

REVIEW

ROME'S EARLY GROWTH

Adam Ziółkowski, *From Roma Quadrata to la grande Roma dei Tarquini: A Study of the Literary Tradition on Rome's Territorial Growth under the Kings*. Potsdamer Altertumswissenschaftliche Beiträge 70. Stuttgart: Franz Steiner Verlag, 2019. Pp. 352, figs 17. Paperback, €58.00. ISBN 978-3-515-12451-5.

As the title of his book makes clear, Ziółkowski's aim is to trace the growth of Rome, from the *Roma quadrata* of 'Romulus' to the great Rome of the Tarquins. The subtitle announces that this is a study of the literary tradition, but it is already apparent that more is involved. '*Roma quadrata*' is Latin, but 'la grande Roma dei Tarquini' is not. Ziółkowski is actually interested in Rome's historical growth, and so in the archaeological evidence too. Since that evidence is not explicitly included in the title, a relationship between it and the literary evidence is inevitably implied from the start, while extremely contentious claims about the historical value of the various ancient accounts of Rome's origins and early history are made as well. The title is entirely apt.

The book is divided into five large chapters, and these are followed with a series of appendices, tables, helpful maps, an extensive bibliography, and an equally extensive set of indices. The heart of the book, however, consists of chapters 3 to 5. The third compiles and assesses the literary evidence for the growth of Rome under the kings (the 'direct dossier', as Ziółkowski calls it). The fourth is concerned with the 'indirect dossier', that is, the literary evidence for vestiges and possible traces of the archaic city inside the larger city of historical times. The fifth and final chapter focuses on the archaeological evidence, existing interpretations of it, and Ziółkowski's own reassessment and reconstruction.

Ziółkowski discerns four stages in the evidence for Rome's growth. The first is the 'Romulean' stage, on the Palatine; next is the 'Romulean-Sabine' stage, on the Palatine-Velia and the Capitol-Quirinal. The fourth stage is the "Servian/Tarquian" City of Four Regions'. As for the third, which is concerned with the Aventine and Caelian hills, Ziółkowski concludes that the direct literary evidence for it is 'falsified' (257) and that it finds no support in the indirect dossier either. It turns out that 'Rome was in fact founded on the Palatine and that it had only one stage of growth between the "Romulean" birth and the "Servian" achievement' (9), and by 'in fact', Ziółkowski means

exactly that. ‘The compatibility’, moreover, ‘between the archaeological evidence and the tradition of the foundation of the City on the Palatine is full’ (258). That last claim may ring a few bells; it may also be cause for alarm.

But this is not just a book with big, bold conclusions. It is also an extremely dense and erudite work. Ziółkowski is intimately familiar with the literary evidence and the many textual problems associated with it (as the appendices on Varro, *Ling.* 5.41–56 and Tac. *Ann.* 12.24.1–2 show), and he is equally at home with the archaeological material, the problems of its interpretation, and the limitations imposed by the nature of the discovery of so much of it. He is very well read, drawing on all manner of studies, tracing various debates back to their origins in earlier centuries, while also engaging with the latest archaeological work. The book is packed full of ideas and arguments—far more than any review could ever begin to do justice to—and, while many are not persuasive, there is much that is bound to influence the course of debate in the future, and a number of old ideas ought, finally, to have been dispatched for good.¹ It is a truly impressive piece of work.

Given the era with which the book is concerned, and that it is ‘a study of the literary tradition’, the question of what the Romans of the second and first centuries BC actually knew about their city as it had existed many hundreds of years before the advent of historiography at Rome is inevitably fundamental. Ziółkowski offers a brief discussion of various approaches to this problem (18–22) and his own position becomes immediately apparent. The optimism with which T. J. Cornell handled the literary evidence in his *The Beginnings of Rome* (an optimism that was not well received by reviewers at the time) is cast as ‘moderate’. The current mood is apparently one of scepticism (a claim somewhat undermined by Ziółkowski’s assessment of Cornell’s approach, in contrast to its reception twenty-five years ago), although the scepticism of the likes of J. Poucet (a regular target, who also gets made into something of a straw man) is instead ‘hypercriticism’.² (Sceptics of the ‘unhypercritical’ kind are merely ‘radical’.)

Ziółkowski sets out what, in his view, are the main principles and aims of the ‘school’ of ‘hypercriticism’ (21). His assessment seems somewhat misrepresentative, but no less problematic is his general use of the term. In a number of places, he employs it to signal his disapproval of views he does not

¹ ‘Ought’ because Ziółkowski’s brilliant discussion of the Lupercalia is an English translation of an article he published in 2016, an article that was simply ignored by the next contribution to the debate (although that contribution appeared too late for Ziółkowski to take it into account, he nonetheless found room to dismiss it in a footnote, rather rudely, it must be said, even if not altogether unfairly).

