

COINS FROM THE SYNAGOGUE OF CAPHARNAUM

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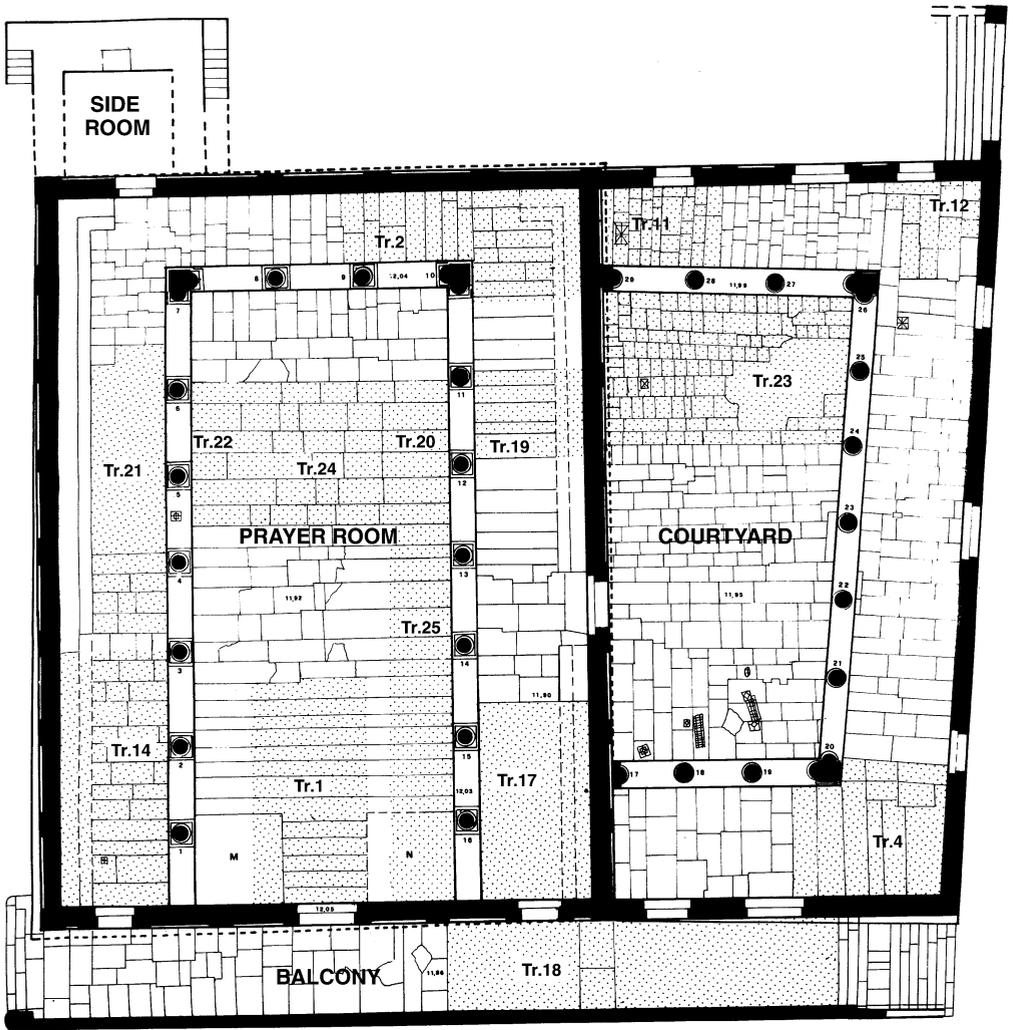
During the excavations of the synagogue in Capharnaum (from 1969 to 1981), the archaeologist Virgilio Corbo, with the assistance of this writer, catalogued more than 24,000 bronze coins. During those years fifteen trenches were opened within the internal section of the synagogue. Ten of these are located in the prayer room, four in the eastern courtyard, and one in the southern balcony. Not included in this mass of coins (but in no way less important) are those found in the large external area which directly connects the synagogue to the rest of the ancient city.

In this short writing I propose to answer two questions. 1) From which stratigraphic context do these coins come? 2) What archaeological evaluation has been proposed? I take up this argument to introduce the study which Dr. E. Arslan contributed for this issue of the *LA* concerning the treasure discovered in Trench 12. At the same time I hope that my article will receive due consideration in the current debate about the chronology of the synagogue.

1. The strata

In the excavation of the synagogue we have distinguished, from bottom to top, three basic strata. They are labelled by the letters A, B, C. Level A: structures, destroyed or partially reused to make place for the construction of the white limestone synagogue. Level B: artificial fill for the podium of the monumental synagogue. Level C: layer of mortar for the setting of the stone pavement and of the side benches. I will describe the basic characteristics of these strata beginning from the top.

Stratum C was found in the entire area of the prayer room and in the eastern courtyard, while it is lacking in the southern balcony, at least in the area of Trench 18. The whitish mortar is about 30 centimeters thick and most probably comes from the same limestone brought from far away for the construction of the synagogue. In the prayer room our ten trenches were intentionally opened in those places where the original stone pavement was not preserved. By systematically clearing the top face of the well preserved mortar, it became possible to restore the complete general scheme of the



The monumental Synagogue of Capernaum.

lines of the missing slabs, both in the prayer room and in the area of Trench 4 (Photo 1). Of course, the marks left by the floor slabs mean that the mortar was fresh, when they continued the process of laying down the slabs. In so far as it was possible we recorded the coins with traces of fresh mortar, as such, to distinguish them from others, less trustworthy, found immediately below the present surface.

