Ch. 4 demonstrates that the 'eclecticism' of Roman sculpture, often regarded as a negative characteristic, has parallels in literary and rhetorical theory. Once again, writers like Quintilian positively advocate the use of multiple models as a desirable part of the process of *imitatio*, and they resort to artistic analogies to make their point. In practice sculptural types like that of the Venus of Capua/Victory of Brescia and its variations show that the typological heritage of Greek art could be manipulated with some subtlety and with regard to the connotations of the various iconographic models used. Yet, in the process, it is likely that the specific classical prototypes for such figures, if they were ever known or esteemed, were often forgotten.

Ch. 5 is a rather less successful, less obviously relevant, discussion of various conceptions of *phantasia* — artistic 'visualization' of non-visible subjects like gods. There is no sustained or compelling argument for linking this to ideal sculpture, though the observations made are fascinating in themselves. Finally, ch. 6 acknowledges the Roman respect for famous Greek artists of the past, but shows that, except perhaps in the religiously motivated replication of specific cult images, there is no consistent link between the most celebrated artists and the works that are believed to have been copied by the Romans. Rather, literary theory again suggests that there was a preference among artists as among writers and orators for seeking to internalize the qualities of different old masters in order to improve one's own work. There was a canon of old masters, but no expectation that they should be slavishly imitated. A comment by Quintilian is indicative, and sums up much of what P. is trying to argue. He disapproves of those painters who desire only to be able 'to copy pictures with measuring rods and plumb-lines' (10.11.6–7).

The use of literary *aemulatio* as an analogy for Roman art is perhaps not fully justified (we are not in fact dealing with Virgils and Horaces here). P. does not particularly seek to elevate the creative status of Roman artists or to make claims for their distinctive cultural identity (though she does employ the fashionable but misleading near-neologism 'romanitas' throughout). What she does illustrate powerfully is the skill of Roman artists in meeting the requirements of their customers. P. succeeds in arguing that certain works of art long assumed to be copies of some kind are better understood as Roman creations by artists well versed in the language of classical Greek art, and her claims for the basic value of literary sources in interpreting the aesthetics of emulation are satisfying.

Where, then, does this leave the study of 'real' copies? A significant aspect of P.'s approach is that she repeatedly accepts the possibility of such 'true' copies — both works that more or less accurately reflect the form of a Greek original, and works that are specifically *intended* to do so. Even while she (rightly) condemns the 'laziness or sheer obstinacy' (16) of authors and museums that continue to label ideal sculptures as copies without any question, she acknowledges that some 'exact' copies appear to exist (91). In view of this concession, it seems inappropriate to reject the claims of those like C. Hallett (13) who have proposed a revised use of *Kopienkritik*. Ultimately a fuller understanding of Roman art will require a fairer attention to all kinds of imitation, and is surely bound to retain a role for copy-criticism, even if the arguments of P. and others restrict the material to which it can reasonably be applied.

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J. W. STAMPER, *THE ARCHITECTURE OF ROMAN TEMPLES*. Cambridge: Cambridge University Press, 2005. Pp. xvi + 287, 162 illus. ISBN 0-521-81068. £50.00.

An encyclopedic monograph on Roman temples would be invaluable, but John Stamper's slim volume is no such study. It is focused on basic architectural features of a few dozen conventional temples in or near Rome, excluding the rest of the Roman Empire and all unconventional designs. The best features of the book are the fine drawings by S. and his students at Notre Dame University and a solid compendium of existing scholarship, which make the book a useful reference.

S. focuses primarily on his thesis that the Temple of Jupiter Capitolinus (hereafter TJC) was so important that its architectural design was an authoritative precedent, influencing all subsequent temple designs. It is a valid thesis, i.e., one that can be proved or disproved, with readily specifiable information. First, we must know the definitive, unique features of the TJC. These must be pioneered by this temple, not merely conforming to a conventional existing type. Second, we must find those features specifically copied in subsequent examples. Third, other sources of information — literary, archaeological, etc. — should confirm that the emulation was intentional.

The cultural importance of the TJC is not significant, since being important does not necessarily mean that the design was copied. Design features must prove that (or not).

