

ST. PETER IN GALLICANTU AND THE HOUSE OF CAIPHAS

BY

P. EDM. POWER, S. J.

In a recent article in this periodical¹ Dr. A. M. Schneider combats the view, which definitely locates the primitive traditional site of the house of Caiphas at St. Peter in Gallicantu and which claims moreover that the authenticity of this fourth-century tradition is strongly supported by various archaeological discoveries. He calls me the protagonist of this view, lists in his bibliography three of the four articles which I wrote on the subject in *Biblica* in the years 1928—1929 and gives several textual citations from two of them. His readers will naturally conclude that he is well acquainted with these articles, has taken directly from them the views and arguments which he combats and has represented these views and arguments with substantial accuracy. Such unfortunately is not the case. Dr. Schneider makes no mention of the arguments from tradition, on which my whole theory is based. He ignores or misrepresents my archaeological arguments for the authenticity of the site. He even attributes to me views and arguments which are not mine and bases explicitly on these a very unfavourable judgment of my scientific method and equipment. I do not for a moment suppose that he has intentionally misrepresented me. He seems to have got most of his scanty and inaccurate information, not from my own writings but from a secondary source. But I am none the less obliged, in the interests of justice and science, to present some observations on his article to the readers of

¹ *St. Peter in Gallicantu (Das Gefängnis Christi im Palast des Kaiphas): Or. Chr.* 27 (1930) 175—190.

Oriens Christianus and I am deeply grateful to its courteous and distinguished editor for giving me this opportunity of doing so.

It will be well to begin by removing some very regrettable misunderstandings. Dr. Schneider excludes me from the class of "Gelehrten, die ernst genommen werden wollen" and attributes to me the "Phantasie eines Küsters" and a "nicht zu entschuldigende Unkenntnis des Talmuds."¹ His alleged reasons are that I misinterpreted a *nefeš* on one of the pillars in the underground galleries as a Christian caricature of Caiphas and that I deduced from a Talmud passage, which I had not read, an installation for the infliction of the Jewish punishment of scourging in these same galleries. He refers his readers for proof of the first accusation to *Bibl. X*, S. 278, but cannot have read that page himself, since the hypothesis of a caricature of Caiphas is there rejected and the *nefeš* interpretation is confirmed by previously unpublished evidence: "I only give this (the Caiphas) interpretation of the sculpture as one naturally suggested by the traditional identification of the site . . . A new piece of evidence . . . only recently communicated to me through the kindness of Fr. Marchet, confirms the *nefeš* hypothesis. A tomb discovered in the neighbourhood of the galleries at the beginning of the excavations on the site contained a similar sculpture." Furthermore the *nefeš* interpretation is reaffirmed in two later passages also unread by Dr. Schneider: "It is now fairly certain, after the discovery of the *nefeš* discussed in the preceding article, that these galleries like the adjoining pit were originally a place of burial" (*Bibl. X*, p. 396). "It does not therefore justify us in supposing that there was a *nefeš* represented on this pillar (the scourging pillar) as there was on the one adjoining it" (*ib.*, p. 401).

¹ *Or. Chr. l. c.*, p. 187—188. Talmud is here to be interpreted Mishna. "Daß dieses Urteil nicht übertrieben ist, bezeugt die Deutung einer rohen Zeichnung, die man nachträglich noch an einem Pfeiler der 'Geißelungsgrotte' entdeckte. Vincent hält sie *RB.* 1925, S. 586 mit guten Gründen für ein *nephes*, vielleicht auch eine *defixio*. Power macht daraus ein karikiertes Porträt des Kaiphas (*Bibl. X*, S. 278) das Christen des 4. Jh. in dessen 'Palast' angebracht hätten!" (p. 187, n. 2). "Nur ist es keine 'installation for the infliction of the Jewish punishment of scourging' (*Biblica X*, S. 404). Marchet zieht zum Erweis den Traktat *Makkot* 3, 12ff. bei, und Power folgt ihm darin . . . Leider hat aber weder Marchet noch Power den berufenen Talmudtraktat gelesen, denn dieser besagt etwas ganz anderes" (p. 187).

No proof is offered of the second accusation, either by Dr. Schneider or by the writer from whom he appears to have borrowed it without acknowledgment, but both cite the passage of the Mishna and Dr. Schneider prints in separate type the part which is considered fatal to my imaginary deduction and which I am supposed not to have read: "Und nicht schlug er ihn, indem er (der Gegeißelte) stand oder saß, sondern indem er niedergebeugt war." Here again it is clear that Dr. Schneider has not read one of my two brief references to the Mishna text, which shows that I had read this part of it and explains why I deduced nothing from it: "According to the Mishna (*Makkoth*, III, 12) which refers to a later date, the victim had his hands attached to a pillar or post and *his body bent to receive the blows*" (*Bibl.* IX, 175). The other reference merely enumerates among the objections made to Fr. Germer-Durand's prison hypothesis: "Moreover the absence of a pillar at the supposed place of flagellation is against Jewish tradition, according to which the victim was tied to a pillar (*Makkoth*, III, 12)" (*Bibl.* X, 397). The accusations of Dr. Schneider are thus unfounded and only show that he has not read my articles.

I.

In the first part of his study, "Der archäologische Befund," Dr. Schneider rejects the conclusion of previous investigators in general, and of such well-known archaeologists as Fr. Vincent¹ and Fr. Germer-Durand² in particular, that the primitive church erected at St. Peter in Gallicantu belongs to the second half of the fifth century. The excavations on the site revealed a church at two levels, which had been manifestly reconstructed at various periods. The lower level is that of the orifice of the sacred grotto. The upper level, 3,50 metres higher, is attested by a fifth-century coloured mosaic, discovered *in situ* at the western extremity of the central quadrilateral, and by a massive substructure, 8 or 9 metres in width, which bounds the quadri-

¹ *Jérusalem*, II (1922) 514.

² *Rev. Bibl.* 23 (1914) 228.

lateral on the east. The sacred character of the cistern-like pit in the centre of the quadrilateral is indicated by fourteen crosses and seems to offer the natural explanation of the erection of a church on so difficult a site. The fifth-century date of the primitive church is mainly based on three discoveries: 1. two Corinthian capitals, exactly similar to those found in the fifth-century church of St. Stephen; 2. the coloured mosaic already mentioned, which clearly belongs to the same family as the mosaics found in the same church of St. Stephen; 3. two separate collections of copper coins, embedded in the masonry of the eastern substructure and determining exactly two periods at which it was utilised for the support of an edifice: the second half of the fifth century and the first half of the seventh century. How does Dr. Schneider refute the conclusion which these discoveries so naturally suggest?