Stratum B is the artificial fill of the podium upon which the monumental synagogue was constructed. It varies from trench to trench. The podium was created to do away with the natural slope of the ground from north to south, and for this reason the fill is deeper in the trenches to the south. Also to be noticed is a natural slope of the ground from west to east. In Trench 17 the fill was made up of superimposed layers of basalt blocks. In general, however, the stones of the fill were thrown together pell-mell and mixed with gravel. The top of the artificial podium was not only levelled off, but was also topped with a thin layer of white chips as if to create a temporary cap during the construction of the synagogue before going on to laying the floors down, which generally was left for the final part of the construction. There is the impression that the final trim of the blocks was partly done inside the synagogue during the building of the edifice. The levelling out of the upper part of the fill cannot be mistaken for a true stone pavement, for the simple reason that it does not have the essential element, namely compacted earth which characterizes a level which has been occupied for a certain length of time.

Most peculiar is the fill of Trench 18 on the balcony: here we ran into a conspicuous pile of limestone chips which were discarded during the construction of the building. They made up a sizeable dump pile which stretched out from north to south for a depth of about two meters. It was not always possible to clearly distinguish the fill of the podium from the level of destruction of the structures met in level A. In Trench 21, for instance, the walls of stratum A almost reach the height of the mortar of stratum C.

As a criterion for recording we always assigned doubtful objects to a higher level, as is the norm in archaeology. In Trench 1 the stratigraphy was badly disturbed. Strictly speaking, most of the material found under the mortar of level C is to be assigned to level A, although with possible intrusions and disturbances of level B.

Stratum A is easily set apart, especially because of the presence of basalt stone pavements preceded by characteristic accumulation of beaten earth. But even in this case, in doubt we assigned the coins (and the ceramics) to level B rather than to level A. The stone pavements of level A, found dis-

turbed in Trench 1, were well preserved in Trench 2 (Photo 4) and in Trenches 24 and 25, where two superimposed stone pavements were found.

In Trench 19 the floor was of white plaster. In Trench 17 there was a sizeable accumulation of compressed earth and there appeared also a terra refractaria oven. Another oven with a thick layer of compacted earth was found in Trench 23. We found not only floors, but various walls, thresholds, steps, drainage ditches for water and many other small details which the director of the excavation abundantly illustrated in the preliminary reports. Real and true floors were found only in level A, that is to say under level B of the artificial podium. No traces of floor were found between the upper part of level B and the mortar of level C.

2. The Coins

Coins from level C

Already during the campaign of 1969 we came upon 112 bronze coins within the synagogue area (Spijkermann 1970, pp. 106, 117). Of these, 101 coins come from level C. The four coins from level C found in Trench 1 seemed to us to be unreliable, in so far as they came from heterogeneous dirt accumulated over the mortar layer. In level C of Trench 2 we found 86 coins. Here too we came across some late Roman coins in the loose dirt near the top (Spijkermann 1970, p. 108 nn. 11-14). Later on however we found many coins encrusted with white mortar. It became clear that these had been abandoned in this level when the mortar was still fresh. The greatest concentration of coins was near the northeast corner where on May 29, 1969 we found some sixty of them. In the opening of Trench 4 in the eastern courtyard we found nothing new: of the 11 coins from level C only one contained "traces of lime mortar still attached" (Spijkermann 1970, p. 115, n. 117), and the others were considered less reliable.

In the summer of 1971, we removed from the synagogue all the blocks out of place which hindered the progress of our excavation, and we decided to clear the whole area of the aisles of the prayer room. In those places where the original slabs were missing, it was possible for us to trace the top part of the mortar layer of both the floor and the benches. In this homogeneous layer we found not tens, but thousands of coins (Photo 3). The greatest concentration was in the southern end of the western aisle, on the inside of the entrance door. There we collected 2,922 coins (Corbo 1972, p. 212, fig. 6.). It was Saturday, September 18, 1971. Already in the first

week of August we had found scores of various other coins in the fill of the eastern aisle. Because of lack of time we did not clean up the central nave. Later on in 1981 we found coins also in this section, when we opened Trenches 20, 22, 24, 25.

Since the coins from level C came from an open level and therefore subject to contamination, we were advised, even strongly encouraged, to open a trench there where the ancient stone pavement of the synagogue was preserved. In this way – they were saying – all doubt would be dispelled about possible intrusion.

Small patches of the ancient stone pavement were preserved in the area of the prayer room, but we refused to sacrifice these slabs. We preferred to open some trenches in the eastern courtyard where the stone pavement was abundantly preserved and was of an inferior quality. We opened Trench 12 near the northeast corner of the courtyard. It was July 6, 1972 (Photo 5). We gradually removed three large slabs and eight medium sized ones. This time we found about 6,000 coins (Corbo 1972, p. 231). The enlargement of Trench 12 was done three years later. In the same stratigraphic context (i.e. upon the mortar of level C) we collected another 19 kilograms of coins. The entire number after cleaning all the pieces amounted (together with the 6,000 coins found previously), to 20,323 pieces. It was June 17, 1971. (Photo 2).

In this Trench 12 we met not one but two levels of mortar one on top of the other and the coins were spread out between the upper layer and the slabs of the floor, or between the first and second layer of mortar (Corbo 1972, p. 231, fig. 22). The upper layer of mortar was less solid and mixed with various chips of stone useful for putting down the bed for the slabs, while the lower layer was more solid and contained smooth surfaces. We asked ourselves whether the lower layer of limestone was the bed of a floor (which disappeared) but the absence of any impression marks of the slabs convinced us that the two layers were simply contemporary. A quick analysis of the coins led us to the same conclusion. Actually, also between the two layers of mortar we found about 1,400 late Roman coins. When the lower layer of mortar was removed, it was possible to trace a water canal that had been cut and put out of use by the foundations of the eastern courtyard. The canal was full of soft dirt. There we found only a few pieces of late Roman potsherds, but no coins. We think that the canal served as a temporary drainage ditch for water, while the synagogue was being constructed.

