THE BYZANTINE REVIEW

ByzRev 06.2024.057
doi: 10.17879/byzrev-2024-5819

FABIAN STROTH, The Church of St. Polyeuktos at Constantinople (Elements in the History of Constantinople). Cambridge: Cambridge University Press 2024. 4, 82 pp. DOI: 10.1017/9781009105729. – ISBNs: 978-1-009-51706-5 (hb. £ 49.99, 64.99 $; 67.76 €); 978-1-009-10132-5 (pb.; £ 17.00; 22,00 $; 21,23 €); 978-1-009-10572-9 Online-Campus); Kindle edition £ 13.60 (22,00 $; 14,86 €).

• GEORGIOS MAKRIS, University of Münster
(pm56247@googlemail.com)

The Church of St. Polyeuktos at Constantinople by FABIAN STROTH is the newest addition to the *Elements in the History of Constantinople*. The series debuted in 2021 with *The Hippodrome of Constantinople* by ENGIN AKYÜREK and *The Statues of Constantinople* by ALBRECHT BERGER. The worthy goal is to present the history of the metropolis on the Bosporus, from its foundation to modern day, through concise monographs written by leading scholars, focusing on one outstanding monument at a time.
STROTH's qualifications for this project seem to be based on one study of 6th-century capitals with monograms. A portion of this study, which focuses on Hagios Polyeuktos[1] and the reconstruction of the exedrae (STROTH 2011, pp. 95–119,) is repeated in the new book, shifting the focus from the church itself to a secondary topic. Besides, this duplication raises the question to why it was done.

In the abstract (actually a blurb) and, once again, in the book itself (p. 6,) STROTH emphasizes, "in the end, the study of St. Polyeuktos will tell us as much about Byzantine architectural history in the second half of the twentieth century as about early Byzantine architecture itself." This is indeed what the reader gets up to a certain point. The study was made possible by using data from the Oxford Archives of MARTIN HARRISON, who directed the excavation of the church from 1964 to1969, and the Archives at Dumbarton Oaks, the research center that funded and supervised the excavation together with the Istanbul Archaeology Museums. However, this approach deviates from the series' original concept and seems to be the author's idiosyncratic choice rather than a necessity. Moreover, it is surprising that STROTH ignores recent archaeological work on the church,

1. In the review at hand, "Hagios Polyeuktos" refers to the church and "Saint Polyeuktos" to the patron saint.

including the excavation that began in June 2022 as part of the Saraçhane-Arkeoloji-Parkı project, which aims to redesign the site under the supervision of the Istanbul Metropolitan Municipality (see, e.g., ALTAN –ERCAN KYDONAKIS 2016; pp. 14–15[2]; reports and pictures can also be found on the common social media sites). Furthermore, the absence of an examination of Turkish archives is also perplexing.

Despite the book's overall structure, this review will evaluate the subject and point out some shortcomings in the content as well as in the methods. Let us first consider the following two examples.

1. The so-called *pilastri acritani*, the marble piers, which were removed from Hagios Polyeuktos by the Venetians after 1204 and have since been relocated outside the church of San Marco, were previously believed to have originated from Acre. These piers carry variants of a Greek monogram (Fig. 1), which over the centuries was either considered indecipherable or read in rather bizarre ways.

Figure 1. Monogram on one of the *pilastri acritani*, Venice (Photo: Reviewer)

2. Henceforth, underlining will indicate the respective content can easily be accesssed online.

However, recent research (GARIPZANOV 2018, pp. 160–167) has suggested that the monogram should be read in genitive form as ΑΓΙΟΥ ΠΟΛΥΕΥΚΤΟΥ (= of, or, rather, dedicated to Saint Polyeuktos.) Of this suggestion is true that the name of Polyeuktos can be read in the monogram in genitive form–and nothing else. Imposing the term for "saint" is erroneous; there is neither a single stroke beyond what is required to write ΠΟΛΥΕΥΚΤΟΥ nor any combination of strokes that would allow for the reading of an A as in *ΑΓΙΟΥ.

Additionally, no composite term of the kind "Saint Polyeuktos" or "Saint X" occurs in the dedicatory epigrams of Hagios Polyeuktos and the ones that are transmitted in Book I of the *Anthologia Palatina*, respectively. Apparently, the composite terms were considered vernacular and avoided in high-level texts then.[3] However, while it is difficult at first sight to understand how the partly incorrect reading ΑΓΙΟΥ ΠΟΛΥΕΥΚΤΟΥ was derived, STROTH informs (p. 43) that it had previously been suggested by MARTIN HARRISON in a letter preserved in his Oxford Archives in 1965, which was never published. HARRISON's work as a field archaeologist and compiler of excavation reports was quite precise, but had no focus in philological issues; thus, it is all the more commendable that he came close to the appropriate solution. A deeper insight into the issue could be expected from STROTH, at least if one takes into account his above-mentioned preoccupation with monograms. Yet, he adopts the half right and half wrong reading without further ado.

