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**WHY WRITE A NEW HISTORY OF BYZANTIUM?**

**WARREN TREADGOLD**

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**In charge of the publication: Helen Saradi, Department of Languages and Literatures,  
University of Guelph, Guelph, Ontario, N1G 2W1 Canada**

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## WHY WRITE A NEW HISTORY OF BYZANTIUM?

Warren TREADGOLD

For the last ten years, I have been writing a new history of Byzantium. Now completed, it is entitled *A History of the Byzantine State and Society*, runs to a bit over a thousand pages, and is scheduled for publication in June.<sup>1</sup> You will probably believe me when I say that it was difficult to write, and that I had at least a dim conception of how difficult it would be when I decided to write it. From the start, however, I believed that there were good reasons for doing it, and, luckily for my peace of mind, I still believe in those reasons. Yet the fact that no one had written a similar book for sixty years, since George Ostrogorsky finished his *History of the Byzantine State*, suggests that the need for it was not obvious to everyone.

More than twenty years ago, Alexander Kazhdan, a Byzantinist for whom I have great respect, observed that even then the absence of a new political history of Byzantium was "surprising." His explanation was not that Ostrogorsky remained satisfactory, but that "our generation does not relish the history of wars, upheavals and religious disputes. We no longer believe that the core of the past can be reached through even the finest analysis of political events. Instead, the fashion is for the history of civilization, the history of man in a broader perspective."<sup>2</sup> Kazhdan's comments do help explain why no one has recently written a general history of Byzantine

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<sup>1</sup> Warren Treadgold, *A History of the Byzantine State and Society* (Stanford: Stanford University Press, 1997, forthcoming). The present lecture is a revised and much expanded version of a paper that is to appear, under the title "Observations on Finishing a General History of Byzantium," in the Festschrift for Cyril Mango (forthcoming in 1997 from B.G. Teubner, Stuttgart).

<sup>2</sup> A. Kazhdan, "Approaches to the History of Byzantine Civilization: From Krause to Beck and Mango," in A. Kazhdan and S. Franklin, *Studies on Byzantine Literature of the Eleventh and Twelfth Centuries* (Cambridge, 1984), p. 1.

politics alone; my own work treats not only the state but society as well. Yet no one had written a broader history of this sort, either.

To be precise, two multivolume histories have appeared recently, both essentially limited to political history and neither well regarded by most scholars. The three volumes by John Julius Norwich (recently made available in a one-volume abridgement) make, in the author's words, "no claim to academic scholarship."<sup>3</sup> In fact, they ignore most recent research and are largely compiled from Edward Gibbon's *Decline and Fall of the Roman Empire*. Aikaterene Christophilopoulou's history of Byzantium, which is still in progress both in its Greek original and in its English translation, clearly shows its origins as a teaching manual.<sup>4</sup> Neither Norwich nor Christophilopoulou has really come to terms with modern Byzantine scholarship, which in turn has more or less ignored both of them.

Several other long and serious general books on Byzantium have been written, none much like my own. Probably the most important, published in 1991, is the three-volume *Oxford Dictionary of Byzantium*, edited and in large part written by Alexander Kazhdan himself. Unlike most such dictionaries, it has a reasonably consistent point of view, briefly outlined by Kazhdan under "Byzantium, history of." The articles on individual emperors could even be read in order as a political history; but it would be a peculiar one, paying disproportionate attention to personalities and to minor rulers, given that every emperor's article is about a column long. The *Dictionary's* alphabetical format seldom allows chronological treatment, or much discussion of connections between social and political developments. All of which is simply to say that this excellent dictionary is not a continuous history. No coherent narrative could provide as much detail as the *Dictionary* does on such topics as costume or food - to say nothing of concepts like "risk" or "sin." But just as in most respects a history cannot substitute for a dictionary, a dictionary cannot replace a history.

Another approach can be found in Cyril Mango's *Byzantium: The Empire of New Rome*, published in 1980. This is the latest and best of a series of analytical surveys of Byzantine civilization, and it gives an inspired picture of Byzantine society and culture.<sup>5</sup> Kazhdan has criticized the book,

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<sup>3</sup> J.J. Norwich, *Byzantium* (3 vols.: London, 1988-96), with the quotation from the first volume, p. 28.

<sup>4</sup> A. Christophilopoulou, *Byzantine History*, 2 vols. (Amsterdam, 1986-93; in progress), trans. of same, *Byzantinê Historia*, 2 vols. (Athens, 1975-88; in progress).

<sup>5</sup> Notable among its predecessors are A. Guillou, *La civilisation byzantine* (Paris, 1974), L. Bréhier, *Le monde byzantin* (3 vols., Paris, 1949-50), and N.H. Baynes and H.S.B. Moss, *Byzantium: An Introduction to East Roman Civilization* (Oxford, 1948).

gently, for implying that Byzantine culture did not change much over its millennial history, even though Mango actually believes that Byzantium changed a great deal. Kazhdan suggests that the "difficulty arises from the structure of Mango's book rather than from its content."<sup>6</sup> Since each of Mango's fourteen topical chapters covers the whole Byzantine period, his descriptions of such things as Byzantine monasticism, eschatology, or literature cannot show in any detail how each of these developed over time, much less how each related to the others or to politics. This is an inevitable disadvantage of topical rather than chronological organization. Although a topical survey can describe individual aspects in a depth that no history can match, it too is no substitute for a history.