We met something similar in the excavation of Trench 23. This is the description I made of it in 1972, “After removing the stone slabs of the

pavement, we came across a thick layer of mortar in which the lower portion of the slabs was imbedded. No coins or potsherds were found here. Immediately under the layer of mortar a fill appeared, made of rough basalt stones and a great amount of discarded white limestone chips. To be sure, the sequence was of no surprise to us; on the contrary, it was the normal phenomenon registered in all the trenches previously cut in the area of the synagogue. Apparently the white limestone slabs were trimmed *in situ* before being set in place. In Trench 23 we noticed an additional detail. Some 50 cm under the stone pavement, we came across conspicuous but discontinuous patches of mortar of the same nature as the mortar directly underlying the stone pavement. The interpretation is obvious: in this area, the mortar was also prepared for the final setting of the stone pavement of the courtyard” (Loffreda 1979, pp. 215-216). In this context we found various potsherds, especially rims of plates of the Late Roman C Ware of the first half of the 5th century, along with 9 Late Roman coins.

In the same year of 1972, Trench 17 was opened in the south side of the eastern aisle of the prayer room, where the ancient pavement had disappeared. In level C we already found, in August of 1971, 67 coins, of which 46 were in the mortar bed of the missing stone pavement and 21 in the mortar of the side benches. In layer C of Trench 21, excavated in 1977, we found 43 coins. In 1981 we opened, in the central nave of the prayer room, four new trenches which then we joined to form one single trench. In level C of Trench 25 we recorded 71 coins.

In conclusion: from level C, came 23,461 coins, with the greatest concentration in Trench 12 and Trench 14, while relatively few coins were gathered in the central nave of the prayer room and in the large Trench 23 which is located in the central part of the eastern courtyard. Even with such a variable intensity, the coins were spread throughout a large area and this is, in a certain sense, not less important than their quantity and indicates that they were not hidden to be found again.

Coins from stratum B

At the beginning of our excavation in the synagogue, the coins found in the artificial fill of the podium (level B) could be counted on one's fingers. The first late Roman coin (which we found in Trench 1 on May 15, 1969 and is dated from between 352 and 360 A.D.) left us perplexed, so much were we unprepared for that kind of dating for the synagogue of Capharnaum. We recorded it with a question mark and the director Virgil

Corbo suggested the hypothesis that the coin could have slipped in from the section of the trench (Spijkermann 1970, p. 106 n. 4). On May 11, I found another coin on the upper ridge of the foundation wall of the synagogue. It had the smaller shape of the late Roman coins, but it crumbled in my fingers. In the meantime we were collecting a large number of potsherds and in my diary I commented: "I already have the feeling that the synagogue reaches back into the third century A.D. and it is not to be excluded that it could also be of the fourth century".

In the fill of Trench 2 we did not find any coin which could be assigned to stratum B. Beginning with June 5 we started the excavation of Trench 4. After removing the thick layer of mortar we found 2 coins of the 4th century at 1.00 m and 1.25 m of depth and a third coin from 179-180 A.D. at 1.80 m. In 1972 we opened Trench 12 and we stopped at level C, because we wanted to keep it as a sample of the context in which we had found so many coins. In Trench 11 we found in the sealed fill only two late Roman coins.

In 1974 we opened Trench 14 in the western aisle of the prayer room and finally, in the sealed layer B, we found for the first time a large number of coins, to be exact, there were 236 of them. The coins were found within the whole depth of the fill. In *IEJ* 25 (1975) p. 261 we wrote: "in Trench 14 (the southern portion of the western aisle) more than one hundred late Roman coins were found in the foundation of the stylobate".

In 1975 we opened Trench 18 in the balcony. The length of the trench was meant to include in it a part of the foundation of the stairs on the southeast, a good portion of the area in front of the eastern courtyard, and the entire area in front of the east aisle of the prayer room. I have already emphasized that in this trench we did not find the usual layer of mortar of stratum C, and that the fill of stratum B was characterized by a conspicuous accumulation of white stone chips. The coins collected there amounted to 570 pieces. We set aside eight coins found in the first 40 cm beginning from the surface because of the possibility of contamination from above. The rest of the coins were found throughout the depth of level B until the appearance of the flooring of stratum A. Of special importance are the ten coins found in the fill of the southeast stairway. A pottery pipe coming from the area of the courtyard and cut off by the southern wall of the balcony was traced in the upper part of the fill. It probably was a canal used during the construction of the courtyard. At any rate the late coins found in the fill of the staircase provide a *terminus post quem* for it.

In stratum B of Trench 21 we found only two poorly preserved coins. It seems that they can be dated between the end of the 4th century and the first decade of the 5th century. The importance of these two coins comes

from the fact that they were found in the foundation of the stylobate. Another late Roman coin, also in a poor condition of preservation, was found in the foundation of the stylobate in Trench 17. To this should be added a coin from the 4th century found in the foundation of the stylobate in Trench 22. Finally, in 1981 three late Roman coins were found in stratum B of Trench 25.

Coins from stratum A

In Trench 1 seven coins were found under the mortar of stratum B. One of them (from 352-360 A.D.) was attributed to the fill, while the others, with a dating from 3rd/2nd century BC to 153-154 A.D., were assigned to stratum A although disturbed during the construction of the synagogue. In Trench 2 the situation is better. Level A is represented by two superimposed stone pavements. One coin from 341-346 A.D. comes from the upper pavement of a dwelling, while another coin (119/120 A.D.) was found in the lower stone pavement at 1.10 m of depth (Spijckermann 1970, n. 26 and p. 113, n. 96). In stratum A of Trench 17 we found two coins, one from the 3rd century A.D. and another belonging to Arcadius (383-388 A.D.). The coin of the 3rd century came from the southern section of the trench underneath the threshold of a dwelling (Corbo 1982, p. 351, table 5). Five coins from Trench 18 have been assigned to stratum A. Of these two are illegible, one seems to be late Roman, and the other two are of Honorius (395-401 A.D.) and of Theodosius (383-388 A.D.). In Trench 24 two coins were found in stratum A, which seem to be late-Hellenistic, but they are still in the process of being further studied by Dr. Arslan.