The first objection to a fifth-century church is based on the capitals. Dr. Schneider assumes, without any attempt at proof, that a third capital of smaller dimensions and inferior technique belongs to the same date as the two symmetrical capitals, which alone are assigned to the fifth century by Fr. Vincent and Fr. Germer-Durand. He then argues that these *three* fifth-century capitals, being different in size and technique, were not made for the same church and must be regarded as "Spolienstücke."¹ He has not seen or measured the second capital, which is incomplete but, as far as can be judged, of the same dimensions and obviously, as appears from the photographic reproductions, of the same technique as the first. His objection is thus entirely based on the supposed fifth-century date of the third capital

¹ "Die dem späten fünften Jahrhundert angehörigen drei Antenkapitelle (*RB.* 1914, Tafel 5) haben nämlich verschiedene Höhe und sind in verschiedener Technik gearbeitet, können also kaum für ein und denselben Bau angefertigt sein. Man wird nicht fehlgehen, wenn man sie als Spolienstücke ansieht" (*Or. Chr. l. c.*, p. 178—179). In a note Dr. Schneider accuses Fr. Marchet and myself of not having accurately measured the fifth-century capitals. Yet he apparently read in *Biblica* my communication from Fr. Marchet: "The dimensions of these capitals, one of which is incomplete but evidently symmetrical with the other, are 0.50 cm. by 0.40 cm., height 0.55 cm.," and discovered by experiment that the measurements given are exact. He neglects to inform his readers that the second fifth-century Corinthian capital is incomplete and only says of it: "Das zweite Exemplar, Tafel V, 2 habe ich nicht zu Gesicht bekommen."

(*Rev. Bibl.*, 1914, pl. V. 4). After assigning two capitals (*ib.* 1—2) to the fifth century Fr. Vincent adds: "D'autres débris de chapiteaux à acanthes plus sèches, aux lobes géométrisés, relèvent d'un art plus tardif."¹ The reader has only to compare Dr. Schneider's third fifth-century capital with the two others if he desires an ocular demonstration of these technical indications of its later date. At the same time the measurement of this third capital is a useful contribution to the problem, since its lesser dimensions, as compared with these of the earlier capitals, support the conclusion that the primitive church of the fifth century was larger than the restored church of the seventh century. Nor will it be argued that the larger fifth-century capitals were too small for a moderately large church, 28 or 30 metres in length, if it be remembered that no prudent architect would overload with ponderous ornamentation the upper storey of a two-storey church erected on a very difficult site.²

Dr. Schneider bases a second objection on the study of the mosaics discovered on the site of the primitive church. Here he again assumes, without any attempt at proof, that Fr. Vincent and Fr. Germer-Durand have erred in assigning the coloured mosaic, discovered *in situ* at the level of the upper church, to the fifth century. It is surprising that he accepts their dating of the two capitals and rejects their dating of the mosaic, since both conclusions are based on the same motive: the similar discoveries made at the fifth-century church of St. Stephen. In any case his erroneous dating of the third capital scarcely authorises us to prefer his unmotivated pronouncement in the present case to the reasoned judgment of two experienced archaeologists.

He also argues that this mosaic cannot have originally belonged to a church, since in that case the vestibule of the church in coloured mosaic would be more elegantly adorned

¹ *Rev. Bibl.* 39 (1930) 236. Cf. also *Jérusalem* II, (1922) 507.

² My error in not observing that the capitals were "Antenkapitelle", which I here retract, does not affect the size of the church. There is no question of a "Riesensbau" (*Or. Chr. l. c.*, p. 181, n. 1) and the "basilica grandis" of the *Breviarius* implies a basilica of moderate size, not a "riesige Kirche" (*l. c.*, p. 186) which is expressed by "basilica magna nimis".

than the interior of the church in white mosaic.¹ But he deprives this argument of all force by assuming that the white mosaics were later in date than the coloured mosaics and were intended to adorn the seventh-century church. It follows that the coloured mosaics may well have been intended for an earlier fifth-century church, which had larger capitals and was more elegantly decorated than the hastily built church of the seventh century. Moreover Dr. Schneider fails to distinguish between the decoration of the crypt at the lower level and of the church proper at the higher level. Our sole means of determining the decoration of the primitive church at the higher level is the fifth-century mosaic discovered *in situ*. It is, according to Fr. Vincent, "le seul fragment de mosaïque en place au niveau de l'église supérieure" and "permet d'apprécier le caractère de la décoration en mosaïque polychrome à ce niveau supérieur"². The crypt of the primitive church may well have been decorated in white mosaic and it is at least probable, according to Fr. Vincent, that one piece of white mosaic, discovered *in situ* in the lower church, belongs to the primitive period by reason of the inscription which it bears.³ Thus Dr. Schneider has not adduced any valid reason for assigning the coloured mosaics to "Memorialbauten."

In discussing the massive substructure to the east of the central quadrilateral Dr. Schneider ignores archaeological evidence of the highest importance. He does not mention that this substructure erected on sloping ground reaches the same level as the fifth-century mosaic discovered *in situ* to the west

¹ "Weitere Mosaikreste sind über dem Viereckbau gefunden worden (Plan I, Nr. 1—10): 1—5 waren farbig, sind aber m. E. zu hoch datiert; Nr. 2 und 3 sind keinesfalls früher als 6. Jh. Die anderen Reste waren nur in weiß gehalten. Dieser Befund beweist, daß 6—10 später sein müssen als 1—3, mit anderen Worten: 2 und 3 gehörten Memorialbauten an, die bei Errichtung der Viereckkirche in diese einbezogen wurden. Es wäre sonst die ganz unerhörte Tatsache zu verzeichnen, daß man Vorräume der Kirche mit buntem Mosaik schmückte, den Hauptraum dagegen in einfachem Weiß hielt. Wir kommen auch so wieder dazu, den eigentlichen Kirchenbau bedeutend später anzusetzen, als es bisher geschah" (*l. c.*, p. 180—181).

² *Jérusalem*, II, 507. 511; *Rev. Bibl.* 39 (1930) 235—236.

³ It commemorates an unknown benefactress, Maria. See *Jérusalem*, II, 511—512.

of the central quadrilateral.¹ This fact suggests an obvious connection between these foundations in the east and the upper church adorned by the mosaic in the west. Neither does he mention the still more important discovery of two collections of copper coins, embedded in two different pieces of masonry which formed part of the foundations and were broken up by the picks of the excavators.² One collection, located to the south-east of the central quadrilateral, contained nine copper coins bearing the effigy of Theodosius II (408—450), Marcianus (450—457) and Leo I (457—474). The second collection, a little to the north of the first, contained five copper coins representing the emperors Phocas (602—610) and Heraclius I (610—641). These obvious indications of date, deposited in the substructure by the founders and the restorers of the edifice erected thereon, correspond exactly with those otherwise known: the second half of the fifth century, attested by the two larger capitals and the elegant coloured mosaic, and the first half of the seventh century, when the churches destroyed by the Persians in 614 were restored. Only a church on this site, by reason of its superior importance, would have these evidences of date deposited in its eastern foundations and only a church on this site, by reason of its necessary orientation, can adequately explain the erection of this extraordinary substructure on sloping ground. Dr. Schneider seems to be acquainted with these discoveries, as he refers to “die Münzfunde aus den Ostfundamenten die bis zu Phokas und Heraclius reichen.”³ Why then has he not drawn from them the obvious conclusion, confirmed by an independent discovery of his own, that the foundations were built in the fifth century and restored in the seventh century? He tells us that the fragments of pottery which he collected from the

¹ *Jérusalem*, II, 505—506 “Ces lignes amorcées à l’ouest contre une escarpe rocheuse (where the mosaic was discovered) haute de 3m, 50, viennent s’appuyer, environ 20 mètres à l’est, sur un blocage très massif . . . Qu’il y ait eu à cette hauteur un niveau étendu à l’ensemble de la ruine, et d’ailleurs plusieurs fois remanié, c’est ce que prouvent les dallages, mosaïques, degrés et seuils de porte.”