² For reviews of Cornell’s book, see *CPh* 92 (1997) 202–7; *CR* 47 (1997) 358–61; *JRA* 9 (1996) 310–15. For a very different assessment of the current mood, see J. H. Richardson, *Kings and Consuls: Eight Essays on Roman History, Historiography and Political Thought* (Oxford, 2020) 1–11.

share. That is one thing, but the word also gets used as a means to undermine other people's arguments, or even to dismiss them outright, as most strikingly at 108 n. 124, where an argument that has considerable bearing on Ziółkowski's own is not only relegated to a footnote, but also dismissed as 'hypercriticism squared and cubed'. When other people's work is treated in such a disdainful manner,³ it may be time for the use of this sort of terminology to be abandoned altogether, as unhelpful and even unfair.

Ziółkowski cites with approval the views of A. Grandazzi, for whom '*les prudents*' (as opposed to the dogmatists, the 'fideists' on the one hand and the 'hypercritics' on the other) are 'those who try to approach every detail of the literary tradition with an open mind and consign it to the realm of history, legend, ideology or erudite speculation only after a thorough analysis supported with data and methods of every possible discipline, starting with archaeology' (21; the words are Ziółkowski's). This does not sound altogether different (to me, at least) from the approach taken by Ziółkowski's 'hypercritics'. Ziółkowski, however, does not really adhere to the approach of '*les prudents*'. There is one type of evidence in particular in which he wants to place special confidence from the start: the ancient accounts of the city itself. This is because 'Republican and Augustan authors, while referring to the Archaic City's material aspect, essentially related what they were seeing with their own eyes'. But what could they actually see, all those centuries later? Apparently quite a lot: 'the ancient sources, and their sources as well, and the material unearthed by the archaeologist, relate the same story. *Ergo*: there is no reason to prefer one dossier to another; *ergo*: the study of the literary tradition is for a modern student of the first City of Rome as indispensable as that of her material remains' (25). That certainly sounds a little dogmatic to me.

What this can turn out to mean in practice may still come as a surprise. No one, in Ziółkowski's opinion, has yet come up with an adequate explanation for the invention of the story of the rape of the Sabine women. The story is deeply unfavourable to the Romans and that, apparently, means that no Roman would have ever invented it (and no Roman would have ever passed it on either, at least, that is, if it had been invented). The explanation is simple: no one invented it (which presumably also explains why it *did* get passed on). According to Ziółkowski, the story is essentially historical (235–8). That may seem implausible enough, but what follows (238–40) is effectively a rewriting of it, and Ziółkowski's own version is quite different from any ancient account (no intermarriage and no synoecism, it would seem, but instead duality, including most notably of the *three* Romulean tribes, one of which therefore

³ Note also 24: '... hypercriticism, for which all the Roman history before the 4th century is a fable' (!), and then, in the same sentence, come the 'moderates', 'one of whose tools is common sense'.

has to get a new date, because the ancient account, for all that it is apparently historical, is inconvenient as it stands).

It hardly needs to be said that to champion the historicity of a story and also rewrite it are mutually exclusive activities. And it is hard not to suspect that the Sabine story is handled in the way that it is, simply because it can be made to fit with Ziółkowski's view of two communities settled on the Palatine and the Quirinal respectively. After all, various other pieces of evidence, those that do not fit the argument, regularly get explained away or are dismissed as mistaken, confused, or affected by some agenda.⁴ This is a necessary approach, of course, if the historicity of the evidence is to be maintained: variant traditions and incompatible accounts are inevitably something of a problem, not least because they cannot all be true.

For all that he is very critical of the methods of A. Carandini, it has to be said that Ziółkowski's own do not always seem so very different. Ziółkowski does, however, express agreement with Carandini on one crucial issue. He maintains that 'Carandini's contention that fortifications corresponding topographically (the Palatine) and chronologically (broadly mid-8th century) to the Romulean wall of our texts should be read as the sign of the foundation of Rome is perfectly legitimate in itself' (227). The main problem with Carandini's reconstruction, according to Ziółkowski, is that the walls he found at the foot of the Palatine were not defensive in nature (see 217–26 for Ziółkowski's assessment). The other problem is that Carandini's walls are in the wrong place. The relevant fortifications apparently had to have been located opposite 'the narrow saddle of the Arch of Titus' (*sic*), to defend the Palatine at its most easily approachable point, and they should also have consisted of an *agger* instead of a wall (227). This is because, Ziółkowski maintains, '*aggeres* are material signs of the birth of the Latin urban communities' (229); thus the construction of an *agger* at Rome 'is the surest sign of the birth of the City' (227).