In conclusion, the distribution of coins is like this: 23,461 from stratum C, 894 from stratum B, 20 from stratum A. Here is a picture, in general, according to trenches and the strata:

	Str. C	Str. B	Str. A		Str. C	Str. B	Str. A
Tr. 1	4	1	6	Tr. 18	0	570	6
Tr. 2	86	0	2	Tr. 21	43	2	0
Tr. 4	11	3	0	Tr. 22	0	1	0
Tr. 11	0	2	0	Tr. 23	0	9	0
Tr. 12	20,323	0	0	Tr. 24	0	0	3
Tr. 14	2,922	236	0	Tr. 25	71	3	0
Tr. 17	1	1	3				

3. Evaluation

The construction of the monumental synagogue of Capharnaum, on the one hand presupposes one single initial project, and on the other hand, the completion of the project through successive phases. This is what the director of the excavation wrote in 1975: “Dall’osservazione dei muri e delle fondazioni dei diversi edifici della sinagoga è possibile stabilire una cronologia relativa nella costruzione dei medesimi: bisogna però ammettere che vi fu sino dall’inizio un progetto unitario che prevedeva la costruzione delle tre parti: sinagoga, cortile e ripostiglio, oltre naturalmente agli accessi a sud e a nord. La sinagoga fu certamente costruita per prima. Il cortile fu addossato in un secondo tempo alla parete orientale della sinagoga; infatti non si nota nessun addentellato fra le due parti e nessuno spozalizio di murature neppure nelle fondazioni dei due blocchi. La stessa cosa avvenne anche per il ripostiglio 143 addossato alla parete nord della sinagoga. Questa non fu un’aggiunta imprevista: infatti l’architetto aveva lasciata una piccola porta di comunicazione ed aveva soppresso anche la lesena che sarebbe stata inglobata nell’interno del ripostiglio. Ci si potrebbe domandare se anche la balconata sia stata costruita in due tempi diversi; in un primo tempo la scalinata di sud-ovest e il tratto antistante la sinagoga; in seguito il prolungamento all’area antistante il cortile e la costruzione della scalinata di sud-est. La domanda è resa legittima dal diverso sistema di fondazione osservato nella muratura, però se osserviamo bene proprio questa muratura dobbiamo scartare l’ipotesi di due tempi. Abbiamo infatti osservato che i ricorsi delle pietre della facciata meridionale della balconata non tradiscono nessun taglio, o interruzione fra le due parti e che anzi il fenomeno dei ricorsi assottigliantisi da est verso ovest si riscontra in tutta la sua lunghezza. Per quanto riguarda la sinagoga, è possibile seguire le diverse fasi di costruzione delle singole componenti interne. Le fondazioni delle due strutture M e N nel lato sud sono anteriori alla posa del pavimento che è addossato contro le medesime. Il pavimento a sua volta fu messo prima che le pareti venissero intonacate: difatti nel lato nord l’intonaco è sovrapposto ai lastroni. I sedili murari finalmente sono posteriori, sia alla posa del pavimento, sia all’intonaco: difatti l’intonaco scende in basso dietro i sedili e i lastroni con le loro malte di posa si spingono leggermente sotto le bancate” (Corbo 1975, p. 167).

This evaluation of the strictly archaeological data, with which I concur completely, brings us to the conclusion that the synagogue not only is a project which was a single unit from the beginning, but was also effectively “inaugurated” after all its component parts were already completed: prayer room, eastern courtyard, side-room on the north, entrance balcony on the south. In other words it is inconceivable that there was a prayer room already func-

tioning before the construction of the eastern courtyard and – obviously – before the construction of the entrance balcony. Strictly speaking, it is not a priori excluded that the setting of the stone floor of the prayer room could be assigned to the final phase of the project provided that the study of all the coins and all the ceramics found could lead to this conclusion.

The increasing number of coins coming out of different stratigraphic contexts has a non negligible weight for anyone who is especially interested in numismatics. This aspect will be emphasized by Dr. Arslan. The undersigned wants to stress how the reading, whether preliminary or partial that Dr. Arslan has done on these coins since 1995, has partially changed the general picture which was possible to trace on the basis of the publication by A. Spijkermann going back to 1970. This is normal in all excavations, but it will be worthwhile to trace again, briefly, the path of the research. Setting aside the coins found on the surface and decided by us to be unreliable, the 63 coins collected in the layer of mortar of Trench 2 were dated by Spijkermann to the 4th century A.D. The latest, probably from Valentinianus I or Gratianus were from 364-375 (Spijkermann 1970, p. 112 n. 57). Also coin n. 4 found in the upper part of stratum B in Trench 1 was not later than 353-360 A.D. (Spijkermann 1970, p. 106). Likewise the coins n. 114 and 115 from level B in Trench 4 were from the 4th century. On the basis of these data, the director of the excavation suggested the hypothesis that the construction of the prayer room was begun under the reign and perhaps with the financial aid of Julian the Apostate. The undersigned agreed with this dating. This is how I wrote in 1970: “Il mio giudizio personale, fondato solo sulla ceramica ritrovata e a prescindere dalle monete è questo: una datazione (della sinagoga) nel I e II secolo d.C. è assolutamente impossibile. Anche una datazione nella prima metà del III è da escludersi. Se avessimo scavato soltanto la trincea I, si poteva ancora rimanere incerti se datare la sinagoga nella seconda metà del III secolo piuttosto che nel IV secolo d.C. Al termine dei lavori non posso onestamente aggrapparmi neppure alla fine del III secolo” (Loffreda 1970, pp. 104-105).

As the excavation progressed, Virgilio Corbo felt himself authorized to write in 1975: “Siamo pienamente convinti che gli edifici della sinagoga furono iniziati, come minimo, verso la seconda metà del quarto secolo dopo Cristo e che il lavoro fu portato a termine, con la posa dei pavimenti, verso gli inizi della seconda metà del quinto secolo d.C. (Corbo 1975, p. 168). With these words, a fairly conspicuous space of time was implied between the beginning and the end of the work of construction of the synagogue.