² Marchet, *Le véritable emplacement du palais de Caïphe*, Paris 1927, p. 69—71. My account of this discovery is based on the unpublished “Journal des Fouilles.”

³ l. c., p. 180, n. 2. Reference is given to Marchet, p. 71 where the second collection is chronicled, but not to p. 69—70 where the earlier group is mentioned.

foundations belonged partly to the early Byzantine period and partly to the late sixth-century.¹ But he ignores in his conclusion the early Byzantine pottery, which attests, like the first collection of coins, the earlier date of the original substructure. There is nothing surprising in the further information which he gives us, that the seventh-century structure is of inferior quality, since many churches were then restored in haste and with insufficient resources and the restored church was, as the capitals and the mosaics attest, less capacious and less ornate than the primitive one.

Dr. Schneider has not raised the question why a church was ever erected on this very unsuitable site, even though he assigns its erection to the very period when the Jerusalem Christians were too fully occupied in restoring their ruined churches to have leisure or resources for new foundations. The site was not a matter of choice, but was imposed on the fifth-century builders by the existence of a traditional sanctuary, the deep pit in the centre of the quadrilateral, the sacred character of which is clearly attested by the fourteen crosses graven on its walls and roof. This is the unanimous conclusion of previous investigators,² but is rejected by Dr. Schneider, who declares that the pit was a Byzantine cistern and at the same time inconsistently ascribes to the Roman or Byzantine period the aperture, 95 cm. long and 53 cm. wide, made in its side.³ He does not inform us that this object of Christian veneration was, after the destruction of the church which enclosed it, transformed into an Arab cistern. For this purpose the lateral apertures were walled in and the inside was coated with *hamra* to prevent percolation. It was after the removal of the *hamra* that the crosses appeared graven on the bare rock.⁴ Thus the absence

¹ "Dazu stimmten auch die Keramikreste, die ich aus den Fugen aufsammelte; neben Frühbyzantinischem findet sich dünnwandige, gerillte Keramik aus sprödem, etwas splitterigem Ton, die nach gesicherten anderweitigen Befunden in das späte 6. Jh. zu setzen ist" (*l. c.*, p. 180).

² Cf. Germer-Durand, *Rev. Bibl.* 23 (1914) 225; Vincent, *Jérusalem*, II, 505 etc.

³ "Die mittlere [Höhle] (II) war in jüdischer Zeit ein Grab mit Treppeneingang von Osten; in römischer oder byzantinischer Zeit vertiefte man dieses, schlug oben eine runde Öffnung ein und rechts oben an der Seite ein gucklochartiges Fensterchen, das nach einen großen Raum (III) führt" (*l. c.*, p. 178).

⁴ See references in n. 2.

of any coating on the inner surface of the pit in the Byzantine period is another obstacle to Dr. Schneider's inconsistent hypothesis.

II.

While Dr. Schneider's contribution to the archaeological side of the question contains, as we have seen, some elements of value, the same cannot be said of his discussion of the site of the house of Caiphas according to the texts of the pre-crusade period. The five texts which he cites could be doubled in number¹ and their obvious implications are in all cases ignored. Only part of the relevant text of Theodosius is given, that which locates the house of Caiphas at *L passus* from Sancta Sion. The *C passus* between it and the Praetorium are not mentioned and thus the unreliability of the *passus* measures of Theodosius here exemplified, since *CL passus romani* do not give a fourth of the distance between Sancta Sion and the Praetorium of that epoch, is not made known to the reader and seems to be unknown to Dr. Schneider himself. In the text of the monk Bernard no notice is taken of the fact that the words "in directum ad orientem" clearly locate the denial of St. Peter on the Assumptionist and not on the Armenian site, as Fr. Vincent and Fr. Abel fully admit.² The text of the pilgrim of Bordeaux: "In eadem ascenditur Sion" is translated: "Wenn man hinaufsteigt, kommt man nach Sion;" and we are informed that this interpretation is final,³ notwithstanding the obvious impossibility of finding

¹ To the texts cited by Dr. Schneider p. 182 should be added: Prudentius, *Tituli Historiarum* XL et XLI (ed. Bergman Corpus Vindob. 61, 445), who seems to indicate a place of tombs; the *Typicon of Jerusalem* (Goussen, *Über georg. Drucke u. Handschr. die Festordnung u. den Heiligenkalender des altchr. Jerusalems betreffend*, München-Gladbach 1923, p. 18 and 29); the *Life of Constantine and Helena* (Rev. Or. Chrét. 1905, p. 167); the *Armenian Description of the Holy Places* (English version in *Pal. Explor. Fund Qu. St.*, 1896, p. 348); Epiphanius the Hagiopolitan (*P. G.* CXX, 261). To the truncated text of Theodosius should be supplied the immediately following words: "De domo Caiphae ad praetorium Pilati plus minus passus numero C."

² Vincent, *Jérusalem*, II, 514; Abel, *Rev. Bibl.* 37 (1928) 585. The "Ecclesia S. Stephani" of Bernard is the Diaconicon of Sancta Sion which contained relics of that saint. Cf. Thomsen, *Zts. Dts. Pal.-Ver.* 52 (1929) 213 and *Biblica* 10 (1929) 92.

³ "Diese, neuerdings heftig umstrittene Stelle ist jetzt in der *RB.* 1929, S. 156, endgültig interpretiert" (l. c., p. 182).

either an equivalent of *in eadem* in the translation or a conditional sentence with two verbs in the original. Dr. Schneider has here given us an incorrect German rendering of an incorrect French rendering of a truncated text. He is apparently unaware of my refutation of the French interpretation¹ or of the fact that it has been subsequently modified by its original proposer, Fr. Vincent.² He has also misinterpreted the text of the priest and monk Epiphanius, which is supposed to indicate the Armenian site of the house of Caiphas, through not knowing the meaning of τὴν ἁγίαν Σιών, "the basilica of Sancta Sion", where earlier and later texts locate the house supposed to have been purchased by St. John in Jerusalem.³ This text informs us that Caiphas during his year of office dwelt in the house of St. John, "who was known to the highpriest," and is the first indication of the migration of the house of Caiphas from the church of St. Peter to the basilica of Sancta Sion. Later pre-crusade writers, the author of the Armenian description and the monk Epiphanius the Hagiopolitan, and even Daniel the Hegoumenos immediately after the arrival of the crusaders, attest this second localisation of the house of Caiphas and also locate the Praetorium in the same basilica.⁴ It is in 1145 that we have the first mention of a small church on the Armenian site, identified with the Praetorium and the house of Caiphas.⁵ The conjunction of these two souvenirs, both in the basilica of Sancta Sion before the arrival of the crusaders and in the church of the Holy Redeemer to the north of the basilica half-a-century after their arrival, assigns the origin of the third traditional site of the house of Caiphas to

¹ Cf. *Biblica* 10 (1929) 116—125: "The House of Caiphas and the Pilgrim of Bordeaux." This article is not mentioned in Dr. Schneider's bibliography at p. 176.

