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## BOOK REVIEWS

DAVID TALBOT RICE, ed., *The Great Palace of the Byzantine Emperors*, Edinburgh University Press, 1958. Pp. 203; 50 pls.; color frontispiece and 3 color pls. in text, 6 folders, 45 text figs. £6.6.0.

Among the famous lost monuments of the history of art, few have presented so many difficulties to archaeologists and historians as the Great Palace of the Byzantine emperors at Constantinople. Since no identifiable traces remained above ground, attempts to reconstruct the Palace, and there have been many in the past century, have had to depend entirely on literary evidence. A great deal about the Byzantine Palace can be learned from the *Book of Ceremonies* and other mediaeval sources. The result is a veritable *embarras de richesses*. Some one hundred buildings belonging to the Palace are known by name, which the investigator must group in an area of about one-third of a square kilometer in such a way as to satisfy all the textual evidence while at the same time creating a plausible architectural ensemble. The magnitude, not to say hopelessness, of the problem may be surmised.

Expectations for definitive results brightened when, in 1935, an expedition under the auspices of the Walker Trust of the University of St. Andrews began digging in what was presumed to be the very heart of the palatine area. The excavators were fortunate in striking almost at once a peristyle with a magnificent mosaic pavement, which in the First Report (published in 1947) was identified with the Heliakon of the Pharos and dated to the reign of Theodosius II, ca. 410. But this identification proved inconclusive; and in an effort to clarify the situation as well as to assure the adequate preservation of the mosaic pavement a second campaign was undertaken in 1953 and continued until 1955 under the direction of D. Talbot Rice. The volume under review is the report of this later campaign.

The Second Report, largely the work of Talbot Rice, J. B. Ward Perkins, and David Oates, consists of the following sections: a factual account of the excavations both in the area of the peristyle and south-east of it; a masterly discussion by Ward Perkins of late Roman and Byzantine building methods that is intended to throw light on the date of the excavated ruins, but in fact goes well beyond the scope of the Report and embraces the whole Mediterranean basin (the comparative material adduced in this section accounts for 20 plates out of a total of 50); a description of minor finds (brick-stamps and pottery); a description and analysis of the mosaics; an essay on the identification of the buildings; an account of minor excavations conducted in the so-called "House of Justinian"; and an appendix containing a review of the First

Report, originally intended for *Antiquity*, by the late A. M. Schneider.

Since it would be impossible and unnecessary to summarize all the information presented in the Second Report, Mr. Mango will confine himself in the remarks that follow to the question of the date and identification of the peristyle, while Mr. Lavin will deal with the architectural forms and the mosaic pavement.

\* \* \*

It should be said at the outset that the Great Palace, far from having a regular plan, was a chaotic agglomeration of buildings that were periodically added to, remodeled, or destroyed over a span of a thousand years. To make things more difficult, our detailed information concerning the Palace belongs largely to the ninth and tenth centuries, whereas we know very little indeed of the five preceding centuries. To unravel this maze, it would have been advisable to begin the excavations at some determinable point, e.g. at the Skyla gate that opened through the southeast wing of the Hippodrome, approximately opposite the obelisk of Constantine Porphyrogenitus.<sup>1</sup> By digging somewhere in the middle of the Palace, the original excavators deprived themselves of the guidance that might have been provided by textual evidence. As a result, it is still largely a matter of conjecture which part of the Palace we have before us.

The identification of the complex naturally hinges upon its date, and in this connection the following archaeological findings are relevant:

1. The peristyle formed the forecourt of a large apsed hall of which, however, only the foundations have been preserved. The peristyle and apsed hall are apparently contemporary.

2. More or less along the east-west axis of the peristyle there ran an earlier structure, a kind of a viaduct, referred to as the "paved way," which presumably gave access to a building that had occupied the same position as the apsed hall.

3. Under the southwest corner of the peristyle was found part of an earlier structure, presumably a cistern, which was cut through by the foundation of the peristyle. This cistern yielded some brick-stamps to which I shall return shortly. On its floor were found three unfinished and rather squat capitals of the impost type. These were not *in situ*, "but would appear to have belonged to the building and to have fallen to the floor when it was pulled down" (p. 15). The use of these unfinished and, in fact, barely roughed-out capitals (pl. 4, E) in the earlier building is not explained; we note, however, the following statement: "It cannot be asserted that they belonged to the building, but they must certainly be earlier than the mosaic" (p. 17).

4. The mosaic floor was in use long enough to need substantial and careful patching. Subsequently, the

1. Cf. E. Mamboury's remarks in *Byzantion*, xiii, 1938, pp. 304-305.

mosaic was covered over with a pavement consisting of big slabs of Proconnesian marble. It may be remembered that the authors of the First Report (pp. 16-17) dated the marble pavement to ca. A.D. 550, a view that they justified by the following considerations: ". . . the architectural fragments of the second period [i.e. the period of the marble pavement] . . . were similar to some pieces of Justinian's St. Sophia (A.D. 532-7). It may be noted that a section through the north-west colonnade . . . revealed a coin of Justinian I (A.D. 527-65) at -1.30 m. which must have been deposited there after the removal of the mosaic. A stratum containing seventh- and eighth-century coins and pottery . . . gives a certain *terminus ante quem* for the removal." The Second Report, however, tells us merely this (p. 23): "We . . . cannot follow the previous excavators in dating this necessarily to the time of Justinian." Since this is a point of considerable importance, one might have wished for further elaboration. Apparently Talbot Rice does not even recognize the seventh- or eighth-century *terminus ante quem* for the marble pavement, since on p. 164 he states, "The laying of the marble floor in the Peristyle could well be associated with Basil I" (867-886).

