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## Books, literacy, and Christian communities : on two recent books by Roger S. Bagnall

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Ewa Wipszycka

**BOOKS, LITERACY, AND CHRISTIAN COMMUNITIES  
ON TWO RECENT BOOKS BY ROGER S. BAGNALL\***

THESE TWO SLENDER VOLUMES are among the most important works on Antiquity written in this generation, owing to both their subject – literacy, and their author – Roger S. Bagnall. They are written with that remarkable clarity and straightforwardness that is achieved (by some) after years of intensive work testifying to the author’s broad horizons and technical mastery in interpreting papyri and inscriptions. Bagnall demonstrates how to write a scholarly text using only the plain language of the humanities, without resorting to technical terminology or introducing new, bombastic-sounding words that annoy everyone except the author himself. This is the reason why he is such an excellent translator of research results produced by papyrologists to the entire milieu of students of Antiquity; as a rule papyrologists write primarily for their colleagues, yet at the same time they get aggravated when researchers from outside the guild neglect to cite their work.

\* Roger S. BAGNALL, *Early Christian Books in Egypt*, Princeton University Press, Princeton – Oxford 2009, 104 pp. with the French version: *Livres chrétiens antiques d’Égypte*, Librairie Droz, Genève 2009, 109 pp (*École Pratique des Hautes Études. Sciences Historiques et Philologiques* – III. *Hautes Études du Monde Gréco-Romain* XLIV); and *Everyday Writing in the Graeco-Roman East*, University of California Press, Berkeley – Los Angeles – London 2011, 179 pp. (*Sather Classical Lectures* LXIX).

The basis for these books are lectures delivered at UC Berkeley, as part of the Sather Classical Lectures (2005), and at the École Pratique des Hautes Études (2006). In both cases the audience consisted of members of the same profession, so the lecturer was free to maintain a high scholarly level and was expected to provide a new approach to fundamental topics rather than (or perhaps besides) new information. The lectures are a part of a splendid academic tradition and the invitation to deliver them is the highest form of recognition of the invited scholar's achievements, all the more effective that it leaves a mark in literature on the subject.

I have started my review with a general opinion on the books, instead of placing it at the end, to make it clear how highly I value them and how much pleasure I have derived from reading them. Their discussion will sometimes lead me to reject Bagnall's ideas, so I do not wish to leave the reader under a false impression of cumulated pretences. A part of my polemic with Bagnall concerns the results of calculations he makes on various occasions, exercising his belief that views should be expressed with figures whenever possible.<sup>1</sup> I do realise that I belong to an older generation and that I was raised in a different milieu of historical culture than Bagnall. I received my academic formation in Warsaw and at the École Pratique des Hautes Études, where I was effectively instructed not to calculate anything that is not directly supported by the sources. I am, however, anything but proud of this and I do my best to curb my distrust for figures presented in tabular form, since I have often learnt in the past that I am utterly anachronistic in my persistent scepticism. I accept most of Bagnall's calculations and consider them of great value. However, in some cases I believe that he is simply wrong because the data he uses do not entitle him to perform his computations.

I will begin my discussion with the Paris lectures published in 2009. They contain a summary of knowledge collected to date on Christian books circulating in Egypt in pre-Constantinian times.

<sup>1</sup> I was amused by passage from p. 61 of *Everyday Writing*, which illustrates the author's attachment to data expressed with figures: discussing research on slavery in the period from the 4th century onwards, when the number of texts attesting slavery dwindles, he writes: 'At all events, I believe the numbers game must be abandoned for this subject.'

A note on the condition of the books is due to the non-papyrologist. Christian books from this period have reached us in fragments, often very small ones. Unfortunately, we seldom know their provenance, let alone their archaeological context. Most of them reached the hands of scholars through the antiquarian market. Their date is usually based on palaeographical grounds and therefore, as it is often the case with literary texts copied by professional scribes, the achieved results are uncertain. The small size of preserved passages is an additional obstacle; the discussion of the size of letters, columns, *etc.*, is poorly founded. And another note: even with small fragments it is possible to tell whether we are dealing with a literary work in scroll or in codex form: in the latter case the text on the verso is a continuation of the recto and this is clear at first glance.

Bagnall's first objective was to compile a register of Christian books for which we have reason to believe that they came into being in the 2nd century or in the first quarter of the 3rd century. The results of his research are presented in table I.1 on p. 17.