As Ziółkowski knows, there is an 'imaginable objection' to this argument; it has already been made in response to Carandini's own. Just as a wall is only a wall, so an *agger* is only an *agger*; neither is necessarily evidence of Rome's foundation or of the birth of an urban community. Ziółkowski's response to this is to refer to the elder Pliny's list of the *populi Albenses* who were accustomed to receive meat on the Alban hill. That list, he says, proves 'that a profound institutional change did in fact take place in Latium not long before the historical era' (229). Elsewhere in the book Ziółkowski is meticulous in his handling of the literary evidence; he sets it out systematically, in full, and with an accompanying translation, before proceeding with his analysis. His approach is exemplary, a model of how to present and assess evidence clearly.

⁴ Did Livy rewrite Rome's earliest history, in order to do away with 'uncomfortable themes'? Ziółkowski thinks he did (102–4, 214–15). But why did he not also get rid of the 'uncomfortable themes' in the Sabine story?

That, however, only makes his handling of Pliny's evidence all the more conspicuous: there is no text, no translation or paraphrase, and not even a reference (*HN* 3.68–9), so that readers can at least go and check what Pliny says; there is no analysis of the passage either.⁵ Instead it is necessary to make do with a reference to the work of Grandazzi and a somewhat vague and—in the context of the book—surprisingly brief discussion, if discussion is even the right word. Some awkward matters are quietly dealt with in a footnote (naturally the Querquetulani and Velienses cannot have lived on the site of Rome), but readers are confidently informed that Pliny's list is 'probably the only quasi-documentary text in our possession reaching back to the pre-urban age'. Whatever is made of *that* claim, it is not obvious that a rather curious list of peoples participating in a shared sacrifice should somehow prove that the construction of an *agger* is indeed evidence of the birth of a specifically urban community, and not simply evidence of a community's need to defend itself. But the matter is clearly not up for debate.

As for the Palatine *agger*, Ziółkowski states that it 'must have disappeared without traces still in Antiquity' (227). What this means is that there is no evidence for it, but that is not how the case is presented. The *agger* existed (the word 'must' is used repeatedly), but just did not leave any 'traces' of its existence. The sentence, however, continues: the *agger* disappeared without trace, 'leaving only two vestiges: the memorial of the gate and the shrine situated outside of it, the Porta Mugonia and the temple of Iuppiter Stator'. But why should a gate and a temple necessarily count as 'vestiges', or even a 'memorial', of precisely an *agger*? Ziółkowski goes further: even the wall at the foot of the Palatine (a wall not built for defensive purposes) could be evidence of his *agger*. The wall is an 'indirect sign', or at least a *terminus ante quem*, however that follows. But, of course, without these vestiges, all that is left is, at best, a circular argument.

In keeping with all this, Ziółkowski maintains that '*Stadtgründung* makes better sense than *Stadtwerdung*' (215). And 'The Late Archaic Rome', he says, 'had all the necessary characteristics of *polis/civitas*—citizenship, an assembly, the duality *urbs/ager*—which an early EIA agglomeration of huts, whatever its

⁵ Another striking instance is Ziółkowski's presentation of the evidence for *Roma quadrata* (a topic of sufficient importance to warrant inclusion in the book's title). Ziółkowski says (111): '*Roma quadrata* is mentioned eleven times in eight extant ancient texts', and then proceeds to set out these eleven mentions in their eight texts in a single sentence. The sentence fills most of the page; it contains some fifteen pairs of brackets, a further two sets of dashes functioning as brackets, four footnotes, and a reference to a later part of the book (on the other hand, no page numbers are given for the passages of Dionysius, Appian, Solinus, and Tzetzes, which are quoted in other places). In complete contrast to the handling of the evidence elsewhere, this is an example of precisely how *not* to do it. Passages like this, which are fortunately few, coupled with the often cumbersome prose and the high rate of spelling and typographical errors convey an impression of a text that is not quite finished and still in need of revision.

size, could not have and which it could hardly acquire piecemeal'. So it would seem that, even when it comes to matters that no Republican or Augustan author could possibly have seen (because they were not visible), and that no archaeologist has unearthed or indeed could ever unearth (because they were not material), the literary evidence is still to be trusted. It is tempting to ask what 'piecemeal' really means, if two of the Romulean tribes were created first, but the third only added later. But it is more important to ask why these various characteristics could not have been acquired piecemeal anyway.