There is one Byzantine history more recent than Ostrogorsky's that might seem to combine all the elements one could wish: the second edition of the fourth volume of the *Cambridge Medieval History*, written by a team of over two dozen scholars and published in two parts in 1966 and 1967.<sup>7</sup> Yet it has never enjoyed particularly high esteem as a whole, though some of its chapters are very well done. For one thing, it lacks any consistent focus or overall point of view. Its first part, *Byzantium and Its Neighbours*, devotes more space to the neighbors than to Byzantium, while each account of Byzantine relations with another power is told from a different perspective. All these chapters suffer from the usual disadvantages of topical organization, as do the chapters in the second part, *Government, Church and Civilization*. Another reason for the book's lack of coherence lies in its being a collaboration, with a different author for nearly every chapter. Even though Kazhdan partly overcame a similar problem by his vigorous editing of the *Oxford Dictionary of Byzantium*, a work by many authors faces obvious obstacles in presenting general ideas.

This brings us back to Ostrogorsky's *History*, which is still the reigning political history of Byzantium, and justly admired. Its original German version was completed in 1937 and published in 1940.<sup>8</sup> Revised editions appeared in 1952 and 1963, each of which was soon translated into English and other languages. The basic organization and conception of the book, however, remained unchanged by the revisions. Moreover, the book was planned, as a volume of the *Handbuch der Altertumswissenschaft*, to

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<sup>6</sup> Kazhdan, "Approaches to Byzantine Civilization," pp. 15-16.

<sup>7</sup> *The Cambridge Medieval History*, vol. 4, 2nd ed., ed. J.H. Hussey (Cambridge, 1966-67).

<sup>8</sup> For his date of completion, see Ostrogorsky, *Geschichte des Byzantinischen Staates* (Munich, 1940), p. ix.

focus on politics and to pay only minimal attention to the period before the year 610. Ostrogorsky, not considering 610 an appropriate starting date for Byzantine history, included a brief introductory chapter that begins with the foundation of Constantinople in 324. In his narrative he even tried, as he put it, "to bring out the essential interdependence of events at home and abroad, political, ecclesiastical, and cultural."<sup>9</sup> Yet his treatment of social and cultural history is very sparse. Moreover, given that even the thirty-four years since the book's last revision have been eventful ones in Byzantine scholarship, most Byzantinists recognize that Ostrogorsky's book has become largely outdated.

In writing my own book, I parted company with Ostrogorsky in several ways, beyond simply taking subsequent research into account. I gave the early Byzantine period much more space than Ostrogorsky had been able to do, the middle Byzantine period about the same space, and the late Byzantine period somewhat less (Ostrogorsky, who finished writing his history in Belgrade, had given relations with the Serbs particular prominence). My plan was to produce a book only a little longer than Ostrogorsky's, while assigning each period treatment more nearly corresponding to its importance (at least for a non-Serbian readership). In the end, my book turned out to be about a third longer than Ostrogorsky's.<sup>10</sup> Because my narrative chapters were almost exactly as long as his, the extra length was due to six long general chapters for which I used a topical format. For those who find that a thousand pages is more Byzantine history than they want, I have recently arranged to write a history that will cover the same ground but be about a third as long.<sup>11</sup>

Much like Ostrogorsky, I tried to write a chronological account that would integrate political, social, religious, cultural, and economic history as much as possible. In practice, however, for me as for him, this turned out to be possible only to a limited extent. Detailed chronological accounts must deal with events that happened at fairly specific times; but most social, religious, cultural, and economic developments occurred gradually, often independently of any specific event. A related problem is that conditions

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<sup>9</sup> Ostrogorsky, *History of the Byzantine State* (New Brunswick, 1969), p. vii (from the preface to the first edition).

<sup>10</sup> I exclude my illustrations and maps, which add about 150 pages. Ostrogorsky's German editions are not illustrated, and the illustrations in his American edition appear on unnumbered pages.

<sup>11</sup> This book, tentatively entitled "The Phases of Byzantine History," is to be published by Macmillan, as part of the series *European History in Perspective*, edited by Jeremy Black.

were often very different in different parts of the Byzantine Empire. Yet making space for frequent discussions of what was happening in each region would have rendered the narrative almost impossible to follow.

I therefore divided my book into six chronological parts, covering, on average, about two hundred years apiece. This span seemed long enough to permit generalizations about gradual developments, and short enough to allow the generalizations to be fairly specific. Each part consists of three or four narrative chapters, followed by a longer topical chapter on society in general. Each of these topical chapters first surveys the empire's regions one by one, then discusses social structures, religion, culture, and economics. Although similar formats have been used for general histories of other civilizations, for a general history of Byzantium this sort of organization seems to be a novelty. In fact, the very idea that most aspects of Byzantine civilization changed much over time is still disputed, and even those who believe in changes argue over what the changes were.

