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# Unpublished letters of Sir W. M. Ramsay in the Cambridge University Library

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by Colin J. Hemer

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*Dr. Hemer, following in Sir William Ramsay's footsteps, has engaged in first-hand research in Asia Minor. The fruits of his research, some of which have already appeared in various periodicals, will be widely appreciated when his major work on the Letters to the Seven Churches is published. The present parergon, the fruit of research in Cambridge University Library, has a personal as well as a literary-historical interest.*

**S**IR William Mitchell Ramsay made in his day a remarkable contribution both to Anatolian studies and to New Testament scholarship, even if his later reputation as an apologist has for many overshadowed his earlier achievements. His writings display a vigour and passion for truth which he retained to the end.

The six letters preserved in the manuscript collection of the Cambridge University Library are all addressed to notable men of the contemporary literary and scholarly world, two to E. H. Blakeney, two to Professor F. C. Burkitt, and one each to W. Robertson Smith and M. R. James. Their force and vitality is characteristic. They have a double interest: they shed further light on Ramsay the scholar in action and debate, and they reveal a more personal dimension of the man.

My present purpose is to quote the substance of four of the letters which touch most illuminatingly on questions of scholarship, annotating them in the effort to explain the context of each and the points at issue.<sup>1</sup>

1. (Add. 7449 D 591) The first in date was addressed to W. Robertson Smith, the subject of a notorious heresy trial which culminated in his dismissal from his Chair at Aberdeen in 1881. It is notable that Ramsay, who bears from our subsequent perspective the image of a defender of traditional positions in New Testament

<sup>1</sup> The other two letters are of more limited interest. Add. 7509 C 455, to Blakeney, is dated 2 May, 1896. Ramsay writes in gratitude for a kind comment and alludes to a point of translation, evidently of *spermologos* (AV "babbler") in Acts 17: 18. He confesses that he feels presumptuous at venturing to write on such a subject as Paul.

Add. 7658 B 815, of 26 March 1913, addressed to Burkitt, discusses the genuineness of a suspect inscription.

criticism, studied the Old Testament under Robertson Smith during the very years of the controversy. He writes warmly elsewhere of this association, and implies that he learnt an indispensable lesson in thoroughgoing critical method from it.<sup>2</sup>

The letter evidently alludes to a point raised by discussion of the continuing question of the date and provenance of the Bezan reviser of Acts, whom Ramsay placed in Asia Minor in the second century.<sup>3</sup> The argument is stated in *The Church in the Roman Empire*, pp. 151-2. The proofs were presumably those of this passage for the book was published in the same year:

Aberdeen 9.3.93

My dear Smith,

I don't think you quite clearly conceive my purpose in the note. I do not quote a statement made by Neil about Aristoph. I transfer it to Hellenistic Greek. He made the statement about names occurring in my proofs, which were under discussion in his rooms, and specially about the name 'Επαφρόδης-Επαφρόδειτος. It seemed to me to illustrate my argument, and I added it to the proof. I believe it to be partly correct: it is true, but there is more to be said. I am of course speaking only of Greek names, not of naturalised Semitic names; and I do not see that Semitic facts had any influence on the Greeks of Asia Provincia. My entire argument turns on the thought and expression usual among the Greeks of Ephesus and the Eastern Highway about A.D. 130. To them Apollos was a hypokoristikon for Apollonius, and they felt it to be less dignified than the full name.

I probably do not catch exactly what your brief note is designed to explain. The fact you state about Semitic hypokoristica [*sic*] was not known to me, and is a very interesting one, which I am very glad to learn, but it does not seem to touch, much less to affect injuriously, my argument.

Ramsay's rather summary dismissal of the possibility of Semitic influence in this part of Asia is characteristic of his mind. In the present instance he is in fact probably justified. Judaism was certainly strong in the area, but its culture was greatly Hellenized. Semitic names are notably rare in the documents preserved. Even if these are unrepresentative, the trend of evidence suggests that even the indigenous languages were fast yielding to Greek. And by the time of a supposed Christian reviser of the second century the church in Asia had, I believe, become sharply separated from the synagogue and so of necessity involved the more deeply in the surrounding Hellenism.

<sup>2</sup> See especially his review of G. A. Smith, *Historical Geography of the Holy Land*, in *The Expositor*, 5th ser., 1 (1895), 55 ff. Cf. *The Bearing of Recent Discovery on the Trustworthiness of the New Testament* (London: Hodder and Stoughton, 1915), p. 17.

<sup>3</sup> For Ramsay's views see further his paper "Professor F. Blass on the Two Editions of Acts", *Expos.* 5th ser., 1 (1895), 140. For a recent, and different, treatment of the problem see R. P. C. Hanson, "The Provenance of the Interpolator in the 'Western' Text of Acts and of Acts Itself", *N.T.S.* 12 (1965-6), 211-230.

The argument runs like this. The Western text of Acts 18:24 differs from that of the great MSS in the use of the full and formal personal name "Apollonius" where they have the shortened, familiar variant "Apollon". The familiar form would have been the original, the formal one due to a later reviser who felt it unworthy of the dignity of the passage.