As for the city's subsequent growth, following its 'birth', Ziółkowski knows that what the sources have to say (his 'direct dossier') is potentially worthless, or at least that the sceptics will have doubts about it (although Ziółkowski himself wants to dismiss selected parts of it, so it is not just a matter of appeasing the sceptics). His solution is to turn to the evidence for gates, walls, and other structures inside the Servian walls (his 'indirect dossier'). The difficulty here is that none of these structures survives. The ancient sources, moreover, are not always concerned with describing what could actually still be seen in their day, or what their sources might have seen (where these things could even still be seen), and that means that it is usually necessary to rely on their judgement. No doubt the Romans could recognise gates and earthworks when they saw them, but the matter is not always as simple as that.

Ziółkowski claims that the Tigillum Sororium was a gate, but no ancient source calls it that. This is because, Ziółkowski argues, the pattern of the streets had changed, with the result that no one remembered that the Tigillum Sororium was a gate. The crucial issue, it turns out, is not what was actually there to be seen, but instead the context of it and what people remembered as a result: 'The element which probably kept most strongly the memory of the original function of the ancient gates *intra muros* were not their visual aspects or myths woven around them but streets which issued from their immediate vicinity' (163).⁶ There may well be something to be said for the argument that ancient remains might have been interpreted in light of their surroundings, and that may very well have been the case when it came to dilapidated or mysterious structures, but this argument cannot be used only when it is convenient. What about some of the other structures—or what remained of them—that *were* identified as gates? Might it have been enough that they were located on roads? And how is it possible to know that these structures had once stood in walls and, moreover, in walls built specifically for defensive purposes

⁶ This line of argumentation gets taken to extremes (173): 'in the first century B.C. the only trace of the Porta Agonensis was its name, remembered by a handful of erudites: a material *lieu de mémoire*, similar to those which kept alive the memory of the Porta Mugonia and the Porta Romanula, would not be necessary considering that the gate effectively remained in existence, even though on a new site and with a new name' (Ziółkowski is here referring to the Porta Collina).

(i.e., city walls)?⁷ Carandini's walls at the foot of the Palatine were inside the Servian city but, as Ziółkowski argues, were not built for defensive purposes.

It is necessary only to think of the 'Tomb of Romulus' in the Forum to appreciate just how unreliable ancient views can be. In the case of the 'Tomb of Romulus', there was even an inscription. Some said it was an account of Romulus' deeds but, according to others, the inscription dealt with the exploits of Hostus Hostilius.⁸ Since a part of the inscription survives, it is possible to rule out both interpretations, not only as mistaken, but as wildly so. Imaginative guesswork was—in this instance, at least—clearly the order of the day, instead of autopsy and careful assessment. It is always dangerous to assume that ancient historians employed the same methods and worked to the same standards as modern.

There is, finally, the question of chronology. Ziółkowski pieces together his various gates and walls, in order to reconstruct the boundaries of his pre-Servian city. In doing so, he assumes not only that all these structures were contemporary with one another, but also that everything dates to the era to which he wishes to assign it. That would be a stretch, even if the ancient sources were all of the same opinion. And the fact that they are not is doubly significant. It shows that the Romans were entirely unaware of this alleged stage in their city's development.

These are, it may be, merely the concerns of a sceptic, a 'radical', or, as some may judge, a 'hypercritic', with all that that supposedly means (although it should not be the case that only the doubts of a fellow optimist are to be taken seriously). In any case, these various concerns should not be allowed to detract from the significance of Ziółkowski's learned and forcefully argued book. This is a book with which everyone who works on early Rome—archaeologist, historian, 'fideist', 'prudent', 'moderate', 'radical', and 'hypercritic' alike—will need to engage, and a book in which all will find something of value. Its principal thesis may be an elaborate house of cards, but many of Ziółkowski's cards are certainly worth a closer look.

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⁷ Note Fest. 318L on the Porta Romana: *sed porta Romana instituta est a Romulo infimo cliuo Victoriae; qui locus gradibus in quadram formatus est* (Ziółkowski translates: 'Porta Romana, however, was put up by Romulus at the lower end of the Clivus Victoriae; this site is formed by steps into a square'). Ziółkowski comments that steps are 'an item which normally does not go with the notion of a gate' (152); more significant, surely, is the shape they formed, which seems inconsistent with the idea of a gate in a wall (which, if it had stairs, ought to have had only one set, or two going in the same direction); it is not enough simply to dismiss Festus' phrase as 'meaningless', as Ziółkowski does.

⁸ D.H. *AR* 2.54.2; 3.1.2.