My choice of periods for Byzantine history mattered more for me than Ostrogorsky's or Kazhdan's did for them, because my periods determined not just divisions in my narrative but the scope of my general chapters. The periods had to define stages of development in both political and social history. I also had to decide where Byzantine history started. The term "Byzantine Empire" is a modern one, since the Byzantines themselves always called their empire the Roman Empire, of which it was a direct continuation. Ostrogorsky took the common-sense view that the Byzantine period began with the foundation of Constantinople in 324 on the site of the ancient city of Byzantium. However, since Ostrogorsky wrote, Cyril Mango and Gilbert Dagron have demonstrated that Constantinople did not become anything like a true capital for the eastern Roman Empire until more than fifty years after its foundation.<sup>12</sup> Besides, few scholars think that having Constantinople for a capital was the most important distinction between Byzantium and the earlier Roman Empire.

Since Ostrogorsky's time a number of historians, led by Kazhdan, have suggested that the true beginning of the Byzantine period came in the middle of the seventh century. They note not just the empire's loss of Syria and Egypt to the Arabs and most of the Balkans to the Slavs, but a severe contraction in the size of the empire's cities under the pressure of enemy raids. In their opinion, the empire became so much less urbanized, and its

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<sup>12</sup> Cyril Mango, *Le développement urbain de Constantinople* (Paris, 1985); Gilbert Dagron, *Naissance d'une capitale: Constantinople et ses institutions de 330 à 451*, 2nd ed. (Paris, 1984).

economy so much less monetarized, as to make it essentially unlike what it had been in ancient times. They accordingly favor calling the empire "Roman" before the seventh century and "Byzantine" afterwards. Others, myself among them, regard this case as somewhat overdrawn. Since everyone agrees that important changes occurred in the seventh century, the dispute is over the extent of the changes and how much significance to attach to them.<sup>13</sup>

Despite his views on the seventh century, Kazhdan did not go so far as to exclude the earlier period from his *Oxford Dictionary of Byzantium*, where the first emperor given a regular biographical entry is Diocletian, emperor from 284. Diocletian's reign is also the point where I begin my history, for several reasons. First, within a year of his accession Diocletian made an administrative division of the Roman Empire into eastern and western parts that turned out to be lasting, the eastern part becoming what we call Byzantium. Diocletian also made far-reaching reforms in the army, bureaucracy, and system of taxation, the effects of which continued far into the Byzantine period. Though the time before him was an age of anarchy, he established a stability and a much more intrusive government that continued after him. Finally, the simple fact that his reign is the first that included some characteristically Byzantine elements makes it a good place to start, because any later starting point would require many awkward references back to him.

Although I agree with Ostrogorsky, Kazhdan, Mango, and most other Byzantinists that the early seventh century was an important turning point, the span of more than three hundred years from 284 to the early seventh century seemed too long to analyze as a single period. I therefore divided it at the mid-fifth century, again for several reasons. Before that time the spread of Christianity and relations with the Western Roman Empire were major themes in Byzantine history, while afterward Christianity had firmly established itself and the Western Empire collapsed and disappeared. The economic picture is also different before and after the mid-fifth century. Before, the eastern empire's economy was plainly struggling, and had probably registered at least a small decline since 284. By the mid-fifth century, however, the archeological record shows signs of economic growth, which continued for almost a century until it was cut short, evidently by the bubonic plague. My reason for not adopting the plague as a division is that its impact can best be shown by discussing how

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<sup>13</sup> For an account of the controversy, see my "The Break in Byzantium and the Gap in Byzantine Studies," *Byzantinische Forschungen* 15 (1990), pp. 289-316.

the empire was before, during, and after it.

In the seventh century the Persian and Arab invasions brought a crisis even worse than the plague, endangering the empire's very existence. My next period is therefore the empire's struggle for survival, up to the time when that survival was no longer in doubt. The date I chose for the passing of the crisis was around 780, which in a previous book called *The Byzantine Revival* I argued was the beginning of an economic and cultural recovery. In this I disagreed with Ostrogorsky and the editors of the *Cambridge Medieval History*, who chose 842 as the beginning of the revival; Kazhdan is somewhat non-committal, putting the "first beginnings" between 800 and 850.<sup>14</sup> As with my choice for a starting date for Byzantine history, the difference may simply be my preference for starting to discuss a gradual development with the first definite beginnings rather than a more advanced stage.

Like just about everyone, I date the end of Byzantium's military recovery to 1025, when major new conquests stopped after roughly doubling the empire's size. The subsequent period used to be considered one of unrelieved decline; but recently a consensus has been emerging - to which I subscribe - that economic expansion continued despite political setbacks that included the loss of inland Asia Minor by 1081. The loss of the interior of Asia Minor represented a grave political and military failure, but not a great economic blow, because the Anatolian plateau was a relatively poor region. As I note in my book on *Byzantium and Its Army*, this loss was accompanied by the virtual disintegration of the Byzantine army, which was never the same afterward.<sup>15</sup> Otherwise Byzantium was not greatly changed, because it kept most of its wealthiest provinces. The first setback to have such profound results that it spelled the end of an age was the fall of Constantinople to the knights of the Fourth Crusade in 1204.