5. Excavations beneath the apsed hall revealed no less than five successive building stages which, however, need not have been separated from one another by considerable intervals of time. Without discussing in detail these exceedingly complex substructures, we need only note that Stage I (representing an earlier building on the site of the apsed hall, a building to which the "paved way" apparently gave access) is characterized by the following system of construction: wide bands of brick-work (average brick size 38 cm. sq. by 4 cm.; 10 courses to 92 cm.) alternating with single courses of "greenstone" (a kind of decomposed granite). Stage II, which is secondary to, but may have been contemporary with Stage I, is represented by a pier built entirely of "greenstone." While our knowledge of early Byzantine building methods in Constantinople is admittedly rather meager, it is nevertheless significant that the type of construction found in Stage I, namely wide bands of brick alternating with single courses of stone, is most closely paralleled in buildings of Justinian's period. This technique is not found in any datable fourth- or fifth-century building in Constantinople, viz. the Hippodrome, the façade of the earlier St. Sophia (360 or 415 ?), the aqueduct of Valens, the Theodosian Land Walls, the Golden Gate, the cistern of Aetius, the so-called martyrion of St. Euphemia, the Myrelaion rotunda, St. John of Studius or St. Mary Chalkoprateia (this is not listed by Mr. Ward Perkins; it is built entirely of brick, as far as one can judge from remains above ground); but

2. The treatment of the brick-stamps is in general unsatisfactory. No. 7 reads *Κυριακῶν πρεσβυτέρων* not *Κυριακῶν ὁ πρεσβύτερος*. No. 8, *ΙΩΑΝΝΟΤΣΚΡΙ*, should be completed *Ἰωάννου σκριβανος* (or *σκριναριου*), not *Ἰωάννης κύριος*. Furthermore, Talbot Rice quotes my study in *AJA*, LIV, 1950, pp. 19-27 for his statement that the indictions mentioned on

it is found in Justinian's St. Sophia, St. Irene, SS. Sergius and Bacchus, as well as outside the capital, in Basilica B of Philippi (narthex), St. John at Ephesus, etc. Regarding the use of "greenstone," Ward Perkins says (p. 76): "The examples known to the writer to which an approximate date can be assigned are all Justinianic."

What conclusions do the excavators themselves draw from this evidence? Here I must say that Talbot Rice cannot be praised too highly for having put all possible alternatives squarely before the reader, instead of imposing the interpretation that he may himself favor. He even went so far as to consign to an appendix the somewhat eccentric views of A. M. Schneider, who suggests that the peristyle was not part of the Imperial Palace at all, but may have belonged to one of the princely residences built by members of the Theodosian dynasty. Since this would require a date of ca. A.D. 390, which is now definitely out of the question, we need say no more of Schneider's theory. But let us return to Talbot Rice's own views. After discussing all the pros and cons, he quite frankly informs us that he cannot make up his mind: on the one hand, he realizes that the archaeological evidence points to the sixth century; on the other hand, the stylistic analysis of the mosaic floor leads him to the fifth. As he puts it, "the mosaics and the structure which they adorned do not speak with quite the same voice" (p. 166). On p. 160 he says, "One may thus conclude that the floor was laid sometime between A.D. 450 and 550"; but he becomes even more flexible on p. 167 and admits Justin II (565-578) as one of the possible builders of the peristyle. Can we narrow down this enormous span of nearly 130 years?

I think we can. Leaving the discussion of the mosaic to Irving Lavin, I should like to draw attention to the consistency of the archaeological evidence. Particularly important in this connection is the cistern under the southwest corner of the peristyle (no. 3 above) and the unfinished impost capitals that were found in it. On the excavators' own admission, capitals of this type are not known before the sixth century. No less significant are the brick-stamps. We are told (p. 15) that about half the bricks of the cistern bore stamps of two kinds: the first (p. 106 and fig. 21. 2) reading  $\text{+}\Gamma\text{A}\text{I}\text{O}\text{Y}$  (stamps with this name have been found in sixth century contexts), the other with a cruciform monogram. The specimen of the latter illustrated in fig. 21. 3 is damaged on one side, but one is able to distinguish the letters A,  $\Gamma$ ,  $\Omega$  and N (or M ?) attached to the arms of the cross. Professor Talbot Rice interprets this stamp too as reading  $\text{+}\Gamma\text{A}\text{I}\text{O}\text{Y}$ , but this is clearly impossible.<sup>2</sup> Now the use of a cruciform monogram offers an important chronological criterion. This question has been carefully studied by E. Weigand, who

brick-stamps refer not to imperial indictions but to the count of bricks. I must protest that I never said such a thing. An indiction is always an indiction; it is a system of counting years, not bricks. What we do not know in most cases is to which 15-year cycle a particular indiction date refers.