With regard to the numerous other monograms found on the entablatures of Hagios Polyeuktos, STROTH repeats the stereotype associated with them, namely, "the name Anicia Juliana ... is not among them." But why should it be? The ineffectuality of this postulation, on the one hand, and the presence of the name of the honored Saint both in several monograms on the entablatures and the *pilastri acritani*, on the other hand, call the alleged reference to Juliana into question. At the same time, this indicates that the monograms were mainly, if not exclusively, intended to honor Saint Polyeuktos. STROTH's reliance on previous research for the monograms, without considering alternative explanations, is a notable flaw in the book.

2. The generally accepted reconstruction of Hagios Polyeuktos is a wooden-roof-covered five-nave basilica, the only one compliant with archaeological findings and at least partly with the course of the famous dedicatory in-

3. The composite term occurs several times in the captions of the epigrams, which were, however, added in the late 9th century.

scription that ran around the naos. This reconstruction has been gradually delineated mainly in papers by the British archaeologist JONATHAN BARDILL, who is working on a comprehensive study of the church. STROTH shows BARDILL's reconstruction (ground plan and cross section) of the church in two small drawings, each about one-fifteenth of a page in size, whereas he repeatedly shows reconstructions of the exedrae and entablatures of the church in considerably larger drawings (e.g., p. 35; p. 52; p. 53) as well as reconstructions of other churches borrowed from his own book mentioned above. STROTH rightly rejects HARRISON's ideas that the church had a dome, was a model for Hagia Sophia, and was an attempt to evoke the Temple of Salomon. The research report is detailed, well documented, well structured, and representative, considering both the archived material and relevant literature (p. 45 ff.; 55 ff.). However, this background information on the rejection of obsolete ideas did not need to be widely presented in a book, especially in a small one. The obsolescence of HARRISON's reconstruction is evident from the removal of the image of Hagios Polyeuktos as a domed basilica from reputable sites.

The Church of St. Polyeuktos at Constantinople begins with the accidental discovery in Istanbul in 1960 of blocks of marble carrying fragments of a Greek text, identified as parts of the dedicatory epigram[4] of Hagios Polyeuktos by Ihor ŠEVČENKO first in a note and then in an article co-authored with CYRIL MANGO (ŠEVČENKO 1960 and MANGO – ŠEVČENKO 1961, respectively). STROTH refers to this well-known account as an anecdote and claims, "In fact, the discovery had its starting points some 350 years earlier" in Heidelberg, as it was here, in 1606, that the French scholar Claude Saumaise came across the manuscript of the *Anthologia Palatina*, which transmits the dedicatory epigram. However, Saumaise took no particular notice of the epigram; neither he nor Heidelberg played a significant role in developing the idea of Hagios Polyeuktos. This idea received a shape in the 1660s, when the court historiographer and librarian in Vienna, Peter Lambeck, included a treatise on Juliana Anicia in his description of the splendid manuscript known as *Vienna Dioscurides*, which has been widely reprinted over the centuries). STROTH's statement about Saumaise and Heidelberg lacks substance.

The full story of the rediscovery of Hagios Polyeukto is underexposed and

4. For the sake of brevity, the singular is used here for the two poems transmitted as *Anthologia Palatina* I 10. Reference is always made to the verses of the first poem (*Anthologia Palatina* I 10,1–41).

complex. The first scholar to mention the findings in 1960 was FERIDUN DIRIMTEKIN, then director of the Ayasofya Museum (DIRIMTEKIN 1960). He associated the site at Saraçhane with Juliana's father, Olybrius Anicius, and the area owned by him in Constantinople. MANGO and ŠEVČENKO cited Dirmtrkin's article imputing to him the assumption that the findings "may have belonged to the palaces of Placidia or Eudoxia" (MANGO – ŠEVČENKO 1961, p. 247, n. 21,) but passing over in silence the fact that he ascribed them to the Anicii. STROTH inaccurately cites DIRIMTEKIN's article, omitting crucial details, probably without having closely reviewed it, if at all. He does not address why the excavation commission went to the Archaeological Museums of Istanbul and NEZIH FIRATLI instead of the Ayasofya Museum and FERIDUN DIRIMTEKIN; nor does he speak to what happened on the site between the discovery in 1960 and the delayed start of the excavation in 1964. One would expect this, particularly as STROTH prioritizes research history over the matter itself. He attempts to avoid debatable issues in a peremptory manner.