² *Rev. Bibl.* 39 (1930) 250. Cf. *Biblica* 12 (1931) 417—418.

³ Sophronius, *Anacreontica* XX, PG. 87, 3821; Hippolytus of Thebes ed. Diekamp, p. 6. 29; Joannes Damsc. Hom. II in Dormitionem BMV., PG. 96, 729; Willibald, ed. Tobler-Molinier, *Itin. Hieros.* I, 264. Of these texts that of Hippolytus alone is explicit; the others are implicit as they locate the death of the Blessed Virgin in the church of Sancta Sion. John's house is thus located on the site of the basilica and not on the Armenian site.

⁴ See the texts in PG., 120, 261; *Pal. Expl. Fund Qu.St.*, 1896, p. 348; Khitrowo, p. 35.

⁵ Anonymus VII, ed. Tobler, *Descriptiones Terrae Sanctae*, p. 104.

the mediaeval period. We can only mention very briefly here the most important of the traditional indications of the primitive site of the house of Caiphas which Dr. Schneider has entirely ignored.

In the first place the most conspicuous opponent of the Assumptionist site of the house of Caiphas, Fr. Vincent, admits that there was only one church of St. Peter at Jerusalem in the period between the Persian invasion and the arrival of the crusaders and that this church was on the Assumptionist site.¹ All the texts of this period, if not distorted from their obvious sense, suggest this conclusion. The church of St. Peter on the site of the repentance mentioned by the *Commematorium* and the *Typicon of the Anastasis* is that of the mediaeval writers on the same site, admittedly revealed by the excavations at St. Peter in Gallicantu. The church of St. Peter on the site of the denial, located by the monk Bernard to the east of Sancta Sion, is also on the Assumptionist site. The church of St. Peter, which marks the station of the house of Caiphas in the *Typicon of Jerusalem* and in the *Typicon of the Anastasis*, is one and the same, which again establishes the identity of the sites of the denial and repentance of St. Peter. The *Typicon of Jerusalem* in its fairly complete liturgical calendar only mentions one church of St. Peter, that on the site of the house of Caiphas. The *Commematorium*,² a century later, in its statistical list of churches and monasteries in and near Jerusalem only mentions one church of St. Peter, that on the site of the repentance. Here again the sites of the denial and repentance of St. Peter are clearly identified and attached to the one church of St. Peter on the Assumptionist site. We have already seen how these

¹ *Jérusalem*, II, 514.

² It is important to notice that the *Commematorium* excludes monks and monastery from the church which only possesses "inter presbyteros et clericos V." The inscription, which attests a monastery on the north side, is assigned epigraphically to the VIII—IX centuries by Vincent (*Rev. Bibl.* 17 [1908] 406) and to a "restauration accomplie vers le IX^e siècle" by the Professeurs de Notre Dame de France (*La Palestine*, ed. 2, 1912, p. 159). It was apparently at this monastic restoration of the church of St. Peter, which had just lost the primitive souvenir of the house of Caiphas, that the sacred grotto became the place of the repentance of St. Peter, since it could no longer be regarded as the prison of the Lord (Cf. *Biblica* 12 [1931] 432—434).

two sites were separated in the late pre-crusade period by the localisation of the residence of Caiphas during his year of office in the house of his friend, St. John, the basilica of Sancta Sion. Hence the church is called "The Repentance of St. Peter" in the *Typicon of the Anastasis*.

Let us now pass to the earlier period before the Persian invasion in 614. According to the *Typicon of Jerusalem* it was the church of St. Peter on the site of the house of Caiphas which was restored and to which the station of the house of Caiphas on Holy Thursday was attached. As a change of the site in the seventh century involved a similar change in the station, it could only have been made by the ecclesiastical authorities of Jerusalem at a period when the original traditional site was well known. It seems unreasonable to attribute such a procedure to the official custodians of the sacred sites, and it is difficult to imagine why they should have selected a new and most difficult site at St. Peter in Gallicantu for the transferred sanctuary, in preference to the supposedly earlier and easy site in the Armenian grounds, at a period when so many other churches had to be rebuilt in haste and with insufficient means. Thus while the change in the site of the house of Caiphas in the late pre-crusade period is perfectly intelligible and solidly attested, such a change in the seventh century is excluded by the *Typicon* and would raise insoluble difficulties.

Again the pilgrims who mention the church of St. Peter before and after the Persian invasion agree in locating it between Sancta Sion and the Praetorium in their descriptions of the sites. The identical order manifested in these itineraria by Theodosius and the author of the *Breviary of Jerusalem* in the sixth century and by the author of the *Commemoratorium* and Bernard in the ninth century is obviously topographical. First comes Golgotha, then Sancta Sion to the south, then St. Peter's to the east, then the Praetorium (or the Temple in the case of Bernard) to the north-east. A late sixth-century pilgrim, the Anonymus Placentinus,¹ confirms this conclusion by a change

¹ Geyer, *Itin. Hieros.*, p. 174.

in the usual itinerarium which accounts for his omission of the church of St. Peter. He mentions the new church of Sancta Maria Nova between Sancta Sion and the Praetorium, and must therefore have gone north-eastwards from Sancta Sion. The church of St. Peter to the east of Sancta Sion was thus excluded from his itinerarium, but would have been included in it if it lay immediately to the north of Sancta Sion on the Armenian site.

The "basilica grandis S. Petri" on the site of the house of Caiphas is obviously too important to be excluded from the churches of Jerusalem represented on the mosaic of Madaba, a contemporary monument of the highest authority. As the basilica of Sancta Sion is identified with certainty on the mosaic there is no difficulty in determining the neighbouring edifices, located on the Armenian site immediately to the north of this basilica and on the Assumptionist site 250 metres to the east of it. On the former site is a small monastery, clearly recognisable as such from its red roof and north-south orientation. On the latter is a basilica of second rank of which the eastern part has perished but the western survives and amply determines its character and approximate size. Thus this contemporary monument at once confirms the Assumptionist site and excludes the Armenian site of the "basilica grandis S. Petri." When this argument was first proposed in 1928 no objection had been raised against the identification of the church of St. Peter in Gallicantu on the mosaic, affirmed alike by defenders of the Armenian site like Vincent-Abel and by defenders of the Assumptionist site like Gisler, Goussen and Dalman.¹ In 1929 Professor Thomsen proposed an exchange of the generally admitted sites of the churches of Siloe and St. Peter in Gallicantu on the mosaic, with the avowed object of correcting an error in perspective, by no means unique, and of identifying St. Peter in Gallicantu with an edifice which might be regarded as a monastery.² The

¹ Cf. Vincent-Abel, *Jérusalem*, II, 4 (1926), p. 923; Gisler, *Das hl. Land* 56 (1912) 223; Goussen, *Über georgische Drucke* . . . , p. 29; Dalman, *Orte u. Wege Jesu*, Gütersloh 1924, Pl. II and p. 347.