has come to the conclusion that the cruciform monogram (as distinct from the "Doppelstabtypus") is not found before the reign of Justinian; even at that time, however, monograms of the "box" or "double-bar" type (i.e. based on a letter with two verticals, such as II, H, N or M) were still in the majority.<sup>3</sup> This accords so well with the evidence of the capitals and the occurrence of what is usually regarded as Justinianic brickwork in Stage I under the apsed hall that it is difficult to avoid drawing the natural conclusion: the peristyle with its mosaics cannot be earlier than the reign of Justinian, and is in all probability later, since we must allow an interval of time both for the destruction of the cistern and for Stages III-V under the apsed hall.<sup>4</sup>

In their discussion of the problem of identification (pp. 161-167), Talbot Rice and Ward Perkins consider the following possibilities: 1. That the peristyle was, as suggested in the First Report, the Heliakon of the Pharos. This is rejected. 2. That the peristyle was the Phiale of the Green circus faction, as proposed by A. Vogt.<sup>5</sup> This is also discounted. 3. That the peristyle was, as suggested by me on a previous occasion,<sup>6</sup> connected with the building activity of the Emperor Marcian (451-457). This is cautiously accepted, although the authors are not decided whether Marcian's buildings are represented by the peristyle or the "paved way" phase. I must regretfully admit, however, that my suggestion, tentatively put forward on the basis of the First Report, is not substantiated by the evidence presented in the Second Report. Even the "paved way" phase now appears to be too late for Marcian, although this Emperor's "Peridromes" could not have been far from the excavation area. As for the peristyle, the most likely date, if the above considerations are accepted, is towards the end of the sixth century, since the style of the mosaic as well as historical factors would appear to preclude a date after the beginning of the seventh century.

Our meager textual evidence relevant to this period does indeed indicate that the Palace was expanding southwestward, i.e. in the general area of the excavations. Justinian's contribution to the inner or residential part of the Palace is, unfortunately, unknown, except for a vague and sweeping statement by Procopius<sup>7</sup> to the effect that this Emperor renovated almost the entire

Palace. The octagonal Chrysotriklinos was, as is well known, built by Justin II (565-578), possibly over earlier buildings, and decorated by Tiberius II (578-582). The latter Emperor was responsible for a radical alteration of the Palace, as we learn from an eyewitness, John of Ephesus. Since this important evidence has been entirely neglected, I take the liberty of reproducing the relevant passage in Brooks' Latin translation, the most faithful available:

*Rex Tiberius, qui etiam Constantinus appellatus est, vivente Iustino cum in palatio magno Authentico habitaret, cum ipse Caesar esset, in uno latere palatii anguste habitabat.<sup>8</sup> Quamobrem, etiam cum Iustinus mortuus esset, Sophia uxor eius in eo palatio Authentico mansit, nec eam eicere potuit, quemadmodum nec ea eum ingredi et in eo habitare sivit. Et, postquam imperator factus est, coartabatur, et praesertim postquam uxor eius cum duabus filiis eius ad eum ingressa est, et, quoniam reginam Sophiam compellere vel cogere noluit, ut ingressus in sede regni magna habitaret, totum latus septentrionale palatii subvertere coactus est et sibi palatium aedificare. Quamobrem aedificia etiam magna et multa diruit et amplissime restauravit, horto etiam pulcro, qui in medio palatio erat, qui in regum delectationem erat, deleto, et aedificiis amplissimis in eo conditis, cum quoquo versus diruisset et dilatasset et magnifice aedificasset, quando balneum etiam amplissimum denuo aedificavit, et stabulum equorum amplissime supra in eo palatio aedificavit, cum ceteris rebus multis.<sup>9</sup>*

It is true that this text raises certain difficulties. In particular, it is not clear what is meant by the "northern side" of the Palace. Strictly speaking, that would refer to the original Constantinian Palace, that of Daphne and the guards' quarters, bordering on the Hippodrome and the Augusteon. Seeing, however, that the Daphne was still at that time the center of the Palace, it was probably there that the formidable Sophia was ensconced; furthermore, the mention of a beautiful garden *in medio palatio* seems to suggest a more private residential location, somewhat removed from the administrative and ceremonial quarters. So perhaps by *latus septentrionale* John of Ephesus means no more than the upper part of the Palace, the part away from the sea. Admittedly, the evidence does not allow us to state that Tiberius' building program (of which the embellishment of the Chrysotriklinos may have been a

3. *Byzantion*, VI, 1931, pp. 412ff.; *Jahrb. d. Deutsch. Archäol. Inst.*, LII, 1937, p. 130. Cf. O. M. Dalton, *Byzantine Art and Archaeology*, Oxford, 1911, p. 682; V. Gardthausen, *Das alte Monogram*, Leipzig, 1924, p. 120; E. Kitzinger, "Mosaics at Nikopolis," *Dumbarton Oaks Papers*, VI, 1951, p. 91.

