact that, in the archaeological literature, such realizations are usually considered within the private building, though with all the necessary distinctions: so it was done, for instance, in the most important synthesis of Roman architecture published in the last years, that of Pierre Gros³. I find it truly difficult to consider mausolea, in particular, "public edifices" to all intents and purposes. At any rate, I repeat, it is a

¹ When I will make a textual reference, I think there is no need to formally distinguish the pages of my book from those of the review. All the pages numbered from 822 onwards refer to Dey's text.

² In another point, taking up again the things "which Rambaldi omits", Dey adds mints (827). Actually an intervention in a mint, the only intervention documented in the period, does appear in my catalogue (no. 160: mint of Rome), as the reviewer himself oddly records elsewhere (823).

³ *L'architecture romaine du début du III^e siècle av. J.-C. à la fin du Haut-Empire*, 2. *Maisons, palais, villas et tombeaux* (Paris 2001) 231-61 for imperial palaces; mausolea are not even treated separately, but in the general context of tombs (428-35).

matter of choices: if I had decided to catalogue these buildings, another reviewer could have reproached me for having done so⁴.

Dey notices that, if I had accepted within the catalogue the many doubtful cases I remembered in the apparatus, I would have doubled the 334 entries of my corpus (824). One of my major difficulties was just to decide how I should behave in such situations. In the end, in addition obviously to the interventions of indubitable dating, I chose to admit those monuments for which, on the basis of what was known, I felt more well-grounded a chronological position, if not within the reign of a precise *Soldatenkaiser*, at least in a relatively limited time space within the Military Anarchy, as I made clear (23-24). Therefore I decided to record in the footnotes the interventions that, in the archaeological literature, result generically dated to the 3rd c. A.D., rather than completely ignoring them. It was surely a risk, but here too I had to take a position and I took it, trying to justify my choices and repeatedly warning the reader against the many uncertain cases. As we shall see afterwards, Dey reports that I ended up by considering several unsure situations as well, so it is strange he is surprised for the caution I used about the by far most problematic cases, which I confined in the apparatus.

Another thing I felt modestly bound to acknowledge is that, being up against such a vast and complex work, it would have been reckless to aspire to a total control of all the available documentation (18-19). In all conscience I do not know who, studying by himself the entire panorama of the public building activity of a fifty-year period and within the whole space of the Roman Empire, could believe to have succeeded in reading and seeing everything. My work was declaredly a work of synthesis, because in no way it would have been possible to study in depth every single matter. Dey notices some bibliographical lacunae: in several cases it was an intentional thing, because, in order not to make the single entries of the catalogue heavier, I gave up the idea of quoting works anyway mentioned within those I actually quoted, as I specified (21). But it was unavoidable – I have no difficulty in acknowledging it – that there were unintentional lacunae too. My work started in 2001 and prosecuted for nine years: for a single person it was absolutely impossible, on such a boundless subject, to maintain a continuous updating on everything he was elaborating. The theme of this research was not a single circumscribed subject, on which it would have been infinitely easier to exert a full control. It was instead a very intricate tangle of topics and pieces of information of every kind that it was necessary to weigh up and interpret, in the context of an immeasurable bibliography, without it was always possible to retrace one's steps. In any urban centre of the Roman Empire, even the smallest one, at any time one could have detect a (archaeological, epigraphic, literary) datum that was worthy of consideration. Besides – and it is another thing I felt bound to clarify (17) – the print delay of my volume was not brief, for reasons beyond my control. Unfortunately such circumstances are far from uncommon, especially in Italy and in times like ours, of crisis without quotes. Even Dey, anyway, can overlook something: among the many recent texts he quotes in the 43 footnotes of his long review, he omits to remember the work now by far the most exhaustive on the Military Anarchy (as I like to go on calling it, even if on p. 824 Dey seems to disapprove of it: it is a conventional expression one can be fond of or not – like the “Severe Style” in Greek sculpture – but it has the advantage that, when a scholar is using it, everybody understands what he is talking about). I am referring to the great treatment edited by Klaus-Peter Johne, *Die Zeit der Soldatenkaiser. Krise und Transformation des römischen Reiches im 3. Jahrhundert n. Chr. (235-284)*, published in two ponderous tomes in Berlin in 2008, which unfortunately I was not in time to consider for my work, but which at least I succeeded in reporting briefly.