After the recent identification of many other coins it seems that the initial date of the entire synagogue building (prayer room, eastern courtyard

and balcony) was not before the beginning of the 5th century, while the final date of the project is still kept at the last quarter of the 5th century. It is a fact that the coins of Leo and Marcianus now are known to be present not only in Trench 12 of the eastern courtyard, but also in the mortar of the side benches and of the stone pavement of the prayer room, as well as in the fill of Trench 14 (western aisle) and Trench 18 (balcony). Also leading us to this conclusion is a coin of Honorius (395-401 A.D.) from level A in Trench 18.

The excavations of the Capharnaum synagogue have been followed with lively interest because of the findings there, which Levine (1981, p. 10) describes as “truly revolutionary”. He says: “The Franciscan archaeologists who excavated the site claim that the monumental synagogue there was not built in the second-third centuries CE, as hitherto assumed, but rather in the fifth century” (ibidem).

Many archaeologists both Israeli and from abroad repeatedly visited our excavation and were able to observe *de visu* the progress of our research and discoveries. We ourselves prepared ourselves to advise in timely fashion the Department of Antiquities concerning our new discoveries. To give an example, when on July 6, 1972 we found the first 6,000 coins in level C of Trench 12, we immediately telephoned Dr. Biran, the director of the Department of Antiquities. The next day we were honored with the visit of many Israeli archaeologists among whom were Yadin, Dothan, Negev, Foerster and various others. Dr. Foerster at that time was the inspector of the northern region and was particularly interested in our excavation because he was writing his doctoral thesis precisely on the synagogues. He honored us with many visits.

Our late chronology of the monumental synagogue could not help but arouse strong opposition on the part of those who still defended the traditional dating and assigned the construction of the synagogue not beyond the 3rd century. That reaction is fully understandable: the synagogue of Capharnaum in fact is “the queen of the Galilean synagogues” and it is well known that a queen never comes out alone from a beehive. Dr. Tzafrir, who at present is the most fervent supporter of the traditional theory and the most zealous opponent of our late chronology, wrote: “If Capernaum were late, all the other synagogues would have been late, since Capernaum is the largest, most lavish, and considered by many as one of the earliest and most formative synagogues of the early type” (Tzafrir 1995, p. 153).

Personally I think that these discussions (at times very lively) which fascinated people for almost thirty years, were not useless for the progress of knowledge. At least, the excavations of the Capharnaum synagogue opened a phase of feverish research through systematic excavations of many other synagogues.

To the architectural, stylistic and historical considerations on which the theories concerning the evolution and dating of synagogues were based in the past (Foerster 1971, p. 209), archaeological data are by now added: “The time has come to dig more and to theorize less”, I wrote with typical youthful boldness (Loffreda 1972, p. 27).

Let us see how the traditionalists would interpret the archaeological data of our excavation.

The Coins of level C

Dr. Avi Yonah explains in this way the presence of thousands of late Roman coins which we found in the mortar of level C: “These hoards show that the synagogue officials (for such hoards were most probably not private) were afraid of attack and ruin. To anyone familiar with Jewish history in fifth-century Palestine, such fears were only too well-founded. The fact that the caches were never recovered is clear evidence that all those who knew of their location either perished or were driven away” (Avi Yonah 1973, p. 44).

My answer: if things did develop in this way, it should seriously be considered that the chiefs of the synagogue lost, if not their own lives, certainly their heads. Was it truly a case of making topsy-turvy a sizeable part of the valuable floor of the prayer room, of the eastern courtyard and of the entrance balcony, simply to hide those hoards of bronze coins of modest value? After all, it would have been sufficient to make a hole or at least to remove one single stone to hide those coins. Furthermore, the coins which are hidden to be recovered later on are not scattered in a large area as grain is sewn and even less to insert them in fresh mortar!

My explanation is that these coins were used to redeem *ma' aser sheni* as Zvi Ilan has suggested in regard to the many coins he found in the synagogue of Meroth (Ilan 1989, p. 28). Here is what he wrote: “Jewish law requires that *ma' aser sheni* (the second tithe), approximately 9% of certain crops, be eaten in Jerusalem. It is permissible to transfer (redeem) the value of the crops, carry that coin to Jerusalem, and purchase food and drink, for consumption in the Holy City. In either case, *ma' aser sheni* could only be eaten in Jerusalem while the Temple stood. After the destruction of the Temple in 74 C.E., crops still had to be redeemed before they could be eaten. Jewish law at this time allowed for the symbolic redemption of large amount of crops with coins of little value. While it was impossible to redeem those coins since the Temple no longer existed, the coins retained a holy status and could not be used for any purpose. Jewish law therefore required that they be destroyed. In practice, since ruling

authorities forbade the destruction of coins other methods of disposing of the coins had to be found. Perhaps the coins underneath Meroth's floor were *ma'aser sheni* coins which were forbidden for use. They may have been collected elsewhere, over a period of many years and when the synagogue was built they were brought there. This would explain the exceptionally large number of coins we found. Unfortunately, this theory is also problematic, since finds in Chorazin and Rimmon include gold coins, which could not be used to redeem *ma'aser sheni*" (ibidem, p. 28). In the synagogue of Capharnaum all the coins of this type are bronze. We too found, it is true, some coins of gold behind the bench of the prayer room at Capharnaum, but these were hidden when the synagogue was already abandoned.

Dr. Foerster, speaking about the coins we found in the open levels of stratum C states: "Since the floor slabs were missing, it is impossible to tell when the coins were left (Foerster 1971, p. 208).

My answer: The observation is valid for those coins we found in the loose dirt on the surface, coins which we ourselves registered as untrustworthy for dating the pavement. On the other hand, for the coins with clear encrustation of mortar, it is not difficult to give a precise answer: they were abandoned when the mortar was still fresh, that is during the setting of the stone pavement of the synagogue.