With the fall of Constantinople to the Crusaders, what had been Byzantium split into four main parts: the Crusaders' Latin Empire and three Byzantine splinter states, each of which soon claimed to be the successor to the Byzantine Empire. One of them, which we call the Empire of Nicaea after its capital, eventually captured Constantinople, persuading modern scholars to call it the restored Byzantine Empire. This empire was indeed a lineal descendant of the state the Crusaders had dismembered in 1204, and at first held more than half of Byzantium's old territories. It lasted almost two hundred years, and only succumbed to the Turks after being crippled

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<sup>14</sup> "Byzantium, History of," p. 346.

<sup>15</sup> See Treadgold, *Byzantium and Its Army* (Stanford, 1995), pp. 214-19.

by a recurrence of the bubonic plague - the Black Death. Yet there were two other lineal descendants of Byzantium, one of which, the little Empire of Trebizond, actually survived eight years after the Turks took Constantinople in 1453. Since my book includes some treatment of all three successor states, it ends only with the fall of Trebizond in 1461.

The attention I pay to economics, and to differences between regions of the empire and its successor states, seem to help resolve some of the old disputes over Byzantine decline. For example, some historians have argued that abandonment of land was a general problem in the fourth and fifth centuries because of evidence from the Balkans; a regional survey reveals that the Balkans were exceptional, because unlike the rest of the eastern empire they suffered constantly from enemy raids. Other historians have argued that the empire was still prosperous in the late sixth century because of evidence from inland Syria; but this too was an exceptional region, because its dry climate was inhospitable to the rats that carried the plague.

Still other historians have argued over evidence that seems to show both Byzantine strength and weakness in the eleventh and twelfth centuries; but as soon as economic prosperity is clearly distinguished from military power, it becomes clear that the empire was at once economically strong and militarily weak. The same was true for most of the rest of the empire's history, and explains why the Ottoman Turks, by devoting more resources to their army than the Byzantines had done, built a far stronger empire on the same land. I consequently disagree with the usual judgments that Byzantium was doomed long before it fell. Its demise became foreseeable only after the Black Death devastated it in 1347, while largely sparing its Turkish enemies.

In a book entitled *A History of the Byzantine State and Society*, I obviously needed to come to conclusions, however tentative, about Byzantine social history. My treatment may well seem unsatisfactory to those historians who want some clear theoretical model to apply to all of Byzantine society. My own view is that no such model is applicable. What many scholars are looking for seems to be a system of class divisions of the sort found in traditional or Marxist interpretations of Western European feudalism. But Byzantium, like the ancient world, had nothing much like feudalism, and its class divisions, though they certainly existed, seem to have been relatively unimportant. Several class-based interpretations of Byzantine history seem to be almost entirely unfounded.

The recent theory that in Late Antiquity bishops led some sort of social revolution against the secular ruling class of city councilors seems to be based on a series of misinterpretations. It idealizes the city councilors and

ignores overwhelming evidence that they were extremely eager to abandon their financially burdensome duties, some of which the Church assumed by default.<sup>16</sup> Another idea that seems baseless, but has long been standard and was adopted by Ostrogorsky, is that the twelfth century was an age of domination by aristocratic landholders.<sup>17</sup> There were many aristocratic landholders then, but they showed no more sense of class solidarity than the earlier city councilors had had. Although the ruling dynasty of the Comneni were indeed landholding aristocrats, they seem to have had scarcely any interest in helping others of their class, who in turn had no love for the Comneni.

Later, in the thirteenth century, some historians have seen class warfare between rich and poor in the Byzantine civil war between supporters and opponents of the emperor John VI Cantacuzenus. What the sources show instead is a dynastic conflict with some elements of a struggle between the towns and the countryside; rich landholders and the rural poor largely backed Cantacuzenus, while rich merchants and the urban poor largely opposed him. The war was between two coalitions, each of which included both rich and poor. In short, the Byzantines seem to have cared much less about class than some modern historians think they should have.

At this point, let me stress that I am well aware that my book is very far from being definitive. Obviously I corrected mistakes when I identified them, but I often made suggestions when the evidence was unclear, because the alternative was to avoid some important questions altogether. As I observe in my preface, "about a quarter of the statements that I qualify by [the word] 'probably' are probably wrong."<sup>18</sup> It will surely be my fate, as it has often been Ostrogorsky's, to be quoted by others in order to be refuted by evidence that I overlooked or misunderstood. Specialists who know their parts of Byzantine history better than I do are particularly likely to find the mistakes in their specialties.

However, this is one of the best reasons for producing new general histories, at least every sixty years or so. Writing a general history is a bit like writing a complete translation of a text. Monographs, like paraphrases, can fudge the hard parts to hide ignorance and misunderstanding. A comprehensive history, even by its weaknesses and errors, can reveal where

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<sup>16</sup> See my "Imaginary Early Christianity," *International History Review* 15 (1993), pp. 535-45, reviewing Peter Brown, *Power and Persuasion in Late Antiquity* (Madison, 1992).