4. The excavators' discussion of the all-important cistern is marked by so much caution and ambiguity that it verges on confusion. On p. 15 we are told that the capitals "would appear to have belonged" to the cistern, and on p. 17 that these same capitals "could be as late as the end of the sixth century and are in any event not likely to be much earlier than the end of the fifth." "The evidence of the brick stamps," the authors continue, "also supports a date in the fifth or even in the sixth century." Yet on p. 106 we are informed that the first of the two brick-stamps, the one reading FAIOT "is probably of the fourth or early fifth century." And whereas on p. xxi we had

been told that the area of the peristyle had "probably" been included in the Imperial Palace by the time of Theodosius II (408-450), we read on p. 17 concerning this same cistern: "It is known that a number of private houses and palaces stood in this area in early times . . . and it is very probable that this earlier building may have been a part of one of them."

5. *Echos d'Orient*, xxxv, 1936, pp. 436-441.

6. *Cahiers archéologiques*, V, 1951, pp. 184-186.

7. *De aedificiis*, I, 10.

8. Tiberius was created Caesar on December 7, 574, and emperor on September 26, 578.

9. *Hist. eccles.*, III, 23. *Corpus script. Christ. orient.*, *Script. Syri*, Ser. III, tom. III, *versio*, 1936, p. 111. Less accurate English translation by R. Payne Smith, *The Third Part of the Ecclesiastical History of John Bishop of Ephesus*, Oxford, 1860, pp. 203-204.

part) was in the region of the peristyle; yet such a possibility is rather tempting. The date would seem to be appropriate, and the extensive leveling of earlier buildings, as described by John of Ephesus, may account for what the excavators found. The next emperor who is known to have undertaken a major replanning of this area is Justinian II (first reign, 685-695), but he is too late to be considered.

Talbot Rice's introductory remarks strike a pessimistic note. He reminds us that the ruins which remain buried on the site of the Palace are mostly substructures, and that these do not necessarily reflect the churches, halls, audience chambers and living-rooms that were situated above them. "Of these main structures on the 'piano nobile' practically nothing remains—nor is it likely to remain in any other part of the Palace that stood on the slopes, except perhaps in so far as the actual floors are concerned" (p. xxiii). This is explained by the abandonment of the Palace several centuries before the end of the Byzantine Empire,<sup>10</sup> and by the systematic ransacking of the whole area for building materials in the early seventeenth century. Another contributing factor may have been the violent thunderstorm accompanied by the explosion of a gunpowder depot that struck this very area (called by the Turks Ishak Paşa) in July 1490.<sup>11</sup> A contemporary source, the Nuremberg Chronicle (fol. CCLVII) says, *Octingentos domos ignis rerum edax consumpsit, et hominum tria milia, ut nec lignum nec forma edificiorum rimansit*. This, incidentally, indicates that a populous quarter was situated in this area before 1490 which, in turn, suggests an earlier spoliation of existing Byzantine ruins.

The Walker Trust excavations closed in 1955, and no further archaeological exploration of the Great Palace is likely to occur in the immediate future. The hope, entertained before the Second World War, of turning this whole area into an archaeological park, like the Athenian agora or the Roman Forum, appears to have been abandoned. This is a sad conclusion to great expectations; for the fact that most clearly emerges from the Second Report is that the archaeological study of the Palace has barely begun.

CYRIL MANGO

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One of the major problems left unresolved by the first campaign was the purpose for which the peristyle

10. On p. xxii it is stated "on the evidence of the pottery" that "much of this area of the Palace was used as a rubbish dump as early as the twelfth century, and its ruin must have begun somewhere around 1150." This is not borne out by the narrative sources which make it clear that the Great Palace continued to be used for ceremonial purposes and occasionally as the imperial residence right up to the Latin conquest in 1204. It would seem to me that the evidence of the pottery—and our knowledge of Byzantine pottery is still quite rudimentary—is rather a shaky basis for such an assertion.

11. See A. M. Schneider, "Brände in Konstantinopel," *Byzantinische Zeitschrift*, XLI, 1941, p. 389, and my Letter to

was intended. The question was settled by the discovery during the second campaign of the foundations of a huge building (32 x 16.5 m.) opening off the southeast portico. It consisted of a rectangular hall preceded by a vestibule, with an apse projecting from the narrow end opposite the entrance. Since there can be little doubt that the peristyle and the apsed hall were contemporary, they must be regarded as an ensemble, the former serving as a forecourt to the latter. With good reason the authors rule out the possibility that the building was a church. The implication therefore is that it was a secular part of the palace; perhaps part of the living quarters, perhaps for ceremonial use (the two functions are not mutually exclusive).