And this is what Dr. Tzafrir concedes when he writes: "There can be no doubt that the excavators were right in stating that the synagogue pavements, as well as the cement bedding and the upper fill that included many limestone chips from the pavers, date to the end of the 4th century and in places even from the 5th century" (Tzafrir 1995, p. 155). I would like to add that the coins with traces of fresh cement are useful for dating not only the setting of the stone pavement, but also of the side benches of the prayer room. It is a detail which escaped Dr. Tzafrir.

Coins from level B

The presence of late Roman coins in the fill of level B creates a more serious difficulty for the traditionalists. Already in 1973 Avi Yonah minimized the importance of these coins in this way: "In order to be truly impressive, numismatic evidence must be sufficient in quantity; we should be wary of drawing too hasty conclusions from finding one coin here, two coins there, and so on. Loffreda's dating is based on four stray coins" (Avi Yonah 1973, p. 44). And he adds three observations: 1. "It must be remembered that coins are small and slippery objects"; 2. "No sealing in an ancient site was ever

‘hermetic’ in the physical sense”; 3. “We should avoid staring, as if hypnotised, at the five-meter squares of a locus and basing all our arguments on the finds in such a small area. This method, favored by many of the younger and less experienced archaeologists - and even some older ones, has led only too often to hasty conclusions, which later must be revised when the excavation area is enlarged” (ibidem).

Alas, the difficulties increased precisely when we enlarged the area of excavation. At present the coins coming from stratum B of the fill are not only “four stray coins,” but eight hundred ninety four. Dr. Tzafrir was sufficiently informed about the increased number of coins coming from stratum B, when he wrote: “The crucial archaeological evidence is the large number of 4th-5th coins found in the fill underneath the pavement of the synagogue, the courtyard and the platform in front of the entrance” (Tzafrir 1995, p. 153). But in practice he believes himself authorized to bring into discussion “only those coins whose stratigraphic context is clear from the information provided by the excavators and by A. Spijkermann, who was until his death responsible for the coins” (Tzafrir 1995, p. 154). On the basis of this principle, really questionable, he goes on to affirm that “from stratum B, the thick layer of fill below the pavement, there are very few coins of the second half of the 4th century. From the fill under the whole of the synagogue proper, I count no more than 2 coins whose stratigraphic context is beyond doubt” (Tzafrir 1995, pp. 155-156). I hope that this article of mine serves as a means of updating. Still less comprehensible is the rhetorical question which Tzafrir makes: “How could it happen that all the coins which were found in the lower layers of fill B are of the earlier periods, not later than the 3rd century, while 4th century coins were found only in the upper part?” (Tzafrir 1995, p. 156). I would remind Dr. Tzafrir that we recorded hundreds of late Roman coins in the whole depth of the fill.

Here is the explanation which, according to Tzafrir, the traditionalists offer for the late Roman coins found in stratum B: “The proponents of the traditional chronological classification have explained the discovery of late coins beneath the pavement (which according to them had been put down many years before the coins were minted) by *the assumption* that *the floor was renovated* at a late date. By this interpretation the synagogue was partially destroyed perhaps in the earthquake of 363. It would then have lain in ruins for a period, and at that time *the upper part of the fill* beneath the floor was disturbed. Then a new pavement was put down not before the early 5th century, and in the courtyard the latest repairs dated after 474. For such an hypothesis no archaeological proof has come from the site, although similar phenomena are attested elsewhere” (Tzafrir 1995, p. 153).

I have underlined some key expressions from this discourse and I would like to offer some clarifications and comment. The earthquake of 363 is a fact of history passed down by literary sources. It caused serious destruction in a large number of cities among them Tiberias. For some time now I have been collecting archaeological data to establish if there were any changes in the strata at Capharnaum which can be ascertained that were caused by that earthquake (Loffreda 1982, p. 426). I will indeed be happy if I arrive at any positive conclusions.

What disturbs me is not so much that recourse of the traditionalists to the earthquake of 363, as much as their effort to minimize and absolutely withhold objective data of our excavations. Already in 1975 Virgilio Corbo wrote on the subject: “Per essere coerenti, i sostenitori di questa teoria dovrebbero ammettere che non soltanto il pavimento in pietra, ma tutta la sinagoga fu ‘ricostruita’ nel tardo periodo romano: muri maestri, stilobati, balconata, bancate murarie, scalinate di accesso, ecc. E’ davvero verosimile che in questa ipotetica ricostruzione si sia sentito il bisogno di ‘rinnovare’ anche la riempitura della piattaforma?” (Corbo 1975, p. 164).

The earthquake of 363 was taken into consideration by other scholars also. Kenneth Russell included Capharnaum among the sites “with unrecognised 363 destruction” (Russell 1980, p. 56), but he reached a conclusion opposed to that of the traditionalists and wrote: “The author thus accepts the late chronology proposed for the synagogue of Capernaum, but would further suggest that its construction occurred after the site suffered damage during the earthquake of 363” (Russell 1980, p. 57).

Hanswulf Bloedhorn also has recourse to the earthquake of 363 and suggests a way to reconcile the position of the traditionalists and that of the excavators. He recognizes that “the stratigraphy of the existing structure of the synagogue allows it to be dated unquestionably to the late 4th century. However, since the decorative elements are undoubtedly older, they must be dealt with separately” (Bloedhorn 1989, p. 49). And he adds the following conclusion: “For the most part, the architectural ornamentation stems from the second half of the 3rd century, at which time the imposing basalt foundations were erected atop an older residential installation. It seems reasonable to assume a connection but unequivocal evidence is lacking. At the close of the 4th century, following the earthquake of 363 CE, a new synagogue was erected on the same foundations, incorporating the remaining ornamental elements. The courtyard was added in the 5th century” (Bloedhorn 1989, p. 52).