<sup>17</sup> Thus in his history Ostrogorsky entitled his treatment of the period from 1081 to 1204 "The Rule of the Military Aristocracy."

<sup>18</sup> Treadgold, *History* (as in n. 1 above), p. xvi.

the worst problems are. Many of the problems that remain in Byzantine history have no doubt escaped my notice and will be seen only by critics who read my book. But some of the problems seemed clear enough to me as I wrote, and I either mentioned them in my text, notes, or bibliography, or reserved them for treatment in another context, such as this lecture. So, in the time that remains, I would like to draw upon what I learned from writing my history to make some general remarks on the present state of Byzantine studies.

My first observation, which applies to many other fields as well, is that writing a survey of any kind is more difficult than writing a monograph, given comparable lengths and comparable conscientiousness. Those who write specialized studies can choose topics that interest them, and can ignore material unrelated to their chosen topics. But those who write general surveys must study many topics, including some not of much interest to them, and a vast body of secondary literature which, though repetitive on some subjects, neglects others that need to be treated in a survey. As the specialist works, he becomes more familiar and comfortable with his subject. The generalist, however, if he is ever to finish writing, as soon as he becomes fairly familiar with one topic must move on to the next, which is again unfamiliar. Unlike the specialist, the generalist must constantly decide what to put in and what to leave out, and must find ways to combine into a coherent composition things that really have little to do with each other.

Now I understand why Richard Krautheimer began the preface to his survey of Byzantine architecture with the words, "This book was difficult to write, and I can truthfully say that I have never faced a harder task."<sup>19</sup> Ostrogorsky, who finished writing the first edition of his Byzantine history at age 35, reportedly said in later life that he was glad he had written it when he was young, because he would never have dared to do it when he was older.<sup>20</sup> Much of the reason that Ostrogorsky's survey has remained unreplaced for so long is surely that with the growth of the secondary literature surveys have become increasingly hard to write. To survey Byzantine history today requires an unusual degree of either perseverance or carelessness, or perhaps both. Surveys cannot have become infrequent simply because they have gone out of style, since any survey that follows

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<sup>19</sup> Krautheimer, *Early Christian and Byzantine Architecture*, 4th ed. (Harmondsworth, 1986), p. 11 (preface to the first edition, dated 1963).

<sup>20</sup> For his date of completion (1937), see Ostrogorsky, n. 8 above. (By contrast, I finished writing my history in 1993 at the ripe age of 44.)

current trends in the secondary literature will be fashionable almost by definition.<sup>21</sup> Kazhdan's remark that political history has become unfashionable explains the rarity of political histories of all kinds, including monographs, but not the rarity of all surveys, which after all could be surveys of social history.

Perhaps paradoxically, one of the main reasons for the underdevelopment of Byzantine social history is probably current boredom with political history. Social history cannot exist in isolation; social historians have only been able to act as if it did in fields where the political history has already been well established. Even if the idea that the seventh-century invasions transformed Byzantine society has been pushed too far, it at least recognizes that political events can have social consequences. That recognition should be enough to show that whatever we say about Byzantine society needs to be qualified by considerations of time and place. And this we often are ill prepared to do.

Looking over the secondary literature of the whole Byzantine period shows how uneven the coverage is, and how many important and interesting subjects await treatment. Too many scholars seem to share the attitude of a questionnaire circulated in 1977 by the then Director of Dumbarton Oaks, which asked Byzantinists to rank which subjects were most "promising and productive," without distinguishing the two. The unquestioned assumption was that the subjects that have been studied most in the past are automatically the most promising for the future. But even in a relatively underdeveloped field like Byzantium, the reverse is more likely to be true: the most promising subjects are those that so far have been studied least. On the other hand, as a prediction of what will be done, a repetition of what has already been done is always a good guess.

Thus, though we have no book on the reign of Leo VI (886-912), five books on the reign of Julian (361-63) appeared between 1976 and 1986 to replace an earlier book that was already one of the best studies of a Byzantine ruler.<sup>22</sup> Both reigns have fairly abundant sources, including

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<sup>21</sup> E.g., Judith Herrin's *The Formation of Christendom* (Princeton, 1987), and Averil Cameron's *The Later Roman Empire, A.D. 284-430* (Cambridge, Mass., 1993), and *The Mediterranean World in Late Antiquity, A.D. 395-600* (London, 1993).

<sup>22</sup> The recent books are Robert Browning, *The Emperor Julian* (Berkeley, 1976), Constance Head, *The Emperor Julian* (Boston, 1976), G.W. Bowersock, *Julian the Apostate* (Cambridge, Mass., 1978), Polymnia Athanassiadi-Fowden, *Julian and Hellenism* (Oxford, 1981), and Edgar Pack, *Städte und Steuern in der Politik Julians* (Brussels, 1986); the earlier book was J. Bidez, *La vie de l'empereur Julien* (Paris, 1930).

many writings by the emperors themselves, but the abundance is greater for the much longer reign of Leo. Even if Leo the Wise may not be quite as intriguing a figure as Julian the Apostate - this largely depends on how much one dislikes Christianity - Leo surely had the more attractive personality. The effects of Julian's reign were mostly ephemeral, while Leo's reign had lasting influence on Byzantine law and encyclopedism, on the history of the church and army, and on court ceremonial. A major study of the life, times, and legend of Leo the Wise might even find more readers than a seventh book on Julian. But at the stage when subjects are chosen (and funded), past interest spurs present interest, and past neglect breeds future neglect.