That much at least seems certain, and yet the authors offer no comments upon the ensemble's relationship to late classical and early Byzantine domestic architecture in general; a curious omission in view of the lengthy and comprehensive analyses included of methods of construction and of the mosaic pavement. To be sure the mere combination of a peristyle with an apsed hall would seem to provide little basis for discussion, especially since it is an arrangement that occurs very frequently in imperial architecture. But by the same token it is also true that the arrangement conforms to a definite tradition of palace and villa design. And consideration of this tradition suggests several observations which, if substantiated by further research, would shed valuable light upon the building's formal ancestry.

To begin with we can be fairly sure that the type in this form was not a Hellenistic legacy, for it does not seem to occur among the numerous houses, villas, and palaces of Olynthus, Priene, Pergamon, or Delos. The Hellenistic tradition, as it was interpreted in the early empire is exemplified by the well-known villa at Oudna in Tunisia.<sup>1</sup> The structure is planned centrally, with the rooms distributed more or less at random about a large courtyard; no particular emphasis in terms of layout is placed upon the larger and presumably more important rooms. In the villas at Pompeii, on the other hand, a definite axis is normally established by placing the tablinum or *oecus* at the center of one side of the atrium or peristyle, opposite the vestibule.<sup>2</sup> And at the end of the first century in an unusually luxurious example like the Palace of Domitian on the Palatine, a ceremonial triclinium has been developed architecturally to a position of dominance in the plan.<sup>3</sup> Ultimately, that is to say by the third century, this tendency to place special emphasis upon a major room (to which for convenience we shall refer hereafter as the triclinium)

the Editor, *ART BULLETIN*, XLI, 1959, p. 354.

1. P. Gauckler, "Le domaine des Laberii à Uthina," *Monuments Piot*, III, 1896, pp. 177ff., where the initial construction of the building (there were later alterations) is dated to the end of the first or the beginning of the second century A.D. Cf. K. M. Swoboda, *Römische und romanische Paläste*, Vienna, 1924, fig. 10, p. 21.

2. See the examples *ibid.*, fig. 4, p. 11 (House of the Faun), fig. 6, p. 14 (villa at Herculaneum), fig. 9, p. 20 (Villa of Fannius Sinistor).

3. *Ibid.*, fig. 3, p. 9.

became a characteristic feature of Roman villa architecture. A villa at Portus Magnus in Algeria from the second half of the third century typifies this phenomenon;<sup>4</sup> the triclinium has been completely isolated and is the goal of a long axial progression from the entrance through the peristyle and gardens.

Paralleling this development is a tendency to enlarge and elaborate the main triclinium itself, with or without the central peristyle. Among these elaborated triclinia, which include trefoils<sup>5</sup> and basilical plans,<sup>6</sup> the rectangular hall with a single apse is by far the most common. Examples proliferate through the third and early fourth centuries, by which time it seems that hardly a residence of any pretension was complete without a portico, open or closed as a peristyle, preceding an apsed triclinium.<sup>7</sup> Perhaps the most telling cases are those, especially in Gaul, in which apsidal arrangements were added (usually in the third and fourth centuries) to pre-existent buildings.<sup>8</sup>

Now the remarkable fact about this whole development is that it seems confined almost exclusively to the western part of the empire. Whereas the western examples of apsed triclinia through the fourth century are far too numerous to list here, I have encountered only one instance in the East during the same period, in the House of the Buffet Supper at Antioch.<sup>9</sup> The latter is particularly interesting since it is the only example in the entire series of villas excavated at Antioch, a series that extends from the first through the early sixth century A.D.<sup>10</sup> My information is certainly incomplete, but it would be strange indeed if such a radical dichotomy between East and West were the result of mere oversight or the accidents of preservation. Most of the western examples, to be sure, are of provincial origin, but at

least two of them may have formed part of royal residences—the newly excavated villa at Piazza Armerina in Sicily, which has been associated with Maximianus Herculeus, and the so-called Palace of Theodoric at Ravenna.<sup>11</sup>

But of special importance for the Great Palace building is the fact that when we move from the fourth to the fifth and the early sixth centuries, the picture changes completely. Dating from this period are a number of examples in the eastern empire, at Stobi,<sup>12</sup> Apamea,<sup>13</sup> and at Rhegium near Constantinople itself.<sup>14</sup> Thus, the evidence seems to suggest that whatever the immediate source, the combination in the Great Palace of open court with apsed hall reflects a western tradition that had been transplanted to the East, perhaps late in the fourth or early in the fifth century.

The full implications of this conclusion can be grasped only in relation to the mosaic pavement of the Great Palace, which decorated certainly three and probably all four porticoes of the peristyle. In the second campaign an additional section of the mosaic in the northeast portico was uncovered. From the moment of its discovery it was obvious that the mosaic constituted a major addition to the corpus of early mediaeval monuments. This is true not only because it decorated what was unquestionably a significant part of the imperial palace, but because it contains many peculiarities, indeed anomalies, of iconography and style, the explanation of which, one feels, would provide new insight into the genesis of Byzantine art.

Represented is a medley of subjects that fall generally into three categories. By far the most numerous are subjects of a rustic, genre nature, including, besides isolated animals, such charming vignettes as peasants

4. *Ibid.*, fig. 11, p. 23, for which a very probable *terminus ante quem* at the end of the third century can be established.