² *Zts. Dts. Pal.-Ver.* 52 (1929) 211 text and nn. 3 and 5. The text cited by Thomsen as suggesting a monastery is the notorious Armenian list of churches and con-

exchange could not be effected without depriving Siloe of its cupola, attested by the "ecclesia volubilis" of the pilgrim of Piacenza, by the excavations of Bliss and Dickie on the site and by Gisler's representation of the church in his coloured reproduction of the mosaic itself.¹ Siloe moreover had to be deprived of its adjoining tower, of its natural position at the south-eastern angle of the city and had to be located north instead of south of St. Peter in Gallicantu. And yet Dr. Schneider has availed himself of the sole authority of Thomsen in this matter, without any discussion of his theory, to inform his readers that the previously unanimous identification of St. Peter in Gallicantu on the mosaic was only "geistreiche Vermutung."² It is obvious that the evidence of this contemporary monument is entirely opposed to his interpretation of the archaeological discoveries made at St. Peter in Gallicantu and he is therefore obliged either to ignore this evidence or to reconsider his archaeological conclusions.

The most important indication of the site of the house of Caiphas is given by the pilgrim of Bordeaux in 333. The text presents little difficulty if it is considered in its entirety and in its context. "Item exeuntibus Hierusalem, ut ascendas Sion, in parte sinistra et deorsum in valle iuxta murum est piscina quae dicitur Silua . . . [a brief description of Siloe]. In eadem (valle) ascenditur Sion et paret ubi fuit domus Caiphae sacerdotis, et columna adhuc ibi est, in qua Christum flagellis ceciderunt."³

vents founded by Tiridates in Jerusalem. As it places the monastery of St. Peter *outside the city* it cannot be earlier than the late tenth century and is probably much later (Cf. *Biblica* 10 [1929] 290—292).

¹ Cf. Geyer, *Itin. Hieros.*, p. 176; Bliss and Dickie, *Excavations at Jerusalem 1894—1897*, London 1898, p. 178 ss.; Vincent-Abel, *Jérusalem*, II, p. 861: "Non moins caractéristique du sanctuaire de Siloé est la petite coupole impliquée par le carré de piles massives au milieu du vaisseau."

² "Jeder der genannten Gelehrten mag seine Gründe für und gegen haben; im Endeffekt bleibt eben alles doch nur geistreiche Vermutung" (*l. c.*, p. 185). The *large* ancient church, definitely assigned to the Armenian site by Dr. Schneider (*l. c.*, p. 182) but not attested by the mosaic, is merely suggested by the discoveries according to Fr. Vincent (*Jérusalem*, II, 500, n. 1). There is no proof that the fragment of pavement discovered and the door-threshold thirty metres to the west of it belonged to the same edifice, no suggestion even that the edifice was a church of St. Peter.

³ Geyer, *Itin. Hieros.*, p. 22.

I have supplied *valle* after *eadem* for three reasons: 1) The other alternatives, *Silua* and *parte*, are obviously excluded, since no ascent to Sion can be made in the pool of Siloe and the ascent to Sion is not, from the point of view of the pilgrim, in the same direction (*in eadem parte*) as Siloe, namely to the left (*in parte sinistra*), but rather in the opposite direction, to the right. 2) The words *in valle* are rendered emphatic by the preceding *deorsum* and the contrast between *deorsum* and *ascenditur* implies a subsequent allusion to the preceding *deorsum in valle*. 3) As the pilgrim first localises and then describes Siloe, it is natural to conclude that he also intends to localise and describe the next site mentioned, the house of Caiphas. It is with a view to a localisation that he substitutes for his ordinary transitional particle *inde* the expression *in eadem valle* and so informs us that the house of Caiphas appears on the side of the valley during the ascent from Siloe to Sion.¹ As the Sion enclosure is not *in eadem valle* it is clear that *ascenditur* cannot imply the completion of the ascent and that the Armenian site, which is based on this sense of *ascenditur*, is necessarily excluded. Thus the text of the pilgrim supports the Assumptionist site, which is on the old road from Siloe to Sion about half-way up the side of the valley. The description of the house of Caiphas is confined to the mention of the pillar of the scourging and this brings us to the third part of Dr. Schneider's article.

III.

My search for traces of a missing pillar at St. Peter in Galllicantu was not due, as Dr. Schneider asserts (naturally without any attempt at proof), to an erroneous conception of the Jewish method of scourging,² but was based on the well known traditional

¹ It seems to me very probable but not certain that *in eadem valle* goes with the *paret* clause as well as with the *ascenditur* clause, but, even if it is not grammatically connected with *paret*, it is undoubtedly used in view of a localisation and the place localised is the house of Caiphas.

² "Diese nicht zu entschuldigende Unkenntnis des Talmuds und die daraus resultierende falsche Interpretation der Steinösen haben Power aber noch zu einer viel bedenklicheren Sache veranlaßt. Er mußte nämlich die Existenz einer Säule nachweisen, gegen die der Gefesselte gepreßt war" (l. c., p. 188).

attestation that a pillar, at which Our Lord was supposed to have been scourged, had been transferred from the traditional site of the house of Caiphas to the church of the Cenacle in the fourth century. The discovery of the original site of the transferred pillar would obviously confirm the textual indications of the primitive traditional site of the house of Caiphas and might be also extremely useful in interpreting other archaeological discoveries. The transfer of the pillar, located in the house of Caiphas by the pilgrim of Bordeaux and in the church of the Cenacle by several later writers, is explicitly attested by Prudentius in the fourth and Theodosius in the sixth century and is admitted by such representative scholars as Zahn, Vincent, Benzinger, Baumstark, Abel, Dalman.¹ Dr. Schneider first declares that this transfer is "unmöglich," but subsequently that its attestation by Theodosius in the sixth century may be founded on fact.² He misinterprets the text of the pilgrim of Bordeaux: "paret ubi fuit domus Caiphae sacerdotis, et columna adhuc ibi est," in which *ibi*, referred by him to the still unreached Sion, naturally refers to *ubi*,³ and *adhuc* implies that the pillar still retains its original site in the house of Caiphas. He ignores the text of Prudentius who says that Our Lord was scourged at the pillar "in his aedibus," that is in the house of Caiphas just described, and that the pillar still exists "templumque gerit." Even still it is unintelligible that he denies any tradition of a scourging in the house of Caiphas before the 16th century,⁴ since he admits that sixth-century tradition, as attested

¹ Cf. Th. Zahn, *N. Kirchl. Zts.* 20 (1899) 389; L. H. Vincent, *Rev. Bibl.* 14 (1905) 151; I. Benzinger in Baedeker, *Palästina u. Syrien*, 1910, p. 67; A. Baumstark, *Byzant. Zts.* 20 (1911) 183; F. M. Abel, *Jérusalem*, II, 454; G. Dalman, *Orte u. Wege Jesu*, 1924, p. 347.

² "Auch die geforderte Translation des Pfeilers nach Sion ist unmöglich" (*l. c.*, p. 188). "Es wird das eine Säule gewesen sein, die man aus den Ruinen des angeblichen Kaiphaspalastes zum Bau des Portikus holte, falls auf die betreffende Notiz des Theodosius überhaupt etwas zu geben ist" (*ib.*, p. 189). This second statement ignores the reason of the transfer and the fact that the pillar was believed to be a scourging pillar both before and after the transfer.

³ "Der Schriftsteller . . . sieht oben auf dem Sion den Kaiphaspalast und die Geißelsäule, die dem Text nach übrigens nicht einmal im Palast stehend gedacht werden muß" (*l. c.*, p. 182).