In 1966, the Metropolitan Museum of Art purchased a marble portrait bust from a New York gallery owned by a notorious antiquities dealer. The bust is now on display at Fifth Avenue, Gallery 301 on the ground floor (Fig. 2). The museum states that it was "possibly found [sic] in Istanbul" and "made in Constantinople."

Figure 2: Bust of an Aristocratic Lady, MET, New York (Photo: Reviewer)

Although an authorized catalog initially dated the bust to the first quarter of the 6th century based on scientific criteria (BRECKENRIDGE 1979) it is now officially dated to the late 300s-early 400s without providing any justification. In neutral terms, the bust is considered an unprovenanced artifact. It probably depicts Juliana, as identification with another aristocratic lady of Constantinople would be less likely. Originally, it may have been a half-scale statue that stood in front of Juliana's grave in the substructions of Hagios Polyeuktos. Over the centuries, it suffered damage, was buried under earthquake rubble, and could have been unearthed, cut down to the present bust, covered up, and smuggled out of Turkey between 1960 and 1964. STROTH, with an authoritative tone, dismisses the identification of the bust with Anicia Juliana, citing HARRISON's book, "*A Temple for Byzantium*," and an image of the bust in this (p. 7, n. 32): "A bust often associated with Anicia Juliana in MET Cloisters Collection, Inv. 66.25, does not depict the princess. Harrison, Temple for Byzantium, p. 39." However, the caption for the image in HARRISON's book reads "Marble bust of a woman, found in Istanbul ... it may be a portrait of Anicia Juliana" (Harrison 1989, p. 37) and, thus, contradicts STROTH's claim. This distorting

quotation, the avoidance of discussing the provenance of the marble bust, and the limited engagement with the issue are unjustifiable.

The subsequent case regarding the supposed influence of Sasanian art on the decoration of Hagios Polyeuktos exemplifies STROTH's handling of data and citation practices. He asserts that "scholars almost unanimously proclaim" a certain group of ornamental motifs as influenced by Sasanian art (p. 42). This is not entirely accurate. While the Sasanian influence is mentioned in secondary literature, it is often refuted in reliable publications for valid reasons. STROTH links the supposed influence to the "Orient or Rome" debate, citing a 2018 edited volume on the same topic and "Strzygowski 1904, 433."

The citation seems puzzling, as the only title by STRZYGOWSKI in the bibliography of *The Church of St. Polyeuktos at Constantinople* is a book from 1903. It is likely that the intended reference is p. 433 of STRZYGOWSKI 1902. Additionally, the volume on the "Orient or Rome" issue reveals STRZYGOWSKI's espousal of racial ideas, particularly in Byzantine art history, where he fabricated connections between Mediterranean works of art and cultures outside the Mediterranean. STROTH does not attempt to justify STRZYGOWSKI's ideas but fails to acknowledge the criticism of his theories. Scholarly rigorous publications dismiss them as a "house of cards" and irrelevant to later scholarship (NELSON 2010, p. 73). Others use, in a metaphorical sense, terms such as "anthropological delirium" and "madness" (LABRUSSE 2009 and CORMACK 2018, respectively).[5]
STROTH's handling of data and citation practices in the issue are off the mark, and his alleged rediscovery of STRZYGOWSKI's theories proves labor in vain.

But how had STRZYGOWSKI actually argued? The following remarks do not refer to the overarching issue of interactions between the Sasanian and late antique Eastern Roman cultures but rather to the origin of the theory of Sasanian influence on Hagios Polyeuktos that STROTH embraces and traces to STRZYGOWSKI.

STRZYGOWSKI famously claimed to have discovered precursors to Mediterranean works of art in regions outside the Mediterranean, such as Persia,

5. The literature on the subject has grown enormously since the 1980s oscillating between discarding Strzygowski's ideas altogether and, occasionally, holding them for visionary purposes. - Ernst Gombrich, who had been STRZYGOWSKI's student, took a romanticized view of him: STRZYGOWSKI was "not an anti-Semite or anything" on the one hand, and "not an uninteresting mind but a crank" on the other (WOODFIELD 2017, pp. 2–3).

Armenia, Central Asia, and elsewhere. He believed that these areas were free from the cultural contamination and degeneration that he believed had affected the Mediterranean region. In essence, STRZYGOWSKI proposed the idea of Sasanian influence based on his belief that Persian culture, in his misguided view, was primarily Aryan.