	Dated II or II/III	Dated early III	Total
Old Testament	3	8	11
New Testament	3	6	9
New Testament Apocrypha	1	2	3
Hermas	1	3	4
Dubiously Christian	0	2	2
Total	8	21	29

The results of Bagnall's research are even more momentous than one might think after a first glance at the table. If we take a closer look at the entries in the first column we see that the oldest texts are datable to no earlier than the last quarter of the 2nd century. What is more, Bagnall proves that their lack (as well as the lack of documents mentioning Christians) is 'normal', in the sense that very small groups had no chance of leaving a trace in the papyrological record.

Bagnall operates on sensitive material (the reader will find an example of this in chapter 2: 'Two Case Studies'). With the exception of those who oppose the Christian God for personal reasons, everyone wants the papyrus finds to be as old and as numerous as possible, believing they constitute the best proof of the strength of Christian communities and the speed of their development. Meanwhile, Bagnall's analysis puts an end to attempts to draw conclusions on the development of Christian communities in the Egyptian *chora* based on Christian manuscripts of the first three quarters of the 2nd century. Therefore, considering that in literary texts we have no information on Christians active in the Egyptian *chora* in the 1st and 2nd centuries, we have to admit that we know nothing about the Christian communities in this region. It cannot be determined whether the silence of the sources should be considered meaningful or incidental. Bagnall insightfully and convincingly argues that not only do we know nothing, but we also have no chance of finding anything in the future. The scientifically correct approach should be to take the advice of Ludwig Wittgenstein: 'Worüber man nicht sprechen kann, darüber muss man schweigen'. This will be very difficult. Bagnall's voice may be the voice of one crying out in the desert.

I have read Bagnall's arguments feeling that in the matter he discusses I do not have a clear conscience. A few years ago I conducted research on the history of Egyptian Christianity in the period before the great turning point constituted by the time of the Little Peace (from the death of Valerian to the Diocletianic persecution); I then published an article proposing a new model of the development of ecclesiastical institutions in the *chora* of this period.<sup>2</sup> Its point of departure was a mention in the *Chronicle* of Eutychios, the Melkite patriarch of Alexandria in the 10th century, according to which it was not until the bishop of Alexandria Demetrios, in office for 43 years (189–233), that the first three bishops for the *chora* were ordained (unfortunately he does not mention when these ordinations occurred, or for which cities). This piece of information, although it differs radically from the traditional portrayal of the develop-

<sup>2</sup> Ewa WIPSYZKA, 'The Origins of Monarchic Episcopate in Egypt', *Adamantius* 12 (2006), pp. 71–89.

ment of the Church in Egypt based on Eusebius' account, is highly probable (arguments in favour of this the reader will find in the cited article; repeating them would occupy too much space). In the light of this account Christian congregations (specifically congregations, not individual Christians) appeared in the *chora* in the last quarter of the 2nd century at the earliest. I must admit, it was hard for me to accept this idea (I was not the only one to object to the theory of the late penetration of the Christian religion into Egypt at a time when the Christian community of Alexandria was flourishing). This protest led to a hypothesis that in the 2nd century there existed Christian congregations in the *chora*, but there were no bishops; the churches would have been run by presbyters elected by members of the congregations. It was only Demetrios who managed to initiate the process of establishing episcopal sees – a process that was slow as it met with resistance of the congregations, which did not wish to be ruled by Alexandria. The entire construct made sense under the condition of finding proof that groups of Christians existed in the *chora*. The reader already realises that to me this proof was the early presence of Christian texts among literary papyri. Bagnall's lecture destroyed my construct, or at least its greater part.

My interpretation also turned out to be wrong for another reason (when delivering his lectures Bagnall did not know about it yet, which explains why he did not mention it when he rightly criticised my model of the Church in the 2nd century). Namely, 2006 brought the first tidings of a new Ethiopian collection of normative texts that included, among other works, a list of bishops ordained by successive leaders of the Alexandrian Church from Mark the Evangelist to Peter I (300–311).<sup>3</sup> The list originally formed part of the *History of the Alexandrian Episcopate*, written in the time of Cyril the Alexandrian (412–444) using documents from the archive of the patriarchate. We learn from this document that Demetrios ordained not three, but ten bishops. It is a significant differ-

<sup>3</sup> A. BAUSI, 'La collezione aksumita canonico-liturgica', *Adamantius*, 12 (2006), pp. 43–70. Waiting for a publication of the entire Ethiopian find, which will include a full commentary by A. CAMPLANI, see the preliminary commentary by A. CAMPLANI, 'Lettere episcopali, storiografia patriarcale e letteratura canonica. A proposito del *Codex Veronensis LX*', *Rivista di Storia del Cristianesimo* 3 (2006), pp. 117–164.

ence, indicating that with strong support of the Alexandrian congregation this bishop could act more efficiently in the *chora* than I gave him credit for. It is not to be excluded that before his episcopate there were no organised groups of Christians in the *chora* (or that they were few and very weak). In this situation the supposition that their emergence was the effect of the actions of the Church of Alexandria becomes justified (which does not mean that it is, in my opinion, very probable, please excuse my obstinacy, but to me the continued existence of such a large blank space on the map of the Christian world is still hard to believe).