⁴ *l. c.*, p. 189.

by Theodosius, ascribed the origin of the pillar to the house of Caiphas and might be expected to know that a contemporary document, the *Breviary of Jerusalem*, mentions two scourgings of Christ, one at this pillar and the other in the Praetorium.¹ He also ignores the attestation of the author of the *Life of Constantine and Helena* in the tenth century that Our Lord was scourged at this pillar *by the Jews*. Nor does he inform his readers that the tradition of a scourging of Christ at a pillar in the house of Caiphas has an apparent scriptural basis, which helps to explain its origin. Our Lord according to the Evangelists was beaten by the Jews in this house and St. Luke uses the very same verb, *δέρειν*, to express this beating and that subsequently inflicted on the Apostles by order of the Sanhedrin, which is generally regarded as scourging at a pillar.² Other exegetical considerations, which prevent us from accepting this traditional conclusion, need not be mentioned here, since we are concerned with the existence of the tradition, not with its authenticity.

There were two obvious indications of the exact spot in the underground galleries at St. Peter in Gallicantu where traces of a missing pillar might be found. A unique pair of rings in the roof, evidently used for suspension, had already suggested to Fr. Germer-Durand the hypothesis of a place of scourging, rendered all the more remarkable by the fact that the traditional evidence just discussed was entirely ignored by the author of this purely archaeological deduction. It was evident moreover that the existence of a pillar at this spot would restore the imperfect and yet obvious symmetry of the two parallel rows of pillars in the interior atrium of this ancient hypogaeum. Hence, after long hesitation, occasioned by the statement that "aucun pilier n'existait primitivement à cet endroit, alors que la symétrie gardée partout ailleurs aurait exigé qu'il y en eût un,"³ I wrote to its author, Fr. Marchet, explaining my views and requesting

¹ Geyer, *Itin. Hieros.*, p. 154—155.

² Cf. *Lc.* 22, 63 *δέροντες* and *Act.* 5, 40 *δείραντες*.

³ *Le véritable emplacement du palais de Caïphe . . .*, p. 81, n. 1. Fr. Boubet had seen the marks of chiselling in 1910 (*Biblica* 10 [1929] 398, n. 2) before erecting there a temporary pillar in masonry which naturally prevented further investigation.

him to examine the site. The results of this examination with the photograph and the necessary measurements were published in *Biblica*,¹ but seem to be in great part unknown to Dr. Schneider. The portion of the assumed site of the pillar which adjoins the atrium originally formed part of a rocky ridge which was levelled by chiselling for some purpose. One dimension of the chiselling, which is slightly irregular like all Jewish work of a similar kind and period, is 55 to 57 cm. The corresponding dimension of the two adjoining pillars is 55 to 58 cm. This most important factor in the identification of the object for which the chiselling was made is entirely ignored by Dr. Schneider, who also ignores the indication offered by the imperfect symmetry of the two rows of pillars. No conclusion can be drawn from the other dimension of the chiselling, 46 cm., as only a partial levelling was required in this direction. The corresponding dimension of the adjoining pillars is 75 to 88 cm. These two pillars, 55 to 58 cm. wide at the base, are only 40 to 43 cm. wide at the top. It is thus certain that a similar pillar, 55 to 57 cm. wide at the base, would fit easily at the top between the two stone rings which are 55 cm. apart. Dr. Schneider's contradictory statement,² that a pillar of the dimensions suggested by the chiselling would have covered both rings at the top, only shows that he neglected to measure the pillars himself and ignored the measurements made by Fr. Marchet and given in my article. His hypothesis of a press as an explanation of the chiselling similarly shows his ignorance of the fact that Fr. Vincent and Fr. Germer-Durand, at an early period of the excavations, observed "l'amorce de tête de cet apparent pilier, bien détachée entre les deux gros anneaux dans la plate-bande de roc sous le plafond."³ This discovery, which excludes a press and implies the presence of

¹ Cf. *Bibl.* 10 (1929) 398—399.

² "Nehmen wir selbst einmal an, es sei ein Pfeiler von der Mächtigkeit der Einarbeitung vorhanden gewesen, und denken wir ihn als oben anstoßend, so verdeckt er die beiden unteren Löcher der Ösen" (*l. c.*, p. 188).

³ *Rev. Bibl.* 39 (1930) 253. It is curious that Fr. Vincent in giving us this information indicates no reason why the wall-hypothesis was accepted and the pillar-hypothesis rejected.

either a pillar or a wall at this spot, was long discussed by the two archaeologists who finally decided in favour of a wall. They must have observed that the first adaptation of the original hypogaeum to another purpose, which necessitated the removal of all the inner walls, naturally supposed the retention of all the pillars for the support of the roof. But this correct observation did not justify the conclusion that a wall had been then removed from this particular spot, since a pillar might have been removed from it for a special reason at a subsequent period. Such a hypothesis however could scarcely have occurred to the two archaeologists, who had no knowledge of the chiselling and its dimensions, which suggest a pillar, and did not advert to the fact that a pillar had been removed from the traditional site of the house of Caiphas in the fourth century.

Another vital element in the identification of the traditional pillar of the scourging, which Dr. Schneider has entirely ignored, is the extraordinary resemblance of the pillar of the Cenacle to the seven extant pillars of the house of Caiphas with which it was originally associated.¹ It is enough to mention here four characteristics in the early descriptions of the pillar of the Cenacle, which have caused surprise, and which are all explained by a knowledge of its origin. Its position in the portico of the church of the Cenacle, "*ecclesiae porticum sustinens*" (Jerome), supporting the church proper in the upper storey of the edifice, "*templumque gerit*" (Prudentius), was evidently suggested to the Christians by its previous function as a supporting pillar. The red streaks of the pillar, piously interpreted as traces of the blood of the Saviour, "*infecta cruore Domini*" (Jerome), are due to oxide of iron with which the pillars of the house of Caiphas are impregnated. The holes and inequalities on its surface, attested by the three sixth-century pilgrims and regarded by them as imprints miraculously made by various parts of the body of Christ, have their counterparts in the porous and friable

¹ Dr. Schneider gets his information about this pillar, "*columna marmorea*," from Bede and Adamnan, two late writers who never saw it, but ignores the more minute descriptions of the three sixth-century pilgrims who undoubtedly saw it (*l. c.*, p. 189). We cannot exclude the possibility that the original pillar was destroyed by the Persians in 614.

limestone pillars of the subterranean galleries. The quadrangular shape attributed to the pillar in its earliest iconographical representation, which surprised Heisenberg and was ascribed by him to an innovation of Roman art, is still attested by the chiselling which excludes the round and more graceful column represented by later artists.¹ It is clear too why the Christians transferred this pillar and none of the seven others to a church for veneration, because it alone was indicated as a scourging-pillar by the rings at each side of it to which the hands of the victim could be attached. As they were presumably unacquainted with the Mishna treatise, *Makkot*, they would have based their inference as to the function of the pillar, not on Jewish prescriptions, but on the prevalent Roman custom² with which they were undoubtedly familiar.