⁴ Dey is amazed that, on the contrary, I accepted two “unrealized complexes” (“complessi irrealizzati”: nos. 136 and 141), we have knowledge of only through the *Historia Augusta* (824). I did so exclusively because it seemed interesting to me to observe even two ‘projects’ that one of the most significant literary sources, though rightly criticized, attributes to two of the most important emperors of the period in question. They are pieces of information useful to better understand how building activity was reckoned as an effect of the policy of a *princeps*.

Up to here, nevertheless, we have remained within lightweight matters. I could still add some incidental remarks made by Dey which frankly puzzle me. For instance, apropos my catalogue he points out: "The 33 Roman entries appear largely to derive from a perusal of the *LTUR*" (824, n. 11). I do not understand how this could seem unbecoming: why are works like the *LTUR* published, if not to assist scholars, helping them in their research? Sure enough it is widely used by all those dealing with the topography of Rome. Whenever I followed the *LTUR*, however, I always duly quoted it, and anyone would have done well to criticize me if I had not done so. But I do not think I have to justify myself for things like that. I am anxious to reach more important and interesting matters.

Dey notices a "cognitive and formal disjuncture" among the opening chapters of the book (where I tackle in order: sources and general historical problems of the period, building activity under the single emperors, patronage of the interventions and respective distribution and typology) and the catalogue (826). If I have rightly understood, he draws such an impression from this: while on the one hand I agree to the idea – shared by many scholars today – that regarding the *Soldatenkaiser's* decades we should not speak of a real crisis, on the other hand the data provided by the catalogue would not be sufficient to confirm this idea, chiefly because we cannot fix exactly not only the dating of a great deal of interventions, but also the patronage. Yet here too I proceeded with the greatest caution, and I myself formulated some doubts advanced by Dey (especially 93-94). I stated there were too many uncertain cases to be able to attempt an even hypothetical estimate of how many interventions were to attribute to each patronage rank, so I chiefly tried to detect the fundamental guidelines in the activity of the diverse 'constructors'. Here it is impossible to me to go over every assertion of mine again, however the considerations I put forward in the opening chapters (especially 4 and 5) were suggested to me essentially by the analysis of the pieces of information I collected in the catalogue. I have drawn from it the opinion that, in the public building of the age, the contribution of the local or private initiative continued to be important, as it always had been before, particularly in the provinces. But I have recognized the most documented patrons are emperors, as after all Dey himself believes. And I too have developed the idea that the central authority was the main responsible for the construction or restoration of urban walls, as it was however decreed by law, even if we know it did not happen in any case (98-99, 126-27).

The construction or restoration operations of many city walls pose serious problems of chronology, which we have to approach with the greatest circumspection. Dey focuses in a particular manner on questioning what I said about many city boundaries of Gauls, because, despite the historiographically 'updated' vision showing itself in the opening chapters of my book, on this point I would have stuck too much to the oldest bibliography (827). Nevertheless I think to have never written that, for these realizations, we can usually rely on a sure chronology, while I strove instead, as usual, to be prudent. The clues we can trace in my opinion license the hypothesis (mark well, the hypothesis) that, around 260 and especially in 275-76, several Gallic towns were fortified. In those years invasions did happen, as registered by later sources, which did not invent non-existent events but certainly exaggerated them (for the reasons, see 48-49). The enemy aggressions of the 3rd c. A.D. were fundamentally episodes of not devastating foray, but sufficient to recommend the restoration or construction of the urban fortifications, when it was possible. The new city defences, like the Aurelian's walls themselves in Rome, in their first phase, were executed in a hasty manner and were not capable of withstand really massive attacks⁵. But Dey is inclined to delay many walls in my catalogue to the 4 c. A.D., when the most dangerous invasions started, overlooking the elements which can often suggest previous defensive cares, in certain towns at least. The fact that I was disposed to accept in my catalogue so many interventions to wall circuits (98 in all the Empire) has not to seem a contradiction with my more 'optimistic' vision

⁵ The frequent recourse to salvaged material cannot simplistically be put down always to the 'hurry', because it justifies itself in another way too (125).

about the Military Anarchy: it is not sufficient to count the number of the interventions, but it is necessary to value their effective nature. The period under examination is undoubtedly marked by weighty problems, among which we have to number barbarian raids. But the general picture we can draw is not that of a cataclysm, because in those towns themselves concerned by fortification programs – however situated in the areas most exposed to the risk of invasions – life ran and went on running its course without particular breaks, not only in the Gauls (133-35).

I hope not to try the patience of the reader, if I now attempt to briefly reply to the detailed criticism which seems to me less justified in connection with the urban walls. I think it is important to do so, because Dey uses this topic to warn the reader against the reliability of the data included in my catalogue (827-30).