5. Of many examples, cf. most recently the villa excavated at Piazza Armerina in Sicily (G. V. Gentili, *La villa imperiale di Piazza Armerina*, Rome, 1954); here, significantly, the axiality is combined with a more organic method of planning that had been developing during the second and third centuries (cf. J. B. Ward Perkins, "The Italian Element in Late Roman and Early Medieval Architecture," *Proceedings of the British Academy*, 33, 1947, pp. 163ff.; *idem* and J. M. C. Toynbee, "The Hunting Baths at Lepcis Magna," *Archaeologia*, 93, 1949, pp. 165ff., esp. p. 192, cf. N. Neuerburg, "Some Considerations on the Architecture of the Imperial Villa at Piazza Armerina," *Marsyas*, 8, 1959, pp. 22ff.).

6. Cf. a villa, probably of the fourth century, at Cherchel in Algeria (V. Waille, "Nouveau rapport sur les fouilles de Cherchel," *Revue Africaine*, 48, 1904, pp. 56ff., pl. VIII).

7. For just a few cf. Swoboda, *op.cit.*, fig. 12, p. 23 (Westenhofen), fig. 14, p. 25 (Balácsa), fig. 26, p. 48 (Tetingen). Apses were already present, for example, in the *Domus Aurea* and in the triclinium of Domitian's palace on the Palatine; cf. also a room with a hemicycle opening off the peristyle of the Casa dei Capitelli at Pompeii (VII, iv, 31/51).

8. *Inter alia*, the villas at Balácsa (J. Wollanka, "Ein römisches Mosaik aus Balácsa," *Jahresh. d. österr. arch. Inst.*, xxv, 1929, pp. 1ff.); at Parndorf (*Fasti Arch.*, 6, 1951, no. 6046, fig. 181, p. 468); at Cadeilhan-Saint-Clar (M. Larrieu, etc., "La villa gallo-romaine de la Tasque à Cadeilhan-Saint-Clar," *Gallia*, xi, 1953, pp. 41ff.); at Lullingstone (G. W. Meates, *Lullingstone Roman Villa*, London, 1955); also at Ostia (G. Becatti, "Case ostiensi del tardo impero," *Bollettino d'Arte*, 33, 1948, p. 122, fig. 23).

9. D. Levi, *Antioch Mosaic Pavements*, Princeton, 1947, pp. 127ff.; third century.

10. It was, moreover, subsequently removed when the room was transformed into a rectangle.

11. The writer does not have the impression, as does Prof. Talbot Rice (p. 157), that a later date for the villa at Piazza Armerina has been "fairly generally" accepted. On the building at Ravenna, cf. G. Ghirardini, "Gli scavi del Palazzo di Teodorico a Ravenna," *R. Accademia dei Lincei, Monumenti Antichi*, xxiv, 1918, cols. 737ff.

12. E. Kitzinger, "A Survey of the Early Christian Town of Stobi," *Dumbarton Oaks Papers*, 3, 1946, pp. 118ff., where the palace (fig. 162) is dated ca. 400. It is perhaps significant that in the Stobi palace, a relatively early member of the eastern group, the apsed triclinium is introduced in a plan that preserves the Hellenistic tradition of rooms arranged loosely around a peristyle and contained within a square or rectangular outline.

13. F. Mayence in *Bulletin des Musées royaux d'Art et d'Histoire* (Brussels), Ser. 3, 8, 1936, pp. 10ff. To my knowledge a plan of the building has not been published; the reviewer was generously provided with one by Mlle. V. Verhoogen of the Brussels Museum. The building consisted essentially of a central court with two apsed halls projecting from the side opposite the entrance. An inscription in the mosaic pavement of the larger room (see below) records a restoration in A.D. 539.

14. A. M. Mansel, "Les fouilles de Rhegion près d'Istanbul," *Actes du VI<sup>e</sup> congrès int. d'études byzantines* (Paris, 1948), II, Paris, 1951, fig. 1, p. 256; 5th-6th century, with various reconstructions.

hoeing, a fisherman, children playing, a man falling off a donkey, etc.; a second category includes scenes from the amphitheater, including animal hunts almost certainly based on the *venationes* conducted as public spectacles; a third relatively minor category includes mythological and fantasy subjects such as a Bellerophon killing the Chimaera (fragmentary), elements of a Bacchic procession, and a griffin devouring a lizard. Try as one may, one can discover no coherent theme or system that would suggest an iconographic program in the ordinary sense. But despite or perhaps just because of the variety, the ensemble does convey a definite mood, which G. Brett aptly defined as a kind of poetic romanticism.<sup>15</sup> Talbot Rice concurs, and suggests that this very quality of sophisticated idealization marks the work as peculiarly Constantinopolitan. As such it offers important early evidence for the character of the great capital's art.