While this is a problem for the Byzantine field as a whole - none of which is truly fashionable - several phases are especially underworked. Comprehensive surveys are few nowadays, for reasons already mentioned here, but nothing else can quite duplicate their functions, because studies of aspects of history almost inevitably omit context. These studies may still be fully valid, as long as the context has already been established, but that presupposes either an earlier survey that the author can draw upon, or the author's willingness to establish the background for his chosen subject. Some works on Byzantium do display such patience, like Jean-Claude Cheynet's study of political power from 963 to 1210 or Alan Harvey's study of economic expansion from 900 to 1200.<sup>23</sup> But even these books are of limited use for different aspects of their periods than the ones they treat.

As I have already observed, most scholars who write specialized articles or monographs avoid subjects that are difficult or boring or not thoroughly covered before. We have so many studies of the causes of Iconoclasm that Peter Brown has observed that "the Iconoclast controversy is in the grip of a crisis of over-explanation."<sup>24</sup> Yet we have no detailed survey, in a monograph or a larger book, of the reign of Leo III (717-741), who introduced Iconoclasm.<sup>25</sup> Cyril Mango has noted that most of the sources for Iconoclasm still "need to be properly edited and analysed in detail," and has suggested that this "task ought to take precedence over the

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<sup>23</sup> J.-C. Cheynet, *Pouvoir et contestations à Byzance (963-1210)* (Paris, 1990); Alan Harvey, *Economic Expansion in the Byzantine Empire, 900-1200* (Cambridge, 1989).

<sup>24</sup> Peter Brown, "A Dark-Age Crisis: Aspects of the Iconoclastic Controversy," *English Historical Review* 88 (1973), p. 3.

<sup>25</sup> Karl Schenk, *Kaiser Leon III: Ein Beitrag des Bilderstreits I* (Halle, 1880), which stops with the year 720, hardly qualifies as a survey of the reign.

formulation of any further interpretations of Iconoclasm.<sup>26</sup> But the warnings of Brown and Mango have generally gone unheeded.<sup>27</sup>

If we consider the number of satisfactory surveys of periods, including both studies of individual emperors' reigns and works of wider scope, we find gaps that cover about half of Byzantine history. For the first three centuries we have two monumental works: that of Ernest Stein, with comprehensive political history and some social and economic history up to 565, and that of A. H. M. Jones, with comprehensive social and economic history and some political history up to 602.<sup>28</sup> Their coverage is not uniform, and they are about fifty and thirty years old respectively, but more specialized studies supplement them for much of the time they cover.<sup>29</sup> For the last two centuries, from 1261, we have nearly comparable coverage in the recently revised work of Donald Nicol, plus some more specialized studies.<sup>30</sup> For the 650-odd years in between, we are much less well served. Even in the rare cases when good specialized studies exist, they usually provide insufficient background to show their period's significance.<sup>31</sup>

Although two authors have written comprehensive works on the seventh century, few of us are satisfied with the five volumes of Andreas Stratos, an amateur in both the good and the bad sense of the word, and many of us think John Haldon's book is warped by a dogmatic (if somewhat heretical) Marxism.<sup>32</sup> But after 711 we miss them, because there is nothing

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<sup>26</sup> Cyril Mango, "Historical Introduction," in Anthony Bryer and Judith Herrin, eds., *Iconoclasm* (Birmingham, 1977), p. 6 n. 12.

<sup>27</sup> For an early example of the continuing literature, see John Haldon, "Some Remarks on the Background to the Iconoclast Controversy," *Byzantinoslavica* 38 (1977), pp. 161-84.

<sup>28</sup> Ernest Stein, *Histoire du Bas-Empire*, 2 vols. (Paris, 1949-59); A.H.M. Jones, *The Later Roman Empire: A Social, Economic, and Administrative Survey*, 3 vols. (Oxford, 1964).

<sup>29</sup> For a full bibliography, see now the handbook of Alexander Demandt, *Die Spätantike: Römische Geschichte von Diocletian bis Justinian* (Munich, 1989).

<sup>30</sup> D.M. Nicol, *The Last Centuries of Byzantium, 1261-1453*, 2nd ed. (Cambridge, 1993).

<sup>31</sup> I leave out of account Romilly Jenkins, *Byzantium: The Imperial Centuries, A.D. 610-1071* (London, 1966) and Mark Whitow, *The Making of Orthodox Byzantium, 600-1025* (London, 1996), which are for most purposes inferior to Ostrogorsky's general history; cf. my review of Whitow in *International History Review* 19 (1997; forthcoming).