Neil had expressed a view, with regard to the familiar "Epaphras" (for "Epaphroditus"), that such variants in -ᾶς were thought contemptuous, and Ramsay added a note of this opinion to his proofs, to reinforce his general point that an undignified form would have been liable to secondary alteration.

Neil's point is slight and doubtful, and adds little to Ramsay's case. It duly appears in the first edition of *The Church in the Roman Empire* (p. 152n.), but was omitted from later editions.

2. (Add. 7481 R 25) The letter to Montague Rhodes James, later Provost of King's College, Cambridge, and author of *The Apocryphal New Testament*, dates from the same year and takes us deeper into the same underlying topic. Ramsay writes from Aberdeen on 23 Aug. 1893, and expresses gratitude to James for the sending of a copy of his *Apocrypha Anecdota*.<sup>4</sup> He continues:

I should like to devote a week to a study of Polyxena in relation to Thekla; but unfortunately I am bound to a piece of work and can't get free. One thing that strikes me is that—as yet—I have noticed no reference to the Iconian additions to the legend of Thekla: in fact Thekla is spoken of as belonging to Antioch, which seems to show that Iconium played so small a part in the tale as to be a vanishing feature in it. Further suspicions must be hidden, as being too vague. I have however an idea that either the Bezan text was used by the authors or that it arose in the same surroundings as *Acta P. et Theklae*: cf. Acts XIX 28 with your second parallel on p. 186.

P.S. Chase seems to me to have greatly advanced the solution of the Bezan problem, but not to have completed it: what I mean may perhaps appear from the statement that his ideas do not seem to conflict with but only to complete and give form to what I have urged on the same subject.

The previous letter dealt with a point of detail. Here Ramsay looks at a complex matter of literary relationships with a bearing on the question of the Western text. The central theme is the dependence of the traditions of Polyxena on those of Thekla.

The nature of this relationship, as Ramsay conceives it, is rather involved. James's book had contained the first publication of the text of the *Acts of Polyxena and Xanthippe*, a compilation not earlier than the third century, in which he found evidence of the use of language and incidents derived from several earlier apocryphal works, including the *Acts of Paul and Thecla*. Ramsay discusses

<sup>4</sup> M. R. James, *Apocrypha Anecdota (Texts and Studies, Vol. 2, No. 3)*, Cambridge U.P., 1893.

this document at length in his almost exactly contemporary *Church in the Roman Empire*.<sup>5</sup> He regards it as based on an early tradition of Pisidian Antioch, to which a second century writer had added the incidents set in Iconium, and which later hands had interpolated with grotesque legends while garbling the setting to fit a mistaken location in Syrian Antioch. His reading of James thus far then gives him no reason to doubt that the Polyxena tale is dependent on that of Thecla in its earlier rather than any of its later forms.

Ramsay's interest in Thecla is inseparable from his ideas about the milieu of the Bexan text. The reference of the last sentence of his paragraph is not quite clear, but the parallel to which he refers is threefold. To James's comparison of *X. and P.* 8 with *P. and Th.* 11 he adds allusion to a well-known Western addition in Acts 19: 28. All three passages speak of "rushing out into the street", and all three use for "street" the rare *amphodon*.<sup>6</sup>

The postscript, with its appreciative reference to the work of F. H. Chase, is also interesting, for shortly afterwards the two men differed deeply on this very issue. When Chase's book *The Old Syriac Element in the Text of the Codex Bezae* was published the same year, he had seen Ramsay's *C.R.E.* in time only to add a late and critical footnote.<sup>7</sup> Ramsay later made a severe comment on Chase's work.<sup>8</sup>

3. (Add. 7509 C 456) This note, written many years later to E. H. Blakeney, is very different in theme and feeling, but of particular interest. In it Ramsay declines to review the work of a friend. The letter is typed.

13 Greenhill Terrace,  
Edinburgh Dec 7th 1920.

Dear Sir,

I have not seen Charles's work on the Apocalypse and am rather avoiding the task of studying it for the present. At any rate I am far too busy with imperative work of a different kind.

I have already stated some years ago in the "First Christian Century" my absolute dissent from the idea that the Apostle John was killed at an early period.<sup>9</sup>

I do not think any healthy literary criticism would permit such an

<sup>5</sup> (London: Hodder and Stoughton, 1893), Chap. 16, pp. 375-428.

<sup>6</sup> The word and derivatives of it occur occasionally in the rather different sense of a "ward" or "quarter" of a town. Its attestations have been significantly increased by documents contained in one of the latest volumes of the *Oxyrhynchus Papyri*, Vol. 38, ed. G. M. Browne *et al.* (London: Egypt Exploration Society, 1971). The official *amphodarches* is known from Asia Minor at Pergamum (*Orientalis Graeci Inscriptiones Selectae* 483.82).

<sup>7</sup> (London and New York, 1893), pp. vii-viii and 137n.

<sup>8</sup> *Expos.* 5th ser., 1 (1895), 221f.