The discussion of the appearance of fragments of Christian literary works among Egyptian papyri required Bagnall to take a stand in the matter of the number of Christians in the *chora*. Demonstrating his fondness for expressing opinions with figures instead of conventional expressions like 'very few' or 'few', Bagnall provides 'a model of what these numbers might have been' (pp. 18–19).<sup>4</sup> This operation, a shock to those who are unaccustomed to using statistical models, is worth describing here. The point of departure are two assumptions, which are correct beyond any doubt: In AD 40 there were probably around 1000 believers in Christ in the entire Roman Empire; by the late 4th century the overwhelming majority of the Empire's inhabitants had abandoned pagan cults. Today ancient historians generally agree that the Roman Empire's total population should be estimated to 55 million, of which a tenth part, or 5.5 million, lived in Egypt. Bagnall assumes that the percentage rate of the annual growth of the number of members of Christian communities was 3.4 on average. The results of these calculations for dates set in 25-year intervals are as follows: 100 – 753; 125 – 1,746; 150 – 4,047; 175 – 9,382; 200 – 21,747; 225 – 50,409; 250 – 116,849 (p. 20). In Bagnall's opinion, of ca. 20,000 Christians in the beginning of the episcopate of Demetrios at least 5000 lived in Alexandria, which leaves us with 15,000 in the *chora*, approximately 300 per nome. I think that a careful reader does realise that we are presented with approximate values and that the data, though

<sup>4</sup> He is inspired by the ideas of K. HOPKINS, 'Christian Numbers and Its Implications', *Journal of Early Christian Studies* 6 (1998), pp. 185–226 and R. STARK, *The Rise of Christianity: a Sociologist Reconsiders History*, Princeton 1996.

given in exact numbers for the sake of statistical elegance, only gives an idea of the scale and cannot be taken literally. However, Bagnall's next step of dividing the 15,000 by the number of nomes is burdened with an error: in the conversion phase the differences between regions were significant, since the effects of it still depended on individual factors like the presence of charismatic personalities (or a lack thereof), the existence of popular pagan sanctuaries, *etc.* (not to mention the banal matter of differences, often significant ones for that matter, in nome population). The numbers levelled out gradually.

I am also disturbed by Bagnall's next sentence: 'It is hard to see why the average nome would have needed a clergyman of higher status than presbyter, and the absence of bishops at this time seems in this light not terribly significant' (p. 23). Bagnall is wrong because he does not take into consideration the logic of the Christian cult. The acceptance of the monarchic episcopate model in the Church led to limiting liturgical competences of the presbyters. A Christian from the 'average nome' in Egypt could not be prepared for baptism or baptised without a bishop and major doubts are expressed in literature on the subject if it was possible to give Eucharist in his absence. The distances from village to metropolis within the nomes were not that great, but a longer journey (to Alexandria!) would have had very serious consequences, completely cutting Christians off from cult practice. Suffice it to see what the network of churches looked like in regions like Syria or Proconsular Africa. All larger towns (not only cities) were episcopal sees. Bagnall is mistaken in thinking that 300 Christians in a given area is a small number. Congregations in the 2nd–3rd centuries were sometimes much smaller than that.

Books were very expensive. Bagnall puts great emphasis on this fact and he is undoubtedly correct. However, such a general statement does not satisfy him and he needs to define what the word 'expensive' means. Bagnall has scarce data on the prices of books, it is always information from a later period than the one that is the subject of his study: they refer to the 5th–7th century (or even later, the 8th century). Nothing can be done about this, the earlier sources are silent (however, there is important 4th-century information on the price of writing material and it is included in Bagnall's book). The oldest attested price is 18 solidi for a codex containing the Old