<sup>32</sup> A.N. Stratos, *Byzantium in the Seventh Century* (Amsterdam, 1968-80); John Haldon, *Byzantium in the Seventh Century: The Transformation of a Culture* (Cambridge, 1990); cf. my review of Haldon in *American Historical Review* 97 (1992), pp. 829-30.

comparable for most of the eighth century. Often we are forced to use the two studies of Iconoclasm by Stephen Gero as if they were general surveys, which they were never meant to be.<sup>33</sup> Then we have four studies variously covering the years from 780 to 867, the old survey of J. B. Bury and newer books by Paul Speck, Pavlos Niavis, and myself.<sup>34</sup> With 867, even though the Byzantine sources are improving and the empire was expanding, the number of recent surveys becomes negligible, and even older ones become scarce.

Hardly a single book surveying any part of the period between 867 and 1081 can be recommended without reservation. Perhaps the best is Sir Steven Runciman's study of Romanus I (920-44), with defects that stem chiefly from its being almost seventy years old.<sup>35</sup> It has aged better than the four antiquated volumes of Gustave Schlumberger on the years from 963 to 1057, the most recent revision of which is just six years older than Runciman's book.<sup>36</sup> Michael Angold's history of the years from 1025 to 1204 is a creditable start, but too compressed to provide the detail that another specialist writing on the period would need.<sup>37</sup> Arnold Toynbee's work on Constantine VII (913-59) is not a history, and shows a lack of system and of focus reminiscent of the works ascribed to Constantine himself.<sup>38</sup> I omit a few not very satisfactory minor studies, but not many.<sup>39</sup>

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<sup>33</sup> Stephen Gero, *Byzantine Iconoclasm During the Reign of Leo III, with Particular Attention to the Oriental Sources* (Louvain, 1973), and *Byzantine Iconoclasm During the Reign of Constantine V, with Particular Attention to the Oriental Sources* (Louvain, 1977).

<sup>34</sup> J.B. Bury, *A History of the Eastern Roman Empire from the Fall of Irene to the Accession of Basil I (A.D. 802-867)* (London, 1912); Paul Speck, *Kaiser Konstantin VI*, 2 vols. (Munich, 1978); Pavlos Niavis, *The Reign of the Byzantine Emperor Nicephorus I* (Athens, 1987); Warren Treadgold, *The Byzantine Revival, 780-842* (Stanford, 1988).

<sup>35</sup> Steven Runciman, *The Emperor Romanus Lecapenus and His Reign* (Cambridge, 1929).

<sup>36</sup> Gustave Schlumberger, *Un empereur byzantin au dixième siècle: Nicéphore Phocas*, new ed. (Paris, 1923), and *L' épopée byzantine à la fin du dixième siècle*, 3 vols. (Paris, 1896-1905).

<sup>37</sup> Michael Angold, *The Byzantine Empire, 1025-1204: A Political History* (London, 1984).

<sup>38</sup> Arnold Toynbee, *Constantine Porphyrogenitus and His World* (Oxford, 1973). Cf. p. viii: "I have concentrated on aspects of Constantine's world in which Constantine himself evidently took a special interest, and here I have gone the most into detail in discussing interests of Constantine's that happen also to be mine...."

For the period from 1081 to 1204 matters improve. The old but precociously rigorous volumes of Ferdinand Chalandon cover the years from 1081 to 1180, and provided a solid basis for the recent work of Paul Magdalino, which has some of the elements of a survey.<sup>40</sup> From 1080 to 1204 we have the history of Charles Brand, supplemented at the end by two recent studies of the Fourth Crusade.<sup>41</sup> Then we lack a general survey of the chaotic but important events from 1204 to 1261; though we do have some competent partial accounts, ideally we should have a connected history of all the successor states, whose fates were closely intertwined.<sup>42</sup> And so we reach 1261, and Donald Nicol's survey, already mentioned.

One can cite various reasons for this mottled pattern of interest and neglect, and pure chance has surely been a factor.<sup>43</sup> In general, Late

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<sup>39</sup> Three studies of this period have appeared since I completed my own book. V. Vlyssidou, *Politique étrangère et réactions intérieures sous le règne de Basile Ier*, (Athens, 1991; in Greek, with French summary) is not a real survey and presents a schematic and distorted picture of the politics of the time. T. Koliass, *Nikephoros II. Phokas (963-969): Der Feldherr und Kaiser und seine Reformtätigkeit* (Athens, 1993; in Greek, with German summary) has its merits but is a study of Nicephorus's military policies rather than a survey. I. Rochow, *Kaiser Konstantin V. (741-775): Materialien zu seinem Leben und Nachleben* (Frankfurt, 1994), is a survey but concentrates on compiling bibliographical references rather than on illuminating its subject.

<sup>40</sup> Ferdinand Chalandon, *Les Comnènes: Études sur l'empire Byzantin au XI<sup>e</sup> et au XII<sup>e</sup> siècles*, 2 vols. (Paris, 1900-1912); Paul Magdalino, *The Empire of Manuel I Komnenos, 1143-1180* (Cambridge, 1993).

<sup>41</sup> Charles Brand, *Byzantium Confronts the West, 1180-1204* (Cambridge, Mass., 1968); Donald Queller, *The Fourth Crusade: The Conquest of Constantinople* (Philadelphia, 1977); John Godfrey, *1204: The Unholy Crusade* (Oxford, 1980).