Like everyone else at the time, STRZYGOWSKIi considered the *pilastri acritani* to be trophies from Acre, but this is irrelevant to our issue. In his article, he first incoherently attested that the mosaics of the 5th and 6th centuries in Ravenna, as well as the Throne of Maximian, were representatives of Antiochian art. His only reason for this was that it seemed so to him (regardless as to how wrong this statement was then and now). After that, he described the *pilastri acritani* and projected in the monogram the name Pataikos of Antioch ("ΠΑΤΑΙΚΟΣ ΑΝΤΙΟΧΕΙΟΣ," pp. 430–432,) which is attested in the second century B.C. This was his only object-related argument, and it cannot be surpassed in terms of defectiveness since none of the necessary letters A, N, or X are graphically present to read the assumed forms on the monogram (Fig. 1). The rest of STRZYGOWSKI's train of thought is even more obscure: He declared the sculptural decoration of the *pilastri acritani*, which stands out due to its high-relief character, to be extremely flat; then, he declared the fantasized flatness, a pomegranate, and even a meander on the relief to be Syrian.

STRZYGOWSKI then drew a relationship of origin from the depiction of acanthus leaves on Sasanian reliefs of Tāq-e Bostān over Persian clothes down to the *pilastri acritani*, not realizing the claimed objects were common and completely suppressing the fundamental difference in the finish (dense fine chasing in the Persian reliefs and comparatively coarse surface in those of Hagios Polyeuktos). Moreover, he did not name a single specific Persian supposed model nor present a photo or any other illustration. In other words, STRZYGOWSKI, who is sometimes considered one of the fathers of comparative art history, broke all rules governing its methods. Additionally, both he and scholars who reproduced his ideas disregarded what is indispensable when making a comparison, i.e., the age of the Persian monuments. Recent research dates them a century later than Hagios Polyeuktos (e.g., COMPARETI 2019, pp. 19–20; cf. EFFENBERGER 2019, p. 159)–a parameter that in itself makes a mockery of the whole theory of Sasanian influence.

Finally, STRZYGOWSKI, with no factual basis, addressed undefined Mesopotamian-Iranian traditions, which, he wrote, that he would discuss else-

where. STROTH presents these non sequiturs as the origin of the "Sasanian explanation" (p. 42), which he erroneously imagines to have rediscovered, and even counterfactually claims the existence of scholarly consensus on it. It would have been more appropriate to quote the short and snappy designation of the supposed influence as a "phantom in research history," and its rejection as "a not entirely new insight, at least among experts" (DENNERT 2010, p. 199).

Beyond field archaeology, when it came to interpreting the remains of Hagios Polyeuktos, MARTIN HARRISON was obsessed with precursorism. He was familiar with STRZYGOWSKI's allegations about the *pilastri acritani*, and he expanded and transferred them to the entire Hagios Polyeuktos. However, in view of the conceptual background of STRZYGOWSKI's theories, he did not cite him by name but vaguely referenced ANDRÉ GRABAR instead, who had taken STRZYGOWSKI's theory at face value and reproduced it. Both GRABAR and HARRISON added photos of floral patterns from Persian reliefs and birds from Persian textiles as evidence. However, neither of them directly compared or contrasted a single Persian artifact with one from Hagios Polyeuktos. For example, HARRISON, alongside his depictions of peacocks, disregarded the fact that they were only commonplace and do not provide a sign of a lineage. Following GRABAR, he assumed that the well-known Persian silk-cloth pattern of a pair of female peacocks might indirectly be the model for sculptures from Hagios Polyeuktos showing pairs of male peacocks or single male peacocks fanning their trains (HARRISON 1989, p. 125, Fig. 166). However, this supposed relationship is out of proportion. Actually, the artistic concept of the peacocks of Hagios Polyeuktos contradicts the theory of Sasanian influence. The sculptural decoration of the church, while a product of its time, was singular in style. There is no data to support a specific formative influence, Sasanian or otherwise. STROTH's claim that "Sasanian stuccowork and the architectural ornament at Taq-e-Bostan in modern Iran provide the closest precursors for most of the material" is unsubstantiated. Additionally, as mentioned above, the supposed models are certainly more recent than Hagios Polyeuktos. In a conspicuously polemical tone, STROTH criticizes "a small school of German archaeologists" that includes CHRISTINE STRUBE, GUNNAR BRANDS, and RICHARD BRÜX. STROTH claims they reject the theory of Sasanian influence on the decoration of Hagios Polyeuktos without offering a more convincing explanation. Apart from the fact that STRUBE occasionally mentions an arrangement of motifs oriented toward Sasanian patterns (STRUBE 1984, p. 64,) STROTH's assertion

may confuse readers. He seriously suggests that scholars who reject the existence of Sasanian influence must vindicate its presence—a contradictory argument. Additionally, the alleged "school" is not small or predominantly German, as it also includes, e.g., MARTIN DENNERT and ARNE EFFENBERGER, as well as a wide range of non-Germans such as ROBIN CORMACK and ROBERT NELSON, to mention just two names per group. STROTH cites relevant publications of most of these scholars on several occasions, but fails to acknowledge their rejection of the theory of Sasanian influence. Overall, this is a strange strategy, if it is one, for avoiding contradictory evidence and gaining authority over issues.