<sup>9</sup> (London: Hodder and Stoughton, 1911), pp. 47-51.

elaborate theory of triplication of Johns and of transpositions and incompetent redactors. Assuming an editor (which I think is a highly probable thing) there can be little doubt that he could not have been so utterly incompetent and ridiculous as Canon Charles makes him out to be. The supposition is impossible.

It is however in the last degree improbable that I shall review the book, as I could not do so without hurting my friend's feelings, and friends grow fewer as one grows older. (I shall tell him.) [MS insertion]

My interest is in truth and in the proper method of literary work and scientific criticism.

R. H. Charles's great two-volume commentary on the Revelation in the *International Critical Commentary* series<sup>10</sup> was the culminating fruit of twenty-five years' study of apocalyptic literature. His work is still a monumental quarry of information for textual and exegetical study. But we must sympathize with Ramsay's views of his redactional theories, which have not commended themselves to later scholarship.

The letter is valuable as the only comment noted from Ramsay's later life on the study of the Revelation, a topic to which he had once made a notable contribution.<sup>11</sup>

4. (Add. 7658 B 816) The series closes on a singularly fitting note, on the principles and methods of the Anatolian research to which Ramsay had devoted so many years of his life. He writes to F. C. Burkitt, already for many years Norrisian Professor of Divinity at Cambridge, from a hotel in Marseille, whence he proposed to travel to Algiers and beyond. The letter expresses his grief at the news of the death of H. S. Cronin, a former valued colleague in the exploration of Asia Minor. His Anatolian friends had been departing one by one and in companies during the war. The date is 18 November 1922.

1. His 3 arts in JHS<sup>12</sup> were good examples of the preparatory work necessary to lay foundations for the history of the country: careful, accurate, and avoiding dangers of hasty premature speculation. At that time we were all still more or less dominated by the idea that Asia Minor was to be looked at through European Greek spectacles, coloured spectacles out of focus. It has taken many years to learn that the Sons of Yavan,<sup>13</sup> the "Old Ionians", were and are markedly different from and in many ways more original and creative than the Greeks of Europe—history, medicine, philosophy, epic and lyric poetry, etc.—The politicians are still ruled by

<sup>10</sup> *A Critical and Exegetical Commentary on the Revelation of St. John* (Edinburgh: T. and T. Clark, 1920).

<sup>11</sup> *The Letters to the Seven Churches and their Place in the Plan of the Apocalypse* (London: Hodder and Stoughton, 1904).

<sup>12</sup> Cronin, "First Report of a Journey in Pisidia, Lycaonia and Pamphylia", *Journal of Hellenic Studies* 22 (1902), 94-125 and 339-376; 24 (1904), 113-128, the last including the texts of inscriptions from Lystra.

<sup>13</sup> This alludes to Ramsay's view of Gen. 10: 4. See his *Cities of St. Paul* (London: Hodder and Stoughton, 1917), pp. 117 ff. Cf. Josephus, *Ant.* 1.6.1.124ff.

the Hellenistic mirage, forgetting that every attempt made by Eur<sup>n</sup> Greeks to dominate As<sup>n</sup> Greek [*sic*] has ended in disaster all round. Cronin belongs to the period of the illusionment, like myself still even there after 22 years of study.

Now there is an Anatolian science of Epigraphy and History. Now we see: then only thro' a glass. Now we have knowledge: then we had only opinion, δόξα more or less ἀληθής, but only δόξα. Cronin did much good work in making the new knowledge possible.

2. His paper on Ptolemy was, I think, in *Geogr. Mag.* of R.G.S.<sup>14</sup> His principle was, I think, quite sound: Ptolemy must be judged according to roads and dead reckoning, not as stating astronomical positions. The principle is right: the carrying out of it needs much delicacy of knowledge and long time research on the spot. He reconstructed for me Ptolemy's map of central Asia Minor, on which I proposed to work out his theory in detail: but time has failed; and peace is not at hand yet. Some time, if I live, I hope to do justice to his paper and his ingenious application of it.

Here is something of the perspective of the veteran scholar on the studies to which he had himself contributed so much. His words are the more poignant when seen against the background of the bitter war then raging for control of Asia Minor between the invading Greeks and the Kemalists.

The reference to Cronin's work on Ptolemy merits a special note. His paper showed with great ingenuity how Ptolemy's inaccuracies and eccentricities were the inevitable result of his method. He is not to be blamed too severely for these flaws: he did his best with the faulty data available to him. He located places on his map on the basis of road distances from reference points at the four corners of the country, Idyma in Caria, Chalcedon, Trapezus and Issus. He realized that this made no allowance for winding of the roads, but gravely underestimated the errors involved. He was forced into arbitrary approximations which became progressively more distorted towards the centre of the map, the area where Ramsay hoped to work out Cronin's theory in detail. I cannot find that he ever fulfilled this purpose.

*Plymouth.*

<sup>14</sup> Cronin, "Ptolemy's Map of Asia Minor: Method of Construction", *Geographical Journal* 25 (1905), 429-441.