and New Testaments. It is mentioned in an apophthegm from the *Alphabetikon* devoted to Gelasios (no. 176), who lived in Palestine.<sup>5</sup> We learn from this text that one of the brothers stole a codex from the old man and tried to sell it. Gelasios did not lose composure when he saw his precious property in the hands of a seller who had come to him seeking advice on what price to propose for it (Gelasios valued the Bible to 16 solidi), he did not protest and did not try to learn the identity of the seller. Using this text to determine the real price of a codex is not as simple as one might gather from Bagnall's argument. The trouble lies in the nature of the source. When giving the price of the codex the author/editor of this apophthegm wants to use it to highlight the virtue of the main character, who is oblivious to worldly matters, contemplating the lesson to be learned by the brother guilty of the theft instead of thinking about his own loss. The price was therefore not a neutral element of the background against which the plot developed; it had to be strikingly high, or else the apophthegm would not have had the proper effect.<sup>6</sup> Bagnall also refers to the price of a codex containing the New Testament mentioned in *Pratum Spirituale* 134 by John Moschos (early 7th century). A certain poor anchorite desired to own a copy of the New Testament. The middleman he turned to, also a monk of the Palestinian desert, found a wealthy brother who had for sale a beautiful parchment volume he had priced at three solidi. When the owner of the codex found out that it was meant for an anchorite, he wanted to simply give it to him. The latter, in turn, felt guilty and desperately wanted to pay

<sup>5</sup> This apophthegm was part of a collection circulating in Palestine in the first half of the 6th century (it was in the manuscript used by Pelagius and John, the translators of the apophthegms into Latin in the mid-6th century). On the date of the apophthegms whose main character was Gelasios see R. S. BAGNALL, 'Monks and Property: Rhetoric, Law, and Patronage in the *Apophthegmata Patrum* and the Papyri', *Greek, Roman and Byzantine Studies* 42 (2001), pp. 19–20.

<sup>6</sup> I am familiar with an apophthegm from the same collection, similar in structure, though it does not mention money. It is *Ammoe* 134, according to which the main character lays out 50 artabas of bread in the open air for drying and then, having come to a conclusion that the locality had something that was potentially harmful to his soul, he abandoned this fortune saying to his disciples 'Let us leave this place'. Specialists on diet claim that an artaba of wheat or bread satisfied the biological needs of an individual for a month. The author/editor of the apophthegm chose a high number to reach a literary aim; one or two artabas would not have ensured the proper effect.

for the codex. He took up a job in the construction of a water reservoir in the Sinai, earning five coins and living on ten lupine seeds a day and managed to save up the desired sum. The contrast between 3 solidi and 10 lupine seeds emphasises the virtue of the owner of the codex and of the buyer (I have no intention of discussing the probability of this information; information on the food eaten by the brethren is among the most intentionally deformed elements of narration of the apophthegms). The story has exactly the same motivational purport as the apophthegm on Gelasios.

Unfortunately, for Bagnall's reasoning also the codex of Gelasios belongs in the realm of fiction. Volumes containing the complete Bible were very rare in Antiquity due to their size and the resulting copying costs, as well as difficulties with their transport and reading. Two such codices, the *Codex Vaticanus* and the *Codex Sinaiticus*, date from the 4th century and their spectacularly ceremonial character and quality as far as the art of bookmaking is concerned even induced some scholars to put forward a most likely erroneous hypothesis that they were among the fifty copies made upon the order of Constantine the Great in Caesarea in Palestine in AD 332.<sup>7</sup> Nonetheless, these copies may have inspired a model of a de luxe edition. We have no indication that there existed editions of the entire Bible or even of the complete New Testament collected in one volume before the Constantinian turning point.<sup>8</sup> The small Palestinian monastery of Gelasios was not the place for a luxury, monumental parchment edition of the complete Bible. One can hardly trust it to be a testimony of a 'non-literary' reality. Of the data cited by Bagnall only the conclusions of Anne Boud'hors (a third of a solidus for a part of the Bible) are reliable, since they are based on documentary texts from the Theban region, unfortunately late ones, from the 7th–8th century.<sup>9</sup>

<sup>7</sup> We learn about this from Constantine's letter preserved in the *Life of Constantine* 4, 36, the work of Eusebius of Caesarea.

<sup>8</sup> For a very convincing discussion of this subject see H. Y. GAMBLE, *Books and Readers In the Early Church. A History of Christian Texts*, Yale 1995, pp. 79–80

<sup>9</sup> Anne BOUD'HORS, 'Copie et circulation des livres dans la région thébaine (VII<sup>e</sup>–VIII<sup>e</sup> siècles)', [in:] A. DELATTRE & P. HEILPORN (ed.), *«Et maintenant ce ne sont plus que des villages...».* Thèbes et sa région aux époques hellénistique, romaine, byzantine, Bruxelles 2008, pp. 149–161.