Sadly, S. treats his thesis as fact, bending the data to prove the point, with little regard for validity or credibility. The truth of the matter, as S.'s own evidence clearly demonstrates, is that the TJC was certainly not an authoritative precedent for later temples, yet, throughout, he insists that it was. Readers should understand that healthy scepticism is appropriate.

Chs 1 and 2 consider the design of the TJC, which is unknown from archaeological evidence because only a fragmentary foundation platform remains. The Sullan version (post-83 B.C.) was described by Dionysios of Halikarnassos as retaining the original design, converted to the Corinthian order. If that is correct, then the original TJC was a large conventional Tuscan temple: prostyle, hexastyle, araeostyle, with three cellas and a three-sided peristyle (peripteral *sine postico*). Dionysios says it was nearly 200 pr (*piedi Romani*) square (59.2 m), which is similar to the extant platform (53.5 m wide), but Dionysios may be describing the whole *temenos*, with the actual building smaller. S. correctly notes that current reconstructions, with the temple itself being the full 53 m wide, result in an impractically large design, including a central intercolumniation of over 30 pr. This might have been structurally feasible for wooden lintels, but not stone. S. therefore validly presumes Dionysios was describing the whole sanctuary, with a smaller temple on a platform like the Temple of Jupiter Anxur at Terracina.

The extant foundations do not specify the size of the temple, however. The deep foundation in the Palazzo dei Conservatori undoubtedly indicates a wall or colonnade, but it is the only known deep foundation. Its location tells us little about the design because we do not know where the other deep foundations were. It is set in from the edge of the platform just where it would need to be to support the second file in a 53 m pronaos. S. takes this foundation as the outer edge of his smaller temple and presumes an answering foundation approximately the same distance in from the other side of the platform. The resulting temple is *c.* 110–125 pr wide, with *c.* 20 pr intercolumniations, just feasible in stone. That would still be the biggest (known) archaic Etruscan temple. S. chooses a width of 115 pr, based on the similarly sized pronaoi of the Temple of Mars Ultor and Hadrian's Pantheon (Agrippa's was wider). Otherwise, S.'s reconstruction matches the standard Etruscan design we have always presumed.

While this is credible, it is also clear that the TJC conformed to the existing Etruscan temple type, but did not create it. The type already existed independently from the TJC. Neither the design features nor their combination is unique to the TJC. In S.'s reconstruction of the TJC the only distinctive feature is size. Otherwise, any similar feature on a later temple is not, *a priori*, based on the TJC, but simply conforms to the same common type. Given such a well established type, the TJC can only be a design precedent if it is copied specifically.

That is easy to prove or disprove, and it never happened. Ch. 3 (early republican Etruscanate designs) unintentionally but systematically proves that no specific detail of the TJC was copied by later architects. S. claims the opposite, but makes no arguments stronger than the fact that the TJC was very important and that later temples had conventional Etruscan layouts. They do conform to the general type, but that does not prove influence from the TJC specifically. On the contrary, the TJC's specific column spacing, cella layout and proportions appear in no other example. They conform to the general type, not to the TJC itself. Chs 4 and 5 (the assimilation of Ionic and Corinthian orders) eliminate Greek orders from consideration too, proving that conventional Roman temple design was Hellenized long before Sulla added the Corinthian order to the TJC. The Hellenized Roman temple type had its own historical integrity, independent from and predating the TJC.

Chs 6–12 cover chronological phases from Pompey through Antoninus Pius, in which S. tries to force all evidence to support his thesis. His accurate descriptions, however, only prove that imperial period temple designs are completely different from the TJC, with single cellas — even a rotunda — different numbers of columns, different column spacing and proportions, different Corinthian orders, etc. S. focuses instead on pronaos width, ignoring both the obvious design differences and the fact that similar pronaos width does not demonstrate design influence anyway. The Temple of Mars Ultor, Templum Pacis, and Hadrian's Pantheon were of similar width, 34–36 m, *c.* 115–120 pr, which S. has already used to reconstruct the width of the TJC. S. then claims that similar widths prove that the TJC influenced the imperial temples (130–2 for Mars Ultor, 157 for Templum Pacis, 186 for Hadrian's Pantheon, 205 for all of them again). It is a circular argument. Furthermore, the Romans were capable of nearly millimetric precision in measuring, while these temple widths differ by some 5 pr. They do not even match each other, let alone the TJC, and if it could be demonstrated that one of them did actually match the TJC, that fact would prove that the others did not.