This point of view of his seeks to conciliate the archaeological data and the stylistic considerations. I am a ceramologist and I do not feel qualified

to make a judgement about the style of capitals and other aspects of decoration of the white synagogue. Only one time did I allow myself to cite a study of Avi Yonah which seemed to me to favor our late chronology. Actually, while speaking about capitals found in the synagogues of Galilee, Dr. Avi Yonah wrote: "The capitals are mostly of the Corinthian type, but they deviate strongly from the classical type, especially owing to the usual absence of the chalices and the inner spirals. The formation of the acanthus leaves on these Corinthian capitals is a particular interest for the history of Jewish art, because they antedate by at least two centuries the typical Byzantine capitals; in fact, if we did not know the approximate date of these synagogues, we would assign them, on the basis of their architectural decoration, to the Byzantine period" (Avi Yonah 1961, col. 165). This reasoning leaves me perplexed. I ask myself: How can Dr. Avi Yonah date these Galilean synagogues to the 2nd-3rd centuries on the basis of stylistic considerations, if he writes that the style itself could assign these synagogues to the Byzantine period? Something is not right with this reasoning, according to me. Dr. Avi Yonah solves the problem and answers that those statements are to be considered as "obiter dicta" (Avi Yonah 1973, p. 43). Frankly, this answer does not calm me down really and my comment behind my back is "obiter dicta sed dicta".

Today the need is felt to return to the argument of these capitals and both Bloedhorn (1988) and Fisher, in their doctoral theses reach the conclusion that these can be dated to the 3rd century, even though they can refer to different subtypes. There is, however a difference between the two writers: while for Fisher all the capitals of the Capharnaum synagogue can be dated to the 3rd century, for Bloedhorn it would be with the exception of the capitals of the eastern courtyard: "In the 5th century a courtyard complex was built onto the side of the synagogue. Following the line of the street, its plan was a trapezium. Four capitals have been found in the courtyard; one column capital, one corner capital, one pilaster capital, and a capital with the Menorah on one side. Their stone mass is decorated predominantly with densely arranged rows of acanthus leaves. The core of the capitals is flanked at the sides by narrow bands of volutes and bordered at the top by a thin abacus. A comparable capital of the 5th century is to be found in the Church on Mount" (1988, p. 52).

It seems to me that I myself may have been the one to provoke, indirectly and involuntarily, the study of Bloedhorn through one of my articles published in 1984. I include the following section: "La interpretazione personale dei ritrovati è questa: agli inizi dell'era cristiana fu costruita a Cafarnao una modesta sinagoga di cui conosciamo per ora

un lungo tratto di pavimento in acciottolato di basalto. Questa sinagoga non oltrepassava i limiti della navata centrale della sinagoga bianca ed era affiancata da abitazioni private. Probabilmente nel terzo secolo, sia la sinagoga del primo secolo che le abitazioni private furono smantellate per dar luogo ad una seconda sinagoga, di cui si è preservato il massiccio muro di basalto. L'area dell'edificio sacro fu praticamente raddoppiata, e coincide sostanzialmente con la sala di preghiera della sinagoga bianca. La terza sinagoga, questa volta costruita in pietra bianca, fu impostata nel tardo quarto secolo sui muri cimati della sinagoga del periodo romano medio e fu ulteriormente ampliata verso est con l'aggiunta di uno spazioso cortile a triportico" (Loffreda 1984, p. 109). Also in one of my guides to Capharnaum I wrote: "What is the relation between the 'basalt stone wall' and the first century stone pavement found in the central nave? The director of the excavations believes that both elements belong to the first century synagogue. Another possibility is in my opinion that the 'basalt stone wall' constitutes an intermediate stage between the first century synagogue and the white synagogue of the late fourth century A.D." (Loffreda 1993, p. 49).

Is it possible to attribute to this "basalt wall" the architectural elements which then show up in the white synagogue? This is what Dr. Bloedhorn suggests, not without hesitation when he writes: "It seems reasonable to assume a connection, but unequivocal evidence is lacking" (Bloedhorn 1989, p. 52). My point of view can be summarized in these words: 1) The "basalt wall" is later than the first century stone pavement found in the central nave of the prayer room. At the same time it is not a simple foundation of the white synagogue. 2. The "basalt wall" is based over the floors of a series of structures which remained in use until the 4th century to judge from the ceramics and some coins. Therefore even assigning a date of the 3rd century to it creates problems. 3. The "basalt wall" was probably built in view of a synagogue which was never completed, maybe because of the earthquake of 363 or some other earthquake of the 4th century. In this case however it must be admitted that a few coins of the 4th century, which we attributed during the excavation, to level A of the occupation, penetrated into this context from the fill. 4. If it is in vain to look for the pavement of this intermediate synagogue at the level of the foundation of the "basalt wall" (as Virgilio Corbo suggests), it is just as vain to look for it at the preserved summit of that same "basalt wall" because the pavement never existed. If it had existed at that height, we would have found some trace and above all we would not have so easily found late Roman coins in the whole depth of the podium of the white synagogue.

Coins from stratum A

They are few, just as relatively few are the coins which we found in the private dwellings of the city, but they exist and cannot be ignored. Already in 1973 I felt compelled to intervene in this matter and I wrote: "Foerster also states that 'only one fourth century coin is attributed to the fill below the sealed floor'. As a matter of fact, the coin was found in the occupational level of a house destroyed before the construction of the synagogue, and does not belong to the fill at all" (Loffreda 1973, p. 42). That coin, found in stratum A in Trench 2, was from 341-346 A.D. (Spijkermann 1970, p. 109, n. 26), and provides the terminus post quem for the end of that stratum. Obviously we, the excavators have been the first to underline that stratum A had not been hermetically sealed and that therefore a coin assigned by us "in good faith" to that stratum could have penetrated itself from the fill B of the podium and that only through our oversight or error it would have been attributed to stratum A.