Talbot Rice's own analysis of the mosaic confirms this view. He is at pains to cite parallels for individual motives as well as general stylistic features from almost every monument of the early mediaeval period that contains comparable elements. Almost as systematically he rejects these parallels as insufficiently close to provide a direct source for the palace mosaic, which thus remains, in style as well as iconography, essentially unique. Accordingly, he retains a great deal of latitude in his conclusion as to the date, A.D. 450-550. All this, however, while unexceptionable in itself, leaves the art historian more than a little dissatisfied. Precisely because the pavement is ultimately unique we must forego the discovery of an immediate source (as after all is often the case with really significant monuments). If we can bring ourselves to do so we may perhaps reach some conclusions that are of value.

The peculiar character of the mosaic's style, and the basic reason why dates for it have varied so widely (from the third through the sixth century), lies in an inherent contradiction. The figures on the whole show a full plasticity, a richness of coloration, and an understanding of organic structural relationships that are worthy of the best Hellenistic tradition. They are placed, however, in a totally abstract space, absolutely without depth and atmosphere—a space in other words that is profoundly mediaeval. For the most part the figures have no ground to stand on, and they are ar-

ranged in horizontal and (by groups) vertical rows that establish a rhythmic pattern on the surface—both again essentially mediaeval features. While admittedly the palace mosaic has no exact counterpart among existing monuments, a similar if less striking dichotomy occurs in a coherent group of mosaics at the eastern end of the Mediterranean. The hunting and animal mosaics at Antioch,<sup>16</sup> Apamea,<sup>17</sup> and Beirut<sup>18</sup> also display figures that seem, as it were, too classical for the space in which they are placed. The remarkable thing about the group is that its members all date roughly from around 450 to around 550 A.D. When we consider that the style has no real precedent at Antioch, where a virtually unbroken series from the first century on is preserved, it becomes plain that something of major significance took place. This conclusion on the evidence of style is reinforced by the relative homogeneity of the group in the matter of iconography. All of them involve genre, particularly circus and hunting motives: this again, to judge from the examples at Antioch and elsewhere, is practically unheralded in this region where mythology and allegory had been the rule for figural compositions.<sup>19</sup>

With regard to the classical figure style, it might be argued that this is exactly what one would expect in that part of the empire where the Hellenistic tradition was most persistent. But for the abstract space and genre subject matter one must perforce look to some outside influence.

Brett in his study of the palace floor in the first report perceived, rightly in this reviewer's estimation, that for scenes of this kind we must look to the West, to Gaul and especially to North Africa.<sup>20</sup> There, literally dozens of examples are preserved in pavements ranging in date from around the second to the early fifth century. Since few of the examples can be dated on external grounds, the exact chronology is a difficult matter (though the Vandal conquest of North Africa in 429-430 A.D. establishes a *terminus ante quem* that is highly suggestive in view of the date of the eastern group, i.e. 450 and after). Nevertheless, they do show a very definite development, a development wherein the atmospheric space of the Hellenistic legacy is transformed into a depthless medium through which figures can be distributed more or less *ad usum*. It is in fact a hunting mosaic from Constantine in Algeria, probably of the

15. "The Mosaic of the Great Palace in Constantinople," *Journal of the Warburg and Courtauld Institutes*, v, 1942, p. 37.

16. Megalopsychia Hunt (Levi, *op.cit.*, pp. 326ff.); House of Ktisis (*ibid.*, pp. 357f.); Dumbarton Oaks Hunt (*ibid.*, pp. 358f.); Honolulu Hunt (*ibid.*, p. 365); also the floor in the Martyrium of Seleucia (*ibid.*, pp. 359ff.). The writer is of the opinion that Levi's dating for the more developed members of the group at Antioch is rather too late.

17. Mayence, *loc.cit.*, for the mosaic in the larger apsed hall of the building referred to above, which is dated 539 A.D. by an inscription. The animal pavement of the long corridor at Apamea, dated 469, should also be considered in this connection (*ibid.*, 5, 1933, pp. 5f.; 7, 1935, pp. 2ff.; 12, 1940, p. 5 [H. Lacoste]).

18. M. Chéhab, "Découverte d'une mosaïque byzantine," *Beaux-Arts*, no. 42, Oct. 20, 1933, 6; "Mosaïques de Beyrouth

et de Baalbeck," *Actes du VI<sup>e</sup> congrès int. d'Et. byz.* (Paris, 1948), II, Paris, 1951, pp. 89ff.; a view of the mosaic as a whole has not been published; a photograph is on file in the study collections of Dumbarton Oaks.

19. Talbot Rice curiously retains the old dating of the mosaic from Beit Jibrin (A.D. 200-250); it has been shown to be much too early by G. M. A. Hanfmann, "The Seasons in John of Gaza's *Tabula Mundi*," *Latomus*, 3, 1939, pp. 116f. (a reference kindly brought to my attention by Prof. Kitzinger) and at length by Levi, *op.cit.*, pp. 7 n. 35, 464 n. 260, 577 n. 89.