S.'s thesis was a good idea, *a priori*, well worth testing carefully, but the evidence is against it. Then again, if S.'s thesis were true, Roman temples would all have to conform to one design. The rich variety that we actually see is much more interesting, clearly worthy of treatment in a great monograph. That remains to be written.

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M. PAPINI, *ANTICHI VOLTI DELLA REPUBBLICA. LA RITRATTISTICA IN ITALIA CENTRALE TRA IV E II SECOLO A.C. VOL. 1. TESTO; VOL. 2. TAVOLE* (Bullettino della Commissione Archeologica Comunale di Roma Supplementi 13). Rome: L'Erma di Bretschneider, 2004. Pp. 556, 522 figs. ISBN 88-8265-282-3. €450.00.

Despite protestations to the contrary, for many the study of ancient art is still indebted to Johannes Joachim Winckelmann. Massimiliano Papini's work, the subject of this review, reveals a 'not unproblematic' legacy of this debt. *Antichi volti della repubblica* rehashes many of the arguments that have plagued the study of Roman art over the last 200 years. Emblematic of this is the anxiety of influence: to just what degree have Greek, mid-Italic, and Etruscan *models* or *originals* served to shape portraiture found at Rome and its environs? Yet in P.'s attempt to distance himself from earlier models of analysis, some readers may conjecture that the German school of '*Stilforschung*' or the purely stylistic analysis of artworks confines him. As if to offset possible criticism, P. comments that 'certain' readers may find his analysis both tedious and dubious. So is the time ripe for such a study?

Recent trends in ancient art have, thankfully, witnessed a resurgence in historiography. For instance, Natalie Boymel Kampen's seminal essay 'On Writing Histories of Roman Art' (2003) addresses the complex nature of defining Roman art. Her piece is especially important as it addresses the problematics and lacunae inherent in the discipline of Roman art history, but it also lauds new and exciting trends in the field (gender, viewer reception, performance, etc.). Her provocative conclusions push for the need for interdisciplinarity, theoretical confidence, and collaborative works. Mark Fullerton's *Ancient Art and its Historiography* (2003) shows that stylistic analyses are indeed tantamount to our understanding of ancient art in general. Methodologically, Fullerton demonstrates that there is a need to examine works within their own cultural contexts and simply for their own worth. He contends that scholars need to be aware of the modern lenses through which they filter ideas about art. In a similar vein Brunilde Sismondo Ridgway's 'The Study of Greek Sculpture in the Twenty-First Century' (2005) argues for stylistic analyses only if complemented by a thorough contextual analysis. For her, stylistic development is not to be treated in a linear fashion, but rather has to be taken in consideration with revival and reuse over specific periods of time.

Where and how, then, does P.'s work fit within the grand scheme of current trends in ancient art? First, the author is careful to establish context. He sets out to place portraiture (as seen on a variety of media such as sculpture, paintings, coins, and gemstones) from central Italy that date to between the fourth and second centuries B.C.E. within their relevant social, historical, political, and cultural frameworks. To interpret the contexts where these portraits may have appeared the author provides a comprehensive analysis of the literary texts. Within these contexts readers revisit several famous pieces that frequently appear in surveys of Republican portraiture, and they are exposed to some lesser-known works. Second, P. is quick to establish the methodological lens through which he views Republican portraiture. His art historical pedigree is especially beholden to a work that needs no further elucidation, Tonio Hölscher's, *Römische Bildsprache als semantisches System* (1987). P., through systematic stylistic analyses coupled with nuanced readings of the literary sources, reveals that portraiture is as multivalent, diverse, and complex as the cultures that produce it. To illustrate this further, what follows is a general overview of P.'s monograph.

Antichi volti della repubblica is a revised version of the author's dissertation completed at Tübigen in 2002 under the supervision of Werner Gauer and Eugenio La Rocca. The author asserts that his work is not set up in the 'traditional' catalogue format usually assigned to stylistic analyses of portraiture. To achieve this, P. divides the book into three main sections. The first and briefest section of the work focuses on the historiography of the study in question. The author leads the reader through a methodical analysis of the study of Republican portraiture from its inception in 1764 by Johannes Joachim Winckelmann up to the year 2000. There is nothing new