Inaccurate citations, like the one mentioned previously of STRZYGOWSKI's article, may result from carelessness. However, in the book under review they recur. For instance, Fig. 28 shows three blocks of the Great Entablature with verses 30–32. The long caption reads: "Three block [sic] of the Great Entablature with verses 30-32 are matching (Stroth 2015 after Harrison, Excavations, 120 Fig. B)." However, STROTH's cited publication from 2015 is imaginary; no such article or book is included in the bibliography nor can it be located otherwise, which causes one to question whether, indeed, it was ever published. In addition, the figure cited from HARRISON's volume does not match STROTH's description. However, a similar juxtaposition of drawings of blocks can be found in a master's thesis by VENLA-EEVA KAKKO (KAKKO 2011, p. 94, Fig. 29, and p. 177).[6] STROTH cites the thesis several times without referencing specific illustrations and, thus, making it unclear how much information was taken from KAKKO's work. Other figures in *The Church of St. Polyeuktos at Constantinople* also share significant characteristics with KAKKO's illustrations.

STROTH introduces the sections on the reconstructions of the exedrae with details about the epigram in the church of Saints Sergius and Bacchus. Thereby, he repeats what he had written in his book from 2021 to immediately clarify, "in the church of St. Polyeuktos, the situation is completely different." This leaves the reader wondering even more intensely about the purpose of reusing one's own content. Additionally, the approaches to the epigrams are, each in their own way, partially mistaken.

In the epigram of the church of Saints Sergius and Bacchus, STROTH interprets "the new titles of the imperial couple as sceptered Justinian and

6. The reviewer is indebted to Mrs. Kakko who sent, upon request, a copy of the thesis in March 2019.

god-crowned Theodora," facing each other "exactly in the lateral axis of the church," as key points that summarize the central message of the dedicatory inscription. However, there are several errors in this interpretation. Firstly, laterality and transversality are not necessarily prevalent architectural properties of octagonal halls (the nave of the church of Saints Sergius and Bacchus is such a hall,) unlike elongated basilicas with decorated side walls and/or entablatures. Then, it is difficult to understand how two single words carved laterally on an octagonal nave could emphasize the significance of the transversal axis, especially if they are carved on a recessed surface between protruding piers, as is the case in the church of Saints Sergius and Bacchus. Regarding STROTH's claim about the "new titles," one would expect personal names or combinations of adjectives and personal names to face each other, ideally in a nominative manner. However, the inscription shows nominative vs. genitive (CKHΠTOYXOCIOYCTINIANOC, l. 3 of the epigram, vs. ΘEOCTEΦEOCΘEOΔΩPHC, l. 10,) and Justinian's name is broken at one corner (IOYCTINIANO|C,) which could be seen as disrespectful, if STROTH's assumption had any merit. In addition, the positioning of the adjectives does not necessarily emphasize the piety of Justinian and Theodora, as further explicit attributes in this regard are present in the text but not symmetrically placed on the inscription (EYCEBEIHN ... AEΞΩN, l. 3, and HCNOOCEYEBEIHΦAIΔPYNETAI, l. 11). Even the name of Christ is shown broken in one corner of the inscription (XPIC|TOIO, l. 7,) indicating that the position of individual words did not play the conceptual role assumed by STROTH.

The chapter on the reconstructions, with a primary focus on the reconstruction of the northern exedra (p. 44-45,) starts with research reports on relevant propositions by Harrison (a central dome and four exedras, two in the middle of the north side and two in the middle of the south side, with each exedra consisting of three carved blocks) and Bardill (five-nave basilica and six exedrae, three per side, consisting of three blocks and two half blocks each, with the middle exedrae flanked by arches). STROTH then states, "The block-wise distribution of the verses can be traced for all preserved structural elements of the two central exedrae." He declares this to be a rule, and proposes that the exedrae consisted of five analogous blocks each, while each block showed one verse of the epigram. Two arches showing two verses each flanked the exedra. This conclusion is compatible with the findings and essentially correct for the middle exedrae. Finally, he sees the elaborate relief and the distribution of the verses as evidence that "The

epigram was already carved on the ground before the blocks were installed."