It seems to me for these and other reasons solidly established that the scourging pillar, venerated in the Cenacle, was transferred thither from St. Peter in Gallicantu and that we have here a definite archaeological confirmation of the primitive site of the house of Caiphas, determined independently by the study of the texts. It is also evident that Dr. Schneider, in attributing the discovery of a traditional scourging pillar in the house of Caiphas to the "Phantasie eines Küsters," has ignored *all* the archaeological proofs on which this discovery is based. We have still to consider briefly, not whether archaeological discoveries establish, independently of scripture and tradition, the site of the house of Caiphas, but whether they solidly confirm scriptural and traditional indications of that site.³

¹ Cf. A. Heisenberg, *Ikonographische Studien in Sitzungsberichte d. Bayer. Akad. d. Wiss. Phil.-Hist. Kl.* 1921, 4 (München 1922) 59—65 and *Biblica* 12 (1931) 230.

² Cf. the description of S. Gregorius Nazianzenus: "καὶ τοὺς πλῆρεις αἰμάτων χίονας ταῖς τούτων χερσὶ κυκλουμένους ξαινομένων ταῖς μάστιγι (Contra Julianum I, 86 in PG. 35, 613). According to Aulus Gellius, *Noctes Atticae*, lib. XII, cap. 3, the legs were also bound, "crura eius et manus," in this case apparently to the rings on the adjoining pillars, but the object of these rings is not so clear as that of the rings in the roof and our argument is not deduced from them.

³ Dr. Schneider affirms that I regard the authenticity of this site as "jetzt zu unumstößlicher Gewißheit erhoben" (*l. c.*, p. 185). In reality I regard the use even of the word "Gewißheit" as quite unwarranted in these delicate questions of authenticity. My view is that St. Peter in Gallicantu is certainly the fourth-century traditional site and very probably the authentic site of the house of Caiphas.

IV.

Three archaeological arguments have been proposed in confirmation of the early fourth-century tradition which locates the house of Caiphas at St. Peter in Gallicantu. The first of these is based on the discovery of a heavy stone door-lintel, bearing an inscription in Hebrew square characters, of which the final word, *gorban*, has been deciphered with certainty. The obscurity of the rest of the inscription, on which Dr. Schneider insists,¹ does not affect the conclusion of all previous investigators that the door-lintel originally belonged to a building in which sacred offerings were deposited. This building would be naturally attached either to the temple or to the official residence of the high priest. Fr. Vincent assigned the door-lintel to an annex of the temple and explained its presence at St. Peter in Gallicantu as an accidental result of its subsequent employment in the construction of the Colonia Aelia Hadriana.² But this hypothesis is excluded by the marks on the stone itself, which attest that the attempt made to hew it into shape for building purposes had been abandoned by reason of its hardness.³ When Dr. Schneider affirms that there is not the least reason to think that the door-lintel was discovered near its original site, he ignores these marks which render improbable the transportation hypothesis by excluding the one plausible reason on which it might be based. Independently of these, the size and heaviness of the stone attach it to the place of its discovery in the absence of all proof to the contrary.

Dr. Schneider objects to the second argument that the weights and measures on which it is based were found dispersed over the whole area of the excavations and are therefore merely part of the débris which rolled down from the upper city.⁴ He

¹ "Zudem ist die Inschrift, wie bereits erwähnt, nicht sicher zu lesen" (*l. c.*, p. 187).

² *Jérusalem*, II, 510 and *Rev. Bibl.* 39 (1930) 250, n. 3.

³ "La pierre sur laquelle elle (l'inscription) est gravée est un linteau de porte en pierre dure, très dure, si bien qu'on a renoncé à la débiter pour en faire de la pierre à bâtir, après avoir essayé" (Germer-Durand, *Rev. Bibl.* 23 [1914] 238).

⁴ "Die Gewichte und Maße waren über das ganze Grabungsterrain zerstreut, sind demnach als von oben herabgerollter Schutt anzusprechen" (*l. c.*, p. 187).

is evidently not acquainted with the real argument which is based, not on the mere discovery of weights and measures, but on the character and grouping of those which were discovered. Nor does he explain why these numerous objects halted half-way on their journey down the hill side at the traditional site of the house of Caiphas. He knows of the discovery of a mill, but has not noticed the fact that the dry measures, distinguished by their rectangular shape and representing multiples and subdivisions of the 'omer, were all discovered in its immediate vicinity and most of them in a rocky chamber beside it.¹ Could blind chance have conveyed them to so appropriate a site? He knows of the discovery of the door-lintel, but ignores the fact that most of the liquid measures, distinguished by their round shape and representing parts of the bath, were discovered in its neighbourhood,² which suggests, but does not prove, a connection between them and the sacred offerings in the store-room. He mentions the theory of a bureau for the verification of weights, but not the very remarkable discovery on which it is based thus described by Fr. Germer-Durand: "Il y a surtout une série de quarts de mines qui présente un intérêt spécial. A côté de ces petites meules de calcaire, au nombre de six, on a trouvé un poids en basalte de forme carrée, qui paraît être un étalon destiné à vérifier les autres, plusieurs sont en réalité plus forts ou plus faibles: l'un d'eux est lesté avec du plomb. Deux se rapprochent exactement de l'étalon (190 grammes) et l'un d'eux est estampillé. Il est marqué de la lettre *chin* qui en hébreu signifie 300. Et en effet le quart de mine dans cette série vaut 300 gérachs ou oboles."³ It has been shown in *Biblica* 10 (1929) 407—8 that the relative value of these weights: 1 mine = 60 shekels = 1200 gérachs, is that of the weights of the sanctuary given by Ezechiel (45, 12) and that the absolute value corresponds to

¹ "Plus de la moitié de ces mesures ont été retrouvées dans la chambre voisine du moulin, les autres n'en étaient pas éloignées" (Germer-Durand in *Conférences de Saint-Etienne*, Paris 1910, p. 99). This article is not in Dr. Schneider's bibliography.

² "Or c'est dans le voisinage de cette inscription que l'on a trouvé la plupart des mesures (the liquid measures)" (*ib.*, p. 96).

³ *Rev. Bibl.* 23 (1914) 237.

that of the ancient half-shekel weights discovered at other Palestinian sites. Again it is evident that weights rolling down the hill-side could not have formed so remarkable a group.

The third and most important archaeological argument is based on the discovery of a Jewish prison with its *arcta custodia*, common gaol and scourging apparatus at St. Peter in Gallicantu. From the prophet Jeremias, who was well acquainted with Hebrew prisons, we learn that such prisons were attached to the houses of official personages and consisted of two parts: a בור or very deep cistern-like pit for solitary confinement and a חצר המטרה or place of general and easier confinement associated with the בור but not otherwise described.¹ We expect therefore to find a similar prison attached to the residence of the chief judicial authority, where prisoners were confined and justice was administered in New Testament times. Our Lord was certainly imprisoned in the house of Caiphas on the night of Holy Thursday and such a prisoner would naturally be consigned to the *arcta custodia*. It is at least probable that the Apostles also were there imprisoned ἐν δεσμότητι τρηγῆσαι and there scourged by order of the Sanhedrin.² St. Luke does not state explicitly where these events occurred but may be supposed to refer to the prison and place of meeting of the Sanhedrin which he had mentioned in his Gospel.³ Early tradition supports this conclusion by locating a prison and a place of scourging in the house of Caiphas, which have been revealed to us by the excavations at St. Peter in Gallicantu.