20. The standard, though by now largely incomplete, corpus in the *Inventaire des mosaïques de la Gaule et de l'Afrique*, Paris (Académie des inscriptions et belles-lettres), 1909ff.; for Gaul it is currently being replaced by H. Stern, *Recueil général des mosaïques de la Gaule (Gallia, Suppl. x)*, I, 1, Paris, 1957.

late fourth or early fifth century that offers the closest parallel of all to the Palace floor from the point of view of design: figures widely separated (decidedly not "crowded" as Professor Talbot Rice maintains North African pavements "invariably" are, p. 145) and aligned both vertically and horizontally in a sort of grid.<sup>21</sup>

The problem of how what appears to have been a local western achievement could have reached the cosmopolitan centers of the eastern empire is far too complex to be discussed here. Be it said merely that a careful scrutiny of the evidence reveals several possible channels of transmission. Another question arises from the very compelling evidence given above by Cyril Mango for a middle or even late sixth-century date for the peristyle and apsed hall. As pointed out above, one suspects that the main obstacle in the way of such a late date has been the markedly classical figure style in the mosaic. But after all a classical figure style is one of the most frequently recurrent phenomena in Byzantine art. And since our direct knowledge of painting in the capital after Justinian is exactly nil, there is no basis for an assumption that what we find in the palace mosaic is impossible for that period. Furthermore, as Talbot Rice himself observes (p. 154), the spatial organization of the palace mosaic seems even more "advanced" than that of the Apamea hunt, which is the latest of the Syrian group (A.D. 539). Consequently, this reviewer cannot agree that the mosaic speaks with another voice than the architecture, which points unequivocally to a date well along in the sixth century. Yet, by accepting such a late date we should be faced with still another dilemma—are we to assume that the Great Palace mosaic was influenced from such relatively secondary quarters as Antioch or Apamea, where its essential characteristics apparently occur earlier? Or do the Syrian examples reflect developments in Constantinople of which it happens that we have no record prior to the Great Palace floor?

For the present one can only say that the over-all picture is remarkably like that which emerged from consideration of the architectural form of the palace building. If the picture is a true one then the discoveries at the Great Palace, now available in definitive form, may necessitate several important changes in our views of the evolution from late antique to early mediaeval art.

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MARIE MAUQUOY-HENDRICKX, *L'Iconographie d'Antoine Van Dyck, Catalogue Raisonné*, Brussels, 1956. (Mémoires, Académie royale de Belgique, Classe des Beaux-Arts, IX.) 800 Belgian francs.

Mrs. Mauquoy-Hendrickx belongs to that group of dedicated scholars who spend their lives accumulating minute, precise data that in the end will benefit a relatively limited number of people. In this case it is col-

lectors of (and dealers in) the so-called *Iconographie* of Van Dyck who will be grateful for her labors; it is for them, as Mrs. Mauquoy-Hendrickx states several times in her text, that the book has been written. In her *catalogue raisonné* she has provided these collectors with a fantastically detailed apparatus that gives exact information about the different states (on the average about seven), a fair idea of the relative rareness of each impression, and a summary of watermarks found on about one-third of the prints studied by the author. The plate section of the book is accompanied by hundreds of scale-sketches of watermarks.

In accumulating these data, Mrs. Mauquoy-Hendrickx was gratefully aware of the contributions of a few equally qualified researchers before her, above all the venerable Wibiral (1877) whose catalogue had been the standard work until now, and whose system of numbering she wisely retained. There is a touching footnote to her observation (p. 47) that Van Dyck and his publisher, Martinus van den Enden, had probably never contemplated providing the series with a title-page. She wonders whether the best proof for the non-existence of this sheet might not be found in the fact that the several students in the field have never discovered one, despite a search which—laid "end to end"—would add up to almost one hundred years. (Mrs. Mauquoy-Hendrickx herself gave twenty years of her life to her work.) Whatever one may think of the relevance of the problem, one cannot help but be awed by the vast labor of love that has gone into these studies.

The *Iconographie* is one of those collections of portraits that since the sixteenth century became more and more frequent. Their history still remains to be written. Of all these series, Van Dyck's is justly the most famous. It is a curious medley, containing not only prints of men long dead but also two engravings that are no portraits at all, a Mocking of Christ and the strange piece called Titian and his Mistress. Some of the prototypes were works by artists other than Van Dyck. A number of portraits were etched by Van Dyck himself, but the majority was engraved, by a large group of engravers (Pontius and Vorsterman contributing more than anyone else) from drawings, oil-sketches, and paintings by Van Dyck.

Mrs. Mauquoy-Hendrickx did not concern herself with the relationship of the prints to these models. Some scholars have studied this aspect of the *Iconographie* (the most elaborate study, on a single print, being E. Göpel's "Ein Bildnisauftrag für Van Dyck," Frankfurt, 1940), but a vast amount of work still waits to be done. Another study, never attempted as far as I know, might scrutinize the selection of models in order to see what principles, if any, determined their choice. In some cases precise dates may be derived from a careful consideration of biographical circumstances. For example, state VII of no. 49 must date from after February 15, 1644, since Gevartius was only then appointed imperial historiographer. What we learn from Mrs. Mauquoy-Hendrickx's text preceding the critical cata-

21. *Bulletin archéologique*, 1928-29, pp. 96ff., pl. 1.