I wonder if in his assessment of the costliness of books Bagnall is not exaggerating after all. The reason for this is my impression from the reading of Pachomian texts, one of which is worth mentioning at this point. I have in mind three passages from the Pachomian rules. In *Praecepta* 25 we read: *Codicem si ad legendum petierint, accipient; et finita ebdomade, propter eos qui succedunt in ministerium, suo restituent loco*; and in *Praecepta* 101: *Codices qui in fenestra, id est in risco parietis, reponuntur ad vesperum, erunt sub manu secundi qui numerabit eos et ex more concludet*; finally, in *Praecepta atque instituta* 2: *Si codicem postulaverint, deferant eis*.<sup>10</sup> Specific passages from the Pachomian rules cannot be dated with precision, but for all of them the *terminus ante* is 404, when Jerome translated them into Latin. In the 4th century Pachomians do not seem to be a rich congregation, but they have codices that monks can take to their cells for private reading. I find it difficult to imagine the monks coming into their possession, were we to consider the sum from the apophthegm of Gelasios trustworthy. Of course we can assume that the monks copied the necessary works on their own, having borrowed them, for instance, from a local bishop. However, one needs to remember that there is no mention of copying books in the Pachomian dossier. Keeping in mind the weakness of the *argumentum ex silentio*, I would nonetheless like to point out that when creating the rule Pachomius and his disciples considered reading (not only listening to) sacred books as something obvious.

Of great significance for Bagnall's line of reasoning is to determine the financial resources of churches and of the group that was the most interested in owning books – the clergy. In order to do this, he uses the results of research carried out by Sabine Hübner, who very diligently collected information on the revenues of the clergy from the time of Saint Cyprian, or the mid-3rd century, until the first half of the 7th century.<sup>11</sup> Data obtained from various sources (Justinian's *Novels*, patristic texts, including the especially important letters of Severus, monophysite patriarch of

<sup>10</sup> I refer the reader to an edition of the rules with a good commentary: H. BACHT, *Das Vermächtnis des Ursprungs* II, Würzburg 1983.

<sup>11</sup> Sabine HÜBNER, *Der Klerus in der Gesellschaft des spätantiken Kleinasien*, Stuttgart 2005, pp. 213–228.

Antioch from 512 to 518, lives of holy bishops). These sources rather consistently show the scale of clergymen's annual revenues: 5-10 solidi for presbyters and deacons, several tens of solidi for bishops of small towns, 250-365 solidi for bishops of large cities.

In Egyptian documentation, which Hübner left aside, there are many texts containing information of interest.<sup>12</sup> They are usually registers of clergymen who received sums in cash or in kind, but unfortunately without indicating the nature of the disbursement (and often not even its size). Luckily there is an exception, a papyrus from Hermopolis dated to the Arab period, the first line of which reads: γνῶ(σις) τῶν κληρικῶ(ν) τοῦ ἁγίου Θεοδώρου καρπ(ῶν) β ἰνδ(ικτίωνος).<sup>13</sup> The register lists one presbyter, five deacons, and one doorman. On the verso the content is summarised as follows: τῆνω(σις) ἡπβεκε ἡνεκληρικο(ς). They receive wheat, eight artabas each, except for the doorman, who gets five artabas. The amounts are modest for an annual income and insufficient for sustaining even a single individual. The church in question was a *martyrion*, not an episcopal or parochial church and this fact may have had a negative effect on the size of annual disbursements for its clergymen. There must have been a relationship between the frequency of religious services (masses, baptisms, in the later period also blessings of marriages, collective prayer at church, participation in processions, *etc.*) and the level of income of members of the clergy. In this respect the differences between churches of various categories were significant. We do not know if the clergymen listed in the register received something else, wine for instance, how often they gathered for a common meal after mass, when they consumed the donations brought by the faithful – it was all part of their individual budgets. From another letter of interest, *P. Köln* II 112 (5th-6th cent.), we learn that a certain presbyter serving in a *martyrion* was unable to sustain himself from the crops of two arouras of land that belonged to this cult

<sup>12</sup> Collected by G. SCHMELZ, *Kirchliche Amtsträger im spätantiken Ägypten nach den Aussagen der griechischen und koptischen Papyri und Ostraka*, Leipzig 2002, pp. 204-217.