The *raison d'être* of the erection of a church on this most difficult site was the sacred grotto which it enclosed, and the only evangelical souvenir of the house of Caiphas which this deep pit could possibly represent was the prison of Our Lord.

¹ Jer. 37, 15—21; 38, 6—13. Note that the בור was in the חצר המטרה.

² Act. 5, 17—41. Cf. C. Fouard, *Saint Pierre et les premières années du Christanisme*¹⁰, Paris 1908, p. 38, n. 4: "Depuis l'établissement des rois les palais et les forteresses avaient habituellement leurs cachots (III Reg. XXII, 27; Jer. XXXII, 2; XXXVII, 20; Neh. III, 25). La résidence des pontifes, vrais princes d'Israël sous la domination romaine, contenait quelque lieu de détention. Ce fut là que l'on enferma les captifs".

³ Lc. 22, 33. 54—66.

We have no direct traditional attestation of this fact for the simple reason that we have no description of the church while it retained the site of the house of Caiphas. But descriptions of the second and third sites of that house attest the prison of Christ as its chief claim to veneration¹ and thus confirm the identification of this prison with the sacred grotto on the primitive site. Originally a tomb about half its present depth, as appears from the dimensions of the six steps which gave access to it, this pit was artificially deepened for another purpose. The thirty-two steps by which the Christians descended into it suppose this transformation, which may be assigned to the pre-Christian period, since it is unnatural to assume that the veneration of the pit would have so considerably transformed it themselves without any conceivable motive. An orifice, 90 cm. long by 70 cm. wide, was made in the roof and a kind of lateral window, 95 cm. long by 53 cm. wide, in the north wall. A raised step, hewn out of the rock at the foot of this wall, enables one to get a full view of the interior through the window. These alterations of the original tomb, which Dr. Schneider makes no attempt to explain, are perfectly intelligible in the traditional hypothesis of a prison like the *בֵּית* of Jeremias. The prisoner would have been lowered by ropes through the orifice at the top, prevented from escaping by the increased depth of the pit and inspected by the gaoler who flashed his torch through the window. The reader can now understand the value of Dr. Schneider's assertion: "Die arcta custodia verdankt ihr Dasein nur der Entdeckung der Vorrichtung zur Geißelung in der Höhle nebenan."² He ignores all the arguments on which this hypothesis is based and attributes it to a discovery with which it has no direct connection. The only other hypotheses which might be advanced to explain the transformation of the original tomb are those of a silo and a cistern. These explanations are both quite possible from the purely archaeological point of view,

¹ The Armenian description mentions the site of the prison in the basilica of Sancta Sion and various texts (*Jérusalem*, II, 495) record the *carcer* on the Armenian site.

² *l. c.*, p. 187.

but the small dimensions of the pit, which is little over 4 metres in length and width, are unfavourable to the former and the fact that the pit had to be subsequently altered in order to serve as an Arab cistern does not recommend the latter. Thus the traditional hypothesis is the most natural and the least exposed to objections.

The ancient hypogaeum adjoining the sacred grotto on the north side was also transformed before the Christian period. Two stone rings were made in the roof at each side of one of the pillars, as we have already seen, and caused the Christians to distinguish this pillar from the seven others and regard it as a scourging pillar. Other rings of a similar character were made in the walls and pillars, with the obvious object of attaching either men or animals. Hence the two hypotheses of prison and stable to explain this transformation. It is clear that such rings were not inserted either by the builders of the hypogaeum or by the Christians. Dr. Schneider regards them as an indication of an Arab stable. Against this is the very serious difficulty that these underground galleries would be scarcely accessible to animals in the Arab period, as the church and its subsequent ruins would have blocked the entrance from the crypt. Moreover the similarity of the rings to the pair of rings on the roof assigns them naturally to the same pre-Christian period¹ and suggests a common purpose. If the early Christian interpretation of the rings on the roof be correct the rings on the walls and pillars were originally intended not for a stable but for a prison or **תַּעַר הַמַּטְרָה**, the natural counterpart of the adjoining **בּוֹר**. The stable hypothesis moreover offers no explanation of the unique pair of rings on the roof.

Against the traditional hypothesis of a place of scourging Dr. Schneider objects that the method of scourging thus implied differs from that prescribed in the Mishna, as the victim, suspended by means of the rings in the roof, would not have his

¹ The two rings on the pillar containing the *nefeš* evidently antedate the masonry which covered its inner surface (cf. *Rev. Bibl.* 34 [1925] 586, Fig. 2). As the object of this masonry was to strengthen the pillar for the better support of the church to be erected above it, the pre-Christian date of these rings also seems to be independently established.

back bent to receive the blows. He assumes therefore, what few modern scholars admit, that the judicial practice of the Sadducees in the time of Christ agreed even in minute details with the judicial theory of the Pharisees two centuries later and seems to ignore the fact that the evangelical narrative of the trial of Our Lord before the Sanhedrin does not agree with the prescriptions of the Mishna for such trials.¹ The bending posture, prescribed by the Mishna, is not given as a traditional datum, but is explicitly based on an erroneous exegesis of Deut. 25, 2. There is thus no sufficient reason for assuming that this practice was in use at an earlier period among the Sadducees, who are more likely to have followed the Roman method of suspension.

Dr. Schneider offers no explanation of the original adaptation of the tomb and the hypogaeum. His Byzantine cistern and Arab stable refer to later periods. Thus the prison hypothesis stands alone and if it cannot be strictly demonstrated, owing to the impossibility of dating exactly the alterations made in the tomb and the hypogaeum, it has the undoubted advantages of being suggested by scripture and tradition and of offering a satisfactory and homogeneous interpretation of all the archaeological data. This underground prison still attests the house of Caiphas on the traditional site after other traces of this house had been removed to make way for the fifth-century church. Dr. Schneider remarks that no traces of a house or palace are found over the mill.² But this mill, which was worked by an animal, is to the east of the church, outside the traditional site of the house, of which it would have been not a part but an annex.

The question of the site of the house of Caiphas is too complicated to be fully discussed in a single article. I hope however to have shown the reader the principal arguments on which my conclusions are based and the scientific method which I have

¹ Cf. as representatives of the modern scientific view G. Hölscher, *Sanhedrin u. Makkot*, Tübingen 1910, p. 14 and 35; H. Danby, *Tractate Sanhedrin*, London 1919, pp. IX—XVI; G. Aicher, *Der Prozeß Jesu*, Bonn 1929, p. 100; K. Kastner, *Jesus vor dem Hohen Rat*, Breslau 1930, p. 111.

² "Über der Mühle sind dazu noch keine Spuren irgendeines Hauses oder Palastes jener Zeit gefunden" (*l. c.*, p. 187). Kitchen utensils and food remains were found near the mill (*Rev. Bibl.* 23 [1914] 233—234).

followed. The study of the texts is fundamental. Tradition cannot affect archaeological data, but by enabling us to identify the site to which they belong, can offer a solid basis for their interpretation. On this basis alone, in my opinion, can some of these data be satisfactorily interpreted at St. Peter in Galliscantu.