The reconstruction of the exedrae is a highly complex subject that cannot be thoroughly discussed in a review. Let us, therefore, focus on how STROTH deals with it, starting with the latter topic. The conclusion that the inscription was carved on the ground is not necessarily wrong, but it is a non sequitur. Depending on the construction process, marble blocks could be carved on the floor, after they had been hoisted into their final position on columns and piers, or even in special workshops (in the cases of fine chiseled capitals and blocks of the corniches containing openwork monograms). However, if the blocks were parts of a supporting structure, it would often be better to install them first and finish them later so that the superstructure components above them could be placed and worked on without delay. In any case, carving can turn out either fine or coarse on the floor and on the scaffolding. Additionally, there is archaeological evidence disregarded by STROTH that in Hagios Polyeuktos, particularly elaborate reliefs were finished after the blocks had been installed (HARRISON 1986, p. 425 n. 32). There are further indications of this, such as the glyphs of the second half of verse 30, which are fitted closely at the outset but then stretched across the band (Fig. 3,) resulting in a discrepancy that would not have occurred if the marble block had been carved on the floor.

Figure 3: Fragment of the Entablature. Istanbul, Archaeological Museum (Photo: Reviewer)

There is a methodological issue in dealing with the relationship between the epigram and the inscription when using the verses to calculate the size

of the entablature and its parts. The verses vary in length, with some being significantly shorter than others. Some verses contain 39 or 40 letters, while others have 31 or 32 letters, making them one-quarter shorter. The difference can be even greater when considering the number of iotas in the verse, as they are narrower letters. For example, when inscribing verse 31, which is one of the shortest (thirty-one letters,) the last third of the strip had to be left blank. We are certain of this, even though the unlettered part of the block has deteriorated, because the beginning of the next verse, 32, is preserved on a different block. Therefore, it appears that for the central exedrae, the verses were distributed in a regular manner, which was done intentionally. However, this does not necessarily mean that the versifier and stonemasons primarily aimed to visualize the lyric rhythm through the distribution. The inscription was likely conceived to have an effect on church visitors through its ornamental disposition rather than its verbality.

Regarding the rule covering the distribution of the verses, STROTH mentions, "The only exception to this rule is a smaller fragment with remnants of verse 25/26 ... from the western part of the northern row of exedrae." This, nonetheless, should be ignored as "We have no other archaeological information about the exedrae placed there." However, STROTH fails to explain how he knows this exception to be the only one, or what makes it an exception, or what the fragment is smaller than (if he meant it in that way). Even a single exception to a rule based on the consistent distribution of verses would inherently break the rule. Furthermore, if there was an "only exception" it would be challenging for it to be singular, as it would eliminate the possibility of positioning adjacent verses regularly.

STROTH's outlined erroneous reasoning is accompanied by factual errors and some terminological confusion as indicated below:

1. STROTH mentions multiple exedrae located in the western part of the northern row. This, taken literally, implies there were at least five exedrae per row. Surprisingly, he reproduces ground plans proposed by HARRISON and BARDILL, from which he distances himself, but fails to provide his own ground plan or a top view image of the site. Thus, the key section of the book, i.e., the chapter on the reconstruction of the exedrae, leaves readers wondering about the number of exedrae the church may have had.

2. STROTH refers to the arches flanking the central exedrae as "niches."

3. STROTH mentions a "fragment with remnants of verse 25/26." However, there is no such fragment. In reality, only fragments from verses 25 and 27 exist.

These flaws make the chapter confusing for less informed readers. Finally, an overarching issue that STROTH does not address should be mentioned: The verses of the epigram are odd in number (forty-one). Thus, it is highly unlikely that they could have been engraved symmetrically in two rows.

In any case, the reconstruction of the exedrae of Hagios Polyeuktos remains an unresolved task.

The reference to the (non-existent) "fragment with remnants of verse 25/26" is also repeatedly encountered in KAKKO 2011, p. 36 ff. However, here the reference is not subject to a factual error but rather used to designate the whole block from which the fragment originates. The use of the same expression, implying that the erroneous reference in STROTH's book derives from the designation in KAKKO's thesis, thus touches again on the issue of STROTH giving narrow credit to it. The main points of the reconstruction presented by STROTH, namely the consideration of the verse meter in the entablature blocks, the five-part structure of the central exedra, and the formation of the arches flanking it, are also found in sufficient detail in KAKKO's thesis. STROTH mentions the thesis in his notes without ever making clear whether there is merely a thematic overlap with it or whether and to what extent he has used it as a source. However, it is particularly striking that he reviews the reconstruction proposals by HARRISON and BARDILL in detail only to discard them, but does not discuss KAKKO's work or the reconstruction proposals in it, even though they are present in his own book. He does not claim novelty for the reconstructions he proposes, at least not explicitly. However, he does not assign them to KAKKO either, neither explicitly nor implicitly, although they originate from her.

It follows from the outlined shortcomings that *The Church of St. Polyeuktos at Constantinople* has not been peer-reviewed. If this conclusion is incorrect, then only unsuitable and unreliable peer reviewers could have been in play. Furthermore, no serious copy-editing or editing seems to have taken place. As far as English is concerned, editors and the publisher have blundered and let down the author.