<sup>13</sup> I know the text from a handout distributed by Nicholas GONIS at the 7th International Congress of Coptic Studies in Leyden in 2000. It was not published in the acts of the congress and I was unable to find it in N. Gonis's publications.

place.<sup>14</sup> He must have had a family – if he were alone, the yield from such a plot (within 20 artabas of grain minus taxes amounting to more or less 4 artabas) would have sufficed for one person.

Regardless of how much can be learned from the information collected by Hübner and Schmelz as relates to the incomes of the clergy, it is clear to me that these data cannot be used for the aims Bagnall set for himself. The reason for this is the peculiarity of the financial situation of the clergy. Let me recall that in Antiquity members of the clergy were recruited among mature men who usually did not abandon their previous economic activities. They derived their income from land, crafts, trade, offices, *etc.*, so the Church allowance received for being part of the clergy was supplementary to sums obtained from non-ecclesiastical sources.<sup>15</sup> We know of clergymen who gave up their assets for ascetic reasons and subsequently lived only from what they received from the Church. However, such cases were very rare. Furthermore, the social composition of the clergy was so varied that wealth disparities must have been commonplace; they make it impossible to establish an average. All in all, we know very little about the financial status of the clergy, too little to be certain that none of its members could afford to buy books.<sup>16</sup>

It has been established for some time that Christians were not responsible for the change of book form that was the transition from scroll to codex, but many researchers are still convinced that Christians in the first three centuries AD had a marked preference for the codex.

<sup>14</sup> The first edition with commentary by H. EIDENEIER 'Ein byzantinisches Empfehlungsschreiben auf Papyrus', *ZPE* 6 (1970), pp. 189–192.

<sup>15</sup> Patristic and normative texts do not tell us this, papyri were needed to give an idea on how misleading is the data which for ideological reasons portray the situation of the clergy as a group fully devoted to work in the Church and completely dependent on it for material goods. Papyri, testimonies of everyday situations, deserve our trust much more than ecclesiastical and secular normative texts. For a listing of data on secular activities of the clergy, unfortunately no longer complete due to the passage of time, see Ewa WIPSYZKA, *Les ressources et les activités économiques des Églises en Égypte du IV<sup>e</sup> au VIII<sup>e</sup> siècle*, Bruxelles 1972, pp. 154–173.

<sup>16</sup> BAGNALL is aware of this peculiar situation of the clergy and he mentions it on p. 62. However, this awareness did not stop him from engaging in a lengthy discussion on the material status of the clergy and the possibility to buy books by its members.

In the light of the data collected by Bagnall, they are mistaken. Here are his results:

Christian and Non-Christian Codices by Century (*LDAB* datings)

<i>Century</i>	I	I/II	II	II/III	III	III/IV	IV
Total codices	3	3	27	67	186	124	252
Non-Christian codices	3	2	21	47	123	80	153
Christian codices	0	1	6	20	63	44	99
Christian as percentage of the total	0	33	22	30	34	35	39

Third-Century Christian Texts

<i>Category</i>	<i>Codices</i>	<i>Rolls</i>
Old Testament	23	3
New Testament	27	2
Apocryphal biblical	3	6
Hermas	4	2
Patristic	6	2
Other	3	4
Total	66	19

As regards the milieu in which the conversion of the book from scroll to codex took place, Bagnall gives his full support to those who believe it was Rome, which hosted a long tradition of using tablets not only in everyday life but also in official situations. In his opinion, the diffusion of the codex is a part of the well-known process of adopting Roman models by provincial elites, in other words – Romanisation.

The evidence concerning the choice of the codex form by Christians for the transcription of their holy books suggests that this innovation was not the result of a long evolution, but a sudden change. In order to explain it Bagnall follows a path indicated by Colin H. Roberts and Theodore C. Skeat, who wrote in 1983 that the adoption of the codex