Bordeaux: The reviewer records I dated the circuit “after 270”, adding “but there exists a detailed publication of the walls”, quoted by him in a footnote. In this publication I read that there are no irrefutable elements for proposing precise chronologies, that it is certainly simplistic to connect all the Gallic boundaries to the invasion of 276, but that however for the fortifications of Bordeaux we can suppose a dating “dans le dernier tiers du III^e s.”⁶.

Poitiers: Here too we are in the field of conjectures, in any case Dey notices that, in asserting a dating from A.D. 275, I quoted “three atlas/gazetteer/manual works”. Since he does not specify my sources, I will do it, because his words could insinuate into the reader’s mind that I used low-profile works. With “atlas” the reviewer surely refers to the *Atlas des villes, bourgs, villages de France au passé romain*, by R. Bedon (Paris 2001), an important work and not a popular one, as perhaps somebody could presume from the title. In the “manual” one has instead to recognize the *Manuel d’archéologie gallo-romaine* by A. Grenier, published in several volumes in Paris between 1931 and 1960. Maybe the term *Manuel*, within the title, makes Dey think to a superficial treatment, but it is still a basic work, even if obviously old, which no serious scholar of Roman Gaul archaeology could do without. It being understood that I consider rather curious to judge a text only by the title, I would like to point out how it is always a gross mistake to think the oldest bibliography is not worthy of notice anymore. It is plain it has to be integrated with the most recent studies, but it has not to be forgotten. There is the term “gazetteer” left: if my command of the English language is not too imperfect, I reckon it should be referred to the Bedon’s *Atlas* as well. The third text quoted by me is in fact an essay published in the proceedings of a French conference⁷, therefore it cannot fall within the threefold bibliographical typology put forward by Dey.

Tours: It is true that I dated the walls “to c.275, contra the author of two summaries” which I cited (Bedon again), but I recorded there is a later chronology (168, n. 76) and said the controversial Eusebius’s fragment “seems referable” (“sembra riferibile”) to the invasions following Aurelian’s death. Here too, as always in similar cases, even if I took sides I did not do it in a peremptory manner.

Rennes: Acknowledging I correctly gave “a terminus post of 273 for the start of construction”, Dey says “the primary publication of its one well-excavated section is the best source”. This is one of those cases in which I did not quote a text because it was already mentioned in one of the texts I cited: being a publication of 1958, I have to notice that, in this occasion at least, my reviewer does

⁶ P. Garmy, L. Maurin (edd.), *Enceintes romaines d’Aquitaine. Bordeaux, Dax, Périgueux, Bazas* (Paris 1996) 75-76 and especially 193.

⁷ B. Boissavit-Camus, M. Fabioux, N. Le Masne de Chermont, “Poitiers (Vienne). Lemonum, Limonum, Civitas Pictavorum”, in *Villes et agglomérations urbaines antiques du Sud-Ouest de la Gaule. Histoire et archéologie: Deuxième Colloque Aquitania, Bordeaux 1990* (Bordeaux 1992) 129-33.

not shrink from having recourse to a much less recent text, where it is still asserted that the best part of the Gallic walls were erected “à partir de la deuxième moitié du III^e siècle après J.-C.”⁸.

Le Mans: Dey is right in declaring the undersigned “entirely overlooks this”, because in my catalogue it is absent “the best-preserved and best-published of all Gallic wall circuits”, most probably datable “in c.275-80” thanks to the study by J. Guilleux⁹. To justify myself I can say that I followed the Bedon’s *Atlas* again, which dates the circuit “probablement entre 285 et 295” (therefore outside the period interested by my research) and does not quote the Guilleux’s work, it having been published the year before. But if for Le Mans the chronology now seems to be surer than elsewhere, I think it is an element which could support the dating I proposed for many Gallic walls and which Dey questions. An isolated fortification intervention for a single town would have been of little importance, while it would explain itself better as a reply shared by several towns that were exposed to assaults in those years. It would account for the similarities in the building technique employed too¹⁰.