It demands effort from the reader to understand that the phrase "walls above the church's main body" refers to walls on the foundations of the church, not to walls above the nave. Likewise, the caption "These pillars originate from St. Polyeuktos, brought to the West after 1204" should not be interpreteted as meaning that Hagios Polyeuktos was relocated to the West. The statement that the complete text of the dedicatory epigrams "is also preserved in the Anthologia Palatina" confuses, as the text is preserved only

there. Lacking amplification, documentation, and citation, the statement that "Eric Ivison was able to clarify some points of chronology and to establish reliable dates for the use of the cemetery" socially pays tribute to a scholar but this is of little use to the reader. Phrases such as "... hypotheses, which assume," "... Paul Speck's disputed reflections on this epigram, which had argued for a reconstruction," and "... shape can also be reliably reconstructed by further fragments" indicate that no one meticulously evaluated the style.

Yet, even more disturbing, especially when attempting to use the book as a bibliographic guide, are formal faults. Here, too, it seems that no vigilant copyeditor, be it the author or a professional, has combed over the proofs. A small sample: "Pattern" instead of "Patterns"; "l' Hypogée des Dunes" instead of "l'hypogée des Dunes"; "Alle gentili art ammaestra. Festschrift Alkistis Proiou ..." instead of "«Alle gentili arti ammaestra». Studi in onore di Alkistis Proiou"; and "Festschrift Marina Righetti" instead of "Scritti di storia dell'arte per Marina Righetti." The volume "Convegno internazionale sul tema: La Persia nel medioevo" is quoted as if it were a journal called "Accademia Nazionale dei Lincei. Quaderno" (obviously a copy of the same error in HARRISON 1986, p. 426, n. 53, whereby it is most likely that neither HARRISON nor STROTH have consulted the volume.) The series "Byzantina Sorbonensia" is cited as if it were a journal called "Byzantia [sic] Sorbonensia," and dated "1984b". Alessandra Guigla Guidobaldi's double-barreled surname is cited as "A.G. Guidobaldi." Hugo Stadtmüller's edition of books I-VI of the *Anthologia Palatina* is cited as "Anthologia Graeca VI: Epigrammatum Cum Planudea," which is similar to the false title on the Amazon website (Incidentally, the bibliography lists more than one edition of the *Anthologia Palatina* with a German translation, but none with an English translation,) and finally, "Davut Pasha" is quoted "Davent Pasha".

As most of what is indicated in the paragraph above occurs on a single page of the book (p. 71,) attempting to list all the formal faults would be tedious and excessive. However, faults in authors' names, captions, titles, and citations are particularly annoying. For example, "Garpizanov" instead of "Garipzanov," "van Dienten" instead of "van Dieten," and errors in titles like "Inscribing Faith on late Antiquity" instead of "Inscribing Faith in Late Antiquity," "Johannis" instead of "Johannes," "Sts. Surges and Bacchus" instead of "Sts. Sergius and Bacchus," and "Notes d'epigraphie et d'archeologie Constantinople, Nicée" instead of " Notes d'épigraphie et d'archéologie: Constantinople, Nicée." These errors occur frequently in

references to non-English language publications and quotes. For instance, there are misspellings, such as "Ayasofia" and "Bakirköy," "Ένίκησά / ΄Ανικία," "οὐκ ἐλέφαςm," "justinainische," and inconsistent use of "ibid." instead of "id." Additionally, different abbreviated titles are used for the same study, such as "Justinians Sieg" and "Sieg über Salomon." There are also inconsistencies, such as the use of "*Antichità Altoadriatiche Aquileia e l'Africa*" as the uniform title of a journal, variations in place names such as Munich and München or Vienna and Wien, faux-pas in wordings such as "die Hagios Polyeuktos in Konstantinopel" (the original reads "die Polyeuktos-Kirche in Konstantinopel") and "der frühbyzantinischen Hagios Polyeuktos." Even *Excavations at Saraçhane* ..., the title of HARRISON's cutting-edge publication on Hagios Polyeuktos, is cited in the distorted form "Harrison, Excavator" (p. 53, n. 217.)

A strength of *The Church of St. Polyeuktos at Constantinople* is how the author effectively debunks clichés about the history of Hagios Polyeuktos and Constantinople. He argues convincingly against the church being a political statement from Juliana's family to Justinian, pointing out that the construction began during Anastasios' reign, not Justinian's. STROTH also skillfully dismantles legends about rivalry between Juliana and Justinian or about Justinian as the new Solomon, both common in flawed historical records and modern publications. He also critically argues against the idea that Hagios Polyeuktos was a prototype for Justinian's domed buildings and rejects theories about its design being based on biblical descriptions of temples.