must derive from a centre with 'sufficient authority and centralization to devise such innovations and to impose them in Christendom generally'.<sup>17</sup> They first suggested Jerusalem and Antioch, and later Rome. Larry W. Hurtado criticised this suggestion for naively assuming the scheme of a centralised ecclesiastical authority and I am profoundly convinced that he is right in doing so.<sup>18</sup> There is nothing to indicate that the heads of ecclesiastical capitals took an interest in such matters as the form of books. Christian congregations of the second half of the 2nd and the 3rd century are characterised by exceptional diversity as far as institutions and customs are concerned. Even liturgy (text of liturgical formulas, calendar, *etc.*) and a large part of the theological sphere were not yet subjected to unification processes. The formation of Roberts and Skeat took place in the mid-20th century when historians of the Church had trouble accepting the multitude of trends and ideas it encompassed and the bishops' liberty of action. This explains their ideas. As far as Rome is concerned, her bishop was a figure of great authority, but he was not in power to impose rules on community life; we have ample proof of this, and if anyone thought differently in the past, it was the effect of Catholic wishful thinking that blocked the view of facts which did not fit into the scheme of the papal primacy. The assessment of the scope of what was unified in the Church and what was subject to local fluctuations has shifted significantly over the last fifty years. It is, at least in part, the doing of those historians of Christianity who have re-defined orthodoxy and its channels of diffusion. I see no reason why the diffusion of the codex should not have been governed solely by cultural patterns. The model was Roman, indeed, but this does not mean it had to have explicit support of the centre of authority, in the case of Christians – of the bishop of Rome.

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<sup>17</sup> C. H. ROBERTS & T. C. SKEAT, *The Birth of the Codex*, London 1983, pp. 57–58.

<sup>18</sup> L. W. HURTADO, *The Earliest Christian Artifacts: Manuscripts and Christian Origins*, Grand Rapids 2006, p. 72, n. 95. BAGNALL rejects HURTADO's criticism, since the latter discussed the idea of ROBERTS and SKEAT with reference to the 1st and early 2nd century, whereas he is interested in the century that follows. However, the Church of the 3rd century was still far from uniformity, which progressed with difficulty in the 4th century, and not in all aspects.

With Bagnall's book *Everyday Writing in the Graeco-Roman East*, the term 'everyday writing' enters the vocabulary of researchers on Antiquity. I am convinced that we will all use it.

Although polemics appear only on some pages of the book, it is as a whole a protest to Harris' views presented in his famous book that significantly lowered the level of literacy in the ancient world, especially in Late Antiquity.<sup>19</sup> Whereas in the first of the works under discussion Bagnall stressed the small size of the circle of people who read and owned books, in the second he characterises the ancient society as the literary society.

My experience as a historian of Egyptian monasticism leaves me enthusiastic about Bagnall's reasoning.

The second book to a much greater extent than the first puts an emphasis on providing information. Although we should all monitor the emergence of new testimonies of literacy, it quickly turns out that our personal knowledge is far from complete. I admit that what Bagnall writes, for instance, about clay seals originally found on documents written on papyrus or leather was something of a surprise to me, although I was familiar with this type of artefacts from Hellenistic Uruk/Warka. The seals survived thanks to fires, which had consumed the archives of acts but preserved the clay. Their numbers go into tens of thousands and their origins are cities as distant as Artaxata in Armenia or Carthage. Around 16,000 seals were found in only one house on Delos destroyed by fire in 169 BC! They prove that texts were often used for economic and legal purposes and the authorities of various states established institutions for their registration.

I also confess that thanks to Bagnall's book I have become more closely acquainted with documents written using Greek letters in the Bactrian language over an extensive area for about a thousand years and hitherto known thanks to a very small group of texts. Over 150 documents from the years 342/3–781/2, brought to light by clandestine digging, appeared on the antiquarian market and some of them still await publication (their English translations are published, however). The Greek

<sup>19</sup> W. V. HARRIS, *Ancient Literacy*, Cambridge Mass. 1989. HARRIS deserves credit for one thing: his hypotheses stirred an uproar, thus contributing to a significant broadening of knowledge on literacy.

alphabet was adopted for the Bactrian language upon the ordinance of Kanishka (probably AD 127–151), a Kushan ruler, and the result proved to be very resistant to political changes over the subsequent eras. The discovery has already supplied a great deal of information on the society of eastern Iran and it significantly changes our view of the scope and longevity of Greek influence on the region, which ceased to be ruled by a Greek dynasty in ca. 130 BC.

To the different categories of testimonies of everyday writing discussed in the book I would like to add one that escaped Bagnall's attention. They are Greek letters dating from the second half of the 6th to the 4th century BC, 25 in total, written on lead plaques and sometimes on earthenware sherds. The texts predominantly come from the north shores of the Black Sea and from Emporion in Spain.<sup>20</sup> They are especially useful to historians of economic and social relations.

I read the chapter 'Writing on Ostraca? A Culture of Potsherds?' with keen interest understandable for someone who systematically uses ostraca in research on monastic centres and has for a long time suspected that there was something that can be called a culture (or perhaps, better said, a custom) of potsherds. The number of ostraca published or ready for publication keeps growing as a result of increasingly careful archaeological investigation; as Bagnall points out, their retrieval requires special caution on the part of the archaeologists, for they are not always immediately visible in refuse or fill layers.