I am going to leave out the Cologne case in Germany, because Dey quotes about it a volume of 2006, which was unfortunately not possible to me to consider. I want instead to stop for a moment to examine the Central Northern territories of Italy, where several interventions of defensive nature were effected under Aurelian, in connection with the Alamannic invasion of his age. Dey is inclined to admit – but without clarifying why – that for three towns (Parma, Piacenza and Reggio Emilia) “a date somewhere in the second half of the 3rd c. is possible”. However I do not understand why he asks: “how might these projects relate to the incursion of the Alemanni and Iuthungi?”. I am asking: well, what should we relate them to? Early in Imperial Era several towns in Italy were provided with walls without a specific reason, if not that of furnishing them with an element which was believed to be particularly prestigious and important in order to completely define the image of an urban centre¹¹. But such a reason could not absolutely justify initiatives like these during the 3rd c. A.D., besides for a not exiguous number of towns. It was instead essential a pressing and really specific cause. I do not even understand why Dey states: “It is inconceivable that such extensive efforts were made in the chaotic months leading up to the expulsion of the raiders”. It would be too long here to go over the problem again, even because, in order to grasp it better, it would be necessary to clear up the relation among the events of the Aurelian’s age and the preceding descent of the Alamanni in Italy in 260. It is a matter I already investigated in a different place¹². But above all I accept by no means the following reflection: “if Rambaldi had taken every similarly undatable restoration to extant public building across the empire into account, his sample would have increased by a factor of at least 10”. I never took data haphazardly, but I always strove to justify my choices and to consider the problems created by each and every case, as I dare to hope what I have written up to here is enough to demonstrate.

As finally regards the walls of the Iberian cities, I only say that, in questioning my dating “to c.260” for the walls of Tarragona and Barcelona, Dey seems to have not noticed I added a “?” to it and I dwelt on how these date are to interpret (117). I thank him for having admitted that an important and recent collection of essays on the theme, in which the already previously proposed

⁸ P. Merlat, “Rapport sur la portion du mur d’enceinte gallo-romain de Rennes découverte 18, Quai Duguay-Trouin”, *Annales de Bretagne et des Pays de l’Ouest* 65.1 (1958) 97-134, especially 99-100.

⁹ *L’enceinte romaine du Mans*, Saint-Jean-d’Angély 2000.

¹⁰ I agree with the idea that in our judgments we cannot base ourselves only on the technique, because we find the *petit appareil* with brick courses later as well, and therefore it cannot be a sure dating criterion. It is another thing I acknowledged (126).

¹¹ P. Gros, “Rapport de synthèse”, in *Les enceintes augustéennes dans l’Occident romain (France, Italie, Espagne, Afrique du Nord): Actes du Colloque International, Nîmes 1985* (Nîmes 1987) 159-64; F. Rebecchi, “Les enceintes augustéennes en Italie”, *ibid.* 129-50.

¹² Since, within his review, Dey is quoting no less than four works of his (one of which is forthcoming), I think I may well mention my article “Aureliano in Cisalpina. I riflessi delle invasioni alamanniche nelle testimonianze archeologiche”, *OCNUS* 14 (2006) 207-36. Here I posed a generally disregarded problem too: were basically these fortification interventions executed before or after the barbarian attacks?

Tetrarchic chronology for many walls of *Hispania Tarraconensis* has been confirmed, has come too late for being considered by me¹³. But it is not true, as he writes in his n. 42, that the undersigned “gives serious consideration to the idea that these walls date c.260”. After remembering that such walls were once dated to the age of the raid of the Franks under Gallienus, I wrote: “The chronology of these structures, however, needs a revision”¹⁴. I am glad this revision has been done.

Towards the end of his review, Dey recommends “great caution if using Rambaldi’s statistical data” (830). But I am the first to declare this. Whatever statistical estimations I tried to propose exclusively apply to the documentation I collected, being conscious of the provisional character and uncertainty of a not small part of these data. That is why I did not get tired of repeating that many pieces of information are not sure, as I have already said above. Nevertheless we have to keep in mind this: whatever is the specific subject we are dealing with, statistical estimations under no circumstances can be taken as gospel truth. Even if we could have the absolute certainty that, in our research work, we succeeded in collecting every, but every available data, we have never to forget that the picture will be anyway conditioned by the objective fortuitousness with which data have arrived to us. We are always confronting with only a limited portion of a whole that was infinitely wider¹⁵.