Overall, STROTH's book falls short of providing a comprehensive contextualized account of the monument, let alone one with topical references, but offers valuable factual insights that challenge common misconceptions. It is hoped that these will serve as a counterbalance to the abundance of publications perpetuating clichés and unfounded interpretations of Hagios Polyeuktos.

References

Altan, Baris – Ayşe Ercan Kydonakis, "Byzantine Archaeology of Istanbul Through a Cultural Heritage Perspective : The Land Walls, the Church of St. Polyeuktos, the Theodosian Harbor," in: Toplumsal Tarih 275 (November 2016) pp. 10–15.

Asutay-Effenberger, Neslihan – Falko Daim (eds), Sasanidische Spuren in der byzantinischen, kaukasischen und islamischen Kunst und Kultur – Sasanian Elements in Byzantine, Caucasian and Islamic Art and Culture, Verlag des Römisch-Germanischen Zentralmuseums, Mainz 2019.

Breckenridge, James D., "Bust of a lady of rank," in: Weitzmann, Kurt (ed.), Age of Spirituality ... Catalogue of the exhibition at The Metropolitan Museum of Art, November 19, 1977, through February 12, 1978, MMA & Princeton University Press, New York 1979, No. 272 (also in the public domain: pp. 292–295).

Compareti, Matteo, "Assimilation and Adaptation of Foreign Elements in Late Sasanian Rock Reliefs at Taq-i Bustan," in: Asutay-Effenberger – Daim 2019, pp. 19–36.

Cormack, Robin, "St. Sophia as Feuertempel : Inspiration or Madness?," in: Foletti, Ivan (ed.), Orient or Rome? (Studia Artium Mediaevalium Brunensia 7), Brno 2018, pp. 15–35.

Dennert, Martin, Review of Brüx, Richard, Faltkapitelle. Untersuchungen zur Bauskulptur Konstantinopels, in: ByzZ 103 (2010) 199–202.

Dirimtekin, Feridun, "Saraçhane, Belediye Sarayının batısındakı eski park yerinde bulunan eserler - Finds from the Site of the Old Square West of the Town Hall at Saraçhane," in: Ayasofya Müzesi Yıllığı – Annual of Ayasofya Museum 2 (1960) pp. 17–18 (Turkish); pp. 42–43 (English); Fig. 9–14.

Effenberger, Arne, "»Sasanidischer« Baudekor in Byzanz? Der Fall der Polyeuktoskirche in Konstantinopel," in: Asutay-Effenberger – Daim 2019, pp. 155–194.

Garipzanov, Ildar, Graphic Signs of Authority in Late Antiquity and the Early Middle Ages, 300-900 (Oxford Studies in Medieval European History), Oxford University Press, Oxford 2018.

Labrusse, Rémi, "Délires anthropologiques : Josef Strzygowski face à Alois Riegl," in: Les actes de colloques du musée du quai Branly Jacques Chirac 1 (2009,) online.

Mango, Cyril – Ihor Ševčenko, "Remains of the Church of St. Polyeuktos at Constantinople," in: Dumbarton Oaks Papers 15 (1961) pp. 243–247.

Nelson, Robert S., "The History of Legends and the Legends of History : The Pilastri Acritani in Venice," in: Maguire, Henry, and Robert S. Nelson (eds), San Marco, Byzantium, and the Myths of Venice (Dumbarton Oaks Byzantine Symposia and Colloquia,) Dumbarton Oaks Research Library and Collection, Washington, D.C. 2010, pp. 63–90.

Ševčenko, Ihor, "Note additionnelle," annexe to: Lafontaine, Jacqueline, "Fouilles et découvertes byzantines à Istanbul de 1952 à 1960," in: Byzantion 29/30 (1959/1960) pp. 339–386: 385–386.

STROTH, FABIAN, Monogrammkapitelle : Die justinianische Bauskulptur Konstantinopels als Textträger, Reichert Verlag, Stuttgart 2021.

STRUBE, CHRISTINE, Polyeuktos und Hagia Sophia. Umbildung und Auflösung antiker Formen, Entstehen des Kämpferkapitells, Verlag der Bayerischen Akademie der Wissenschaften, Munich 1984.

STRZYGOWSKI, JOSEF, "Antiochenische Kunst," in: Oriens Christianus. Römische Halbjahrhefte für die Kunde des christlichen Orients 2 (1902); also in further formats, freely accessible pp. 421–433.

WOODFIELD, RICHARD, "Gombrich on Strzygowski," in: Journal of Art Historiography 17 (2017).

Keywords

Saint Polyeuktos; Late Antique Constantinople; Juliana Anicia; Early Byzantine Architecture