Ostraca come to light on many sites in the Mediterranean and in Iran. In Egypt they existed before the coming of the Greeks, but there was no place for them in Arab Egypt after the 8th century, when the scale of everyday writing decreased. In general, ostraca are the ideal writing support for various short, temporary, purely functional texts. Since they keep well in a geographical setting where rain is scarce and the material used is free and ubiquitous, they should be everywhere. But they are not.

<sup>20</sup> See for instance: P. CECCARELLI, 'Messaggio scritto e messaggio orale: strategie narrative erodotee', [in:] M. GIANGIULIO, (ed.), *Erodoto e il «modello erodoteo». Formazione e trasmissione delle tradizioni storiche in Grecia*, Trento 2005, pp. 29–58, and M. DANA, 'Lettre sur plomb d'Apatorios à Léanax. Un document archaïque d'Olbia du Pont', *ZPE* 148 (2004), pp. 1–14

Bagnall somewhat prematurely assumes that good excavations always yield ostraca; they most certainly do not. Works at Naqlun have been conducted with great care from the very beginning, but ostraca were very few, although a monastic environment of exactly the same type as the ostraca-yielding Monastery of Phoibammon in Western Thebes should be rich in inscribed potsherds. There are no ostraca in the hermitages of Esna and Kellia (but there are numerous graffiti and dipinti on walls, so we are without a doubt dealing with dwellings of people who can write). We can give multiple examples while waiting for an explanation, which I cannot say that I can offer.

Studying the manner of administrating a large monastery like Bawit teaches us to what extent the choice between ostrakon and papyrus can depend on various factors, including the nature of the texts. Invoices handed to camel- and donkey drivers (probably barge operators as well) transporting wine, grain and other goods were usually written on ostraca, but orders for payment or issue of goods from storerooms of the same monastery were written on papyrus. This cannot be incidental. Alain Delattre found a sensible explanation for this phenomenon: ostraca were disposed of after the cargo was registered, but orders for payment/issue were kept, at least for some time, in the office of the management of the monastery's economic affairs (*diakonia*). Papyrus had the advantage of taking up less archive space and sheets needed for such small texts could be obtained by cutting up large scrap documents.<sup>21</sup>

A significant place in Bagnall's work is occupied by the analysis of a phenomenon occurring in the Middle East and consisting in the emergence of languages other than the dominant metropolitan tongues, Greek and Latin, as vehicles for both literary and everyday written expression. From these languages he selects two for case study: Coptic and Syriac. Coptic, more familiar to a historian of Egypt like Bagnall, is discussed with great care (albeit on few pages) and with new ideas about the circumstances in which it emerged and gained popularity. Bagnall has long been a defender of the hypothesis that until the Arab invasion Coptic was a lan-

<sup>21</sup> *Papyrus coptes et grecs du monastère d'apa Apollo de Baouït conservés aux Musées Royaux d'Art et d'Histoire de Bruxelles*, Bruxelles 2005, pp. 165–166.

guage of private communication used primarily in monastic environments. Leslie MacCoull, who in her numerous articles has argued to make Coptic writing equal in status to the Greek, has done it *per fas et nefas* against statistical obviousness, only to glorify Coptic culture. Bagnall's argument shows how dangerous such an approach is, how it leads to a deformation of the image of the Byzantine period and, most of all, of the important phenomenon of bilingualism, and demonstrates that, if we do not take it into account, we cease to understand the society of this period.

The reading of chapter 3 'Documenting Slavery in Hellenistic and Roman Egypt' must be intimidating to the non-papyrologist. Bagnall chose this topic to show the reader how to use papyri when working on a specific problem; how often one needs to ask about the criteria decisive in making the document (mostly deeds of sale and manumission of slaves); what part of them may have been preserved and where; how much of it may have been published and whether or not it was done competently enough. He patiently argues that the papyri at our disposal do not provide a photographic image of the reality, that a simple summary of texts does not bring forth knowledge of social and economic life. I realise that for those who follow in his footsteps Bagnall sets high standards, which they will meet only after completing a training under the master's watchful eye.

What of people who are full of good will and would like to use papyri in various situations, but have not had the privilege of training long enough? They cannot learn the art on their own. Such researchers should find works of papyrologists who interpret the data from papyri in such a way that they would be able to safely use it for their own benefit and for their readers. There is a need for many scholars capable of playing the role of middlemen, as Bagnall has done.

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