We have to consent to the fact that our documentation will always be marred by lacunae. These can come from the distraction of the scholar or from the fortuitousness in the information transmission, however they are there and, even if we do not notice and think of them, they distort our interpretation. Therefore, when we happen to manage a mass of data, we have not to worry as much about the numbers as about the proportions in the data arrangement. But in any case it is necessary to attempt an overall interpretation, even if it involves a certain amount of risk, because confining oneself only to an operation of information collection is no good without trying to draw any conclusions. Nobody can claim to reach final results in such circumstances, because of the arguments I have just set out and the endless becoming of the scientific research. All the more reason for a so intricate period and now object of deep revision like that I dealt with. Nevertheless I am always convinced that, on the basis of the documentation I collected, in the building activity of the Military Anarchy we can detect some trends fitting the present direction of the historiography about the ‘crisis’ of the 3rd c. A.D. Details are surely perfectible, but it is the global picture that counts. From this point of view, what we get from a sector like the public building one, that was never so far investigated in an exhaustive manner, seems to me to coincide with the positions of the best part of today’s historians, confirming how life in Roman towns essentially took its course without being radically changed by the difficulties of the period.

In summing up, Dey notices the “rather general and unoriginal points” that “can stand”, among which there is “that the relatively long and busy reigns of Gallienus, Aurelian and Probus witnessed a profusion of building activity in comparison with was accomplished under most of the other mid-3rd c. emperors” (830). I allow myself to point out that in the past, in the scientific literature, a particular awareness of a great building activity under Gallienus and Probus was not emerged (the Aurelian’s situation is different, but above all by virtue of the walls of Rome), what results instead from the interventions I collected (85 for Gallienus, rising to 107 if we count those

¹³ A. Rodríguez Colmenero, I. Rodá de Llanza (edd.), *Murallas de ciudades romanas en el Occidente del Imperio. Lucus Augusti como paradigma: Actas del Congreso internacional, Lugo 2005* (Lugo 2007).

¹⁴ “La cronologia di questi impianti, tuttavia, necessita forse di una revisione” (220, n. 220). Therefore I did not accept these initiatives in my catalogue.

¹⁵ In the introduction to a very recent book of his on the Greek *apoikiai* in South Italy and Sicily (*Fondazioni greche. L’Italia meridionale e la Sicilia (VIII e VII sec. a.C.)*, Roma 2011, 16), Pier Giovanni Guzzo makes the following general remarks, which I translate from Italian: “Archaeological data are, by definition, fortuitous in cognition and incomplete in comparison with the whole they constituted in ancient times. Fortuitousness and incompleteness are conditions reflecting in a negative manner on the reliability of the results elaborated by the research, on many fronts. The general result we would aim at applies to the whole of the current knowledge of the not only material configuration of the ancient world”.

effected in the period of the joined reign with the father Valerian as well; 45 for Probus)¹⁶. Among these there are some uncertain actions, that Dey surely would not be inclined to accept. But if he notes what he has noted in the just quoted passage, it means he too ends up by agreeing with the essence of the material I collected on the subject.

I fully agree instead with what the reviewer proposes in the last lines of his text: "What is called for, it seems to me, is a (probably never-ending) collaborative project on public buildings throughout the empire over its entire chronological span. Specialists from many different fields would contribute to a searchable, flexible and infinitely-updatable online format". A data base of this kind would certainly be very useful. But if, starting from this, we would draw an overall view of a given phenomenon or period, we should anyway stop and consider 'suspended' for the moment the information collection. Otherwise anyone, a single scholar or a team, would be overcome by the endless mutation of the available documentation: anything could be already old just as we are dwelling on examining it, because probably another thing has already been modified in another place, so altering the global perspective. Such an analysis like the one I strove to do is aimed at showing the state of the art in a given moment, because after a while it is necessary to stop to take stock of the situation. Dey concedes that my work is perhaps too ambitious for a single researcher (830), but I continue to believe more profitable that a reflection of this kind, even with its efforts and dangers, is attempted by a single person. A team of researchers would certainly exert a more analytic and organized control in the collection of the data, but, when it would have to gather up the threads of the whole, it would much hardly succeed in working out the overall view of the phenomena, that is possible, instead, to a single individuality. Though imperfect, the work of a sole scholar can always aspire to a more general and organic interpretation of a series of phenomena, all the more reason in connection to a subject so hard to penetrate in its entirety as the Military Anarchy or 'crisis' of the 3rd c. A.D. is.

It is not so difficult to insert entries into a data base, and above all it poses few problems, when the standards to respect have been rigorously predetermined. But a scholar of the ancient world must not be afraid of facing the problems, because in the scientific research it is very arduous to reach interesting results without difficulties. Of course there is always the risk of making mistakes in good faith, but it is a risk that, in my opinion, is worth running. This is a matter of choices, too.

¹⁶ On p. 135 of my text I had stressed that the values are to be taken as an indication and we have always to carefully consider on an *ad hoc* basis.