In regard to the small amount of coins assigned by us to stratum A, Dr. Tzafrir writes: "In order to refute my criticism from reviewers that later coins might have penetrated into the bedding layer A and the fill (B) during the hypothetical period of repair the excavators emphasized that such later finds were found not only in layer B and C, but that 4th century coins were found in the settlement (A) predating the synagogue. Thus the second half of the 4th century is not only the date of the building but the terminus post quem of its construction" (Tzafrir 1995, p. 154). He adds: "In their later work it became clear that this interpretation was wrong. The buildings of stratum A were then correctly redated by the excavators to the Hellenistic and early Roman periods" (*ibidem*).

I am sorry that I must contradict Dr. Tzafrir once more. We plead with Dr. Tzafrir to not confuse the initial date and the final date of stratum A. In regard to the initial date, there has been no step backward on our part. We even also make note that even before the excavation of Trench 24 we found in Trench 21 not only remnants of the Early Roman period but in addition from the Bronze Age! Now the terminus post quem for the construction of the monumental synagogue is not dated obviously from the Bronze Age remains and not even from the remains of the Hellenistic period and from the early Roman period, but from the latest coins and from the latest potsherds found in stratum A. Let us make an example: in Trench 2 we found two superimposed stone pavements, at a depth respectively of m 1.10 and m 0.90. We found a coin of 119/120 A.D. in the lower stone pavement (Spijkerman 1970, p. 113, n. 96) and a coin of 341-346 A.D. in the upper

stone pavement of the same stratum A (Spijkerman 1970, p. 109, n. 26). Which of these two coins provide the terminus post quem for the construction of the white synagogue? Why did Dr. Tzafrir give so much importance to that coin of 119/120 A.D.? Why did he assign it to the fill of stratum B (Tzafrir 1995, p. 154)? What should we do with the other coin of the fourth century?

As for Trench 24, Dr. Tzafrir stresses our initial date of level A in the Hellenistic and Early Roman period, but then forgets that the pottery found in the upper pavement “covers the complete middle Roman period (135-300 A.D.) with rare examples which can be dated either to the late 1st century or to the 4th” (Loffreda 1982, p. 289, group D).

Dr. Tzafrir writes that “despite the accumulation of new data, the excavators’ interpretation of the history and chronology of the synagogue has not changed”. To be sure, our chronology did change, but not in the direction desired by Tzafrir and – it can be said frankly – neither in the direction desired by the undersigned. Personally I would actually have preferred, on the basis of the study of the ceramics, that the prayer room was built and terminated already in the 4th century, while the coins of the late 5th century in the eastern courtyard do not surprise me at all since they are accompanied by fifth century terra sigillata plates and by local ware of the same period.

4. Conclusions

Did we reach a dead end after so many years of discussions? Not at all. Far reaching conclusions have been attained in the last decades of excavations and studies. Already in 1973 Dr. Avi Yonah, while confuting my innovative theories, would write with open mind: “It is too early to accept the proposed ‘late chronology’ for the synagogue of Capernaum. Perhaps we ought to await further developments at Capernaum and elsewhere” (Avi Yonah 1973, p. 45). Dr. Foerster also, who at first expected nothing new from our excavations in the synagogue, ended his article/review hoping for new research both at Capharnaum and elsewhere (Foerster 1971, p. 209), and this was certainly a right step in the right direction. In 1996 Dr. Tzafrir could write some statements which would have been interpreted as nonsense and blasphemy two decades ago: “The emergence of new data has made the classification of synagogues more difficult”... “since the above typology was proposed (early type, later type of synagogues), work on synagogues has advanced and many of its points are no longer accepted”...

“It has become clear that different kinds of synagogues were in use and even being built at about the same time”... “Even scholars who accept the above typology are unable to point to a single building of the ‘early type’ dated by archaeological means to the 2nd c.”... “It seems that the above mentioned ‘Galilean synagogues’ should be dated to the 3rd and first half of the 4th c.”... “It has become clear that not only the chronological factor but also local tradition of masonry and the quality of materials available in each region influenced the form of the synagogues”... “The decoration of Capernaum does belong to the artistic milieu of the 3rd or, at least, early 4th c., rather than the second half of the 4th c. or later” etc. (Tzafrir 1995, pp. 152, 157). The last statement is a step forward. But, alas, exactly at the time when it seemed actually a certain coming together between our ‘late chronology’ and the position of the traditionalists, the numismatists arrived to remind both sides that 5th century coins are present not only in the eastern courtyard, nor only in the balcony, but also in the fill of the prayer hall of Capharnaum!

Dr. Tzafrir suggests that the late chronology of the synagogue of Capharnaum “based on a large number of late Roman coins ... creates the wrong impression (Tzafrir 1995, p. 155, n. 18). Again I would insist on the point that the archaeological weight of those 24,000 coins should not be assessed on the basis of their kilos, but on the fact that they were found in various and clear stratigraphic contexts.

Since today still, neither I nor the traditionalists are able to find a convincing meeting point between the archaeological data and the considerations coming from the stylistic viewpoint, that does not authorize us *ipso facto* to minimize the arguments of the other side, but to be aware humbly of our common ignorance and to continue onward with research. If it is true that “there is a time to throw stones and a time to gather them together” (Qoheleth 3,5), we all will be better off to spend less time in throwing stones at each other and instead to “gather stones,” i.e. new data. For my part, I can make known that the study of the material of the excavations at Capharnaum still continues at a steady pace. For some time I have been preparing a huge graphic and photographic documentation of the ceramics and various other objects found during nineteen seasons of excavation (1968-1986). I still have many things to clear up for myself and in all honesty I do not know how much time I need for this study until its final publication. The numismatists, Drs. Ermanno Arslan and Bruno Callegger, on their part, have done a gigantic work, by studying the coins which we found in the excavation of the city as well as the greater number of coins found in the synagogue. The article which Dr. Arslan published in this issue of

the *LA* is only one aspect of his serious and challenging study. He informed me that we still need three or four years to reach a definitive form for the material already scheduled. In any case a good portion of the catalogue has already been consigned to me and on the basis of this catalogue I have written the present article.

What is important is to offer to the readers an abundant documentation of the excavations. That is what remains forever, while our interpretations can be modified or even fade in the background.

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