<sup>42</sup> On Nicaea, Alice Gardner, *The Lascarids of Nicaea: The Story of an Empire in Exile* (London, 1912), is now supplemented by Michael Angold, *A Byzantine Government in Exile: Government and Society Under the Lascarids of Nicaea (1204-1261)* (Oxford, 1975). On Epirus, D.M. Nicol, *The Despotate of Epiros*, (Oxford, 1957), is supplemented by the introduction to the same author's *The Despotate of Epiros, 1267-1479: A Contribution to the History of Greece in the Middle Ages* (Cambridge, 1984). On Trebizond, William Miller, *Trebizond: The Last Greek Empire* (New York, 1926), can be supplemented to some extent by S.P. Karpov, *L'impero di Trebisonda, Venezia, Genova, e Roma, 1204-1461* (Rome, 1986).

<sup>43</sup> As a contributor to one of the worst cases of apparent scholarly redundancy, I may be able to shed a little light. I decided to write on the years from 780 to 842 because I thought Bury's survey of the years from 802 to 867 had failed to explain (or even to notice) the empire's cultural, economic, and political advances during the time. I began my research before the appearance of Speck's volumes on the years from 780 to 802, which in any case did not address the problem that interested me; Nivias's study of the

Antiquity up to the death of Justinian in 565 has been the most popular period, no doubt because Classicists have found it the most familiar. But even this cannot explain the relative neglect of the middle of that period, the century or so from about 410 to 518. We have every reason to believe that this was a time when the Eastern Roman Empire, which had seemed to be going the way of the Western, made an impressive economic and military recovery. Progress made then meant that in the following years Justinian had the money and power to fulfill many of his boundless ambitions. Although the fifth-century literary sources are somewhat meager, the archeological sources are ample, especially in Syria and Palestine. A thorough study of the fifth century could show precisely when the recovery began, and why the fourth and sixth centuries, which we know much better, were so different from each other.

One reason for the neglect of the fifth century is that a full study of it would need to include treatment in depth of both the army and the economy. Like Byzantine social history, both Byzantine economic and military history remain backward. The two subjects are related, because the large and costly Byzantine army had pervasive effects on the Byzantine economy, and vice versa; slowness to realize this fact, and the difficulty of mastering the two fields together, has contributed to the neglect of both. One who has seriously tackled both of them is Michael Hendy, whose massive collection of interlocking studies on the monetary economy almost amounts to a survey.<sup>44</sup> Published ten years ago, it has won general respect and has often been cited, but its conclusions have been less readily understood and adopted. Although part of the trouble is doubtless its bulk and intimidating style, a more serious obstacle is that it tells Byzantinists things that they are unprepared and unwilling to hear.

Hendy's economic evidence refutes several popular historical theories. He has argued, for example, that the middle Byzantine government was generally successful in keeping Anatolian magnates from buying out smallholders, that the Byzantines never really recovered any part of the Anatolian plateau from the Seljuk Turks, and that the economic scale of Italian trade within the empire was modest up to the end of the twelfth

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years from 802 to 811 appeared only after my own book had gone to press. Although the overlapping content among the books is actually rather modest, a wider distribution of scholarly effort would admittedly have been better.

<sup>44</sup> Michael Hendy, *Studies in the Byzantine Monetary Economy* (Cambridge, 1985). See my review, "Numismatics and the Ripening of Byzantine Studies," *Rivista di Bizantinistica* 3 (1993; forthcoming).

century.<sup>45</sup> He has also shown that a financial crisis in the seventh century would have left no time for the gradual introduction of land grants to soldiers, or of any other system of paying the army.<sup>46</sup> While Henny's findings have seldom been challenged directly, some continue to assume anything they please about the Byzantine economy, regardless of logic or evidence.<sup>47</sup>

Such an attitude particularly affects military history. For years historians have postulated the recruitment of new soldiers, or blamed the empire for failing to recruit new soldiers, with a blissful disregard for the costs. They have also praised the Byzantines for their conquests and blamed them for their losses without considering the numbers of soldiers on each side. The assumption has been that we cannot know anything useful about the costs or numbers. Yet in writing my general history I found so much neglected evidence for the size and cost of the army that finally I wrote a separate monograph, *Byzantium and Its Army*, on the army up to 1081.<sup>48</sup> And this is only a preliminary sketch. A recent book by Mark Bartusis has covered the army after 1261 more completely, but a gap remains between our two studies.<sup>49</sup> Much remains to be written to explain how the economy and army worked, and, most of all, how each affected Byzantine society.

To turn from periods and topics to places, the various volumes of the *Tabula Imperii Byzantini* have gradually been showing us how different the parts of the empire were from each other.<sup>50</sup> But the volumes of the

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<sup>45</sup> Michael Henny, *Studies*, pp. 100-108 and 131-38 (on the Anatolian magnates), 109-31 (on the failure to recover the plateau), and 590-602 (on Italian trade).

<sup>46</sup> Henny, *Studies*, pp. 619-662.

<sup>47</sup> E.g., John Haldon, *Byzantium in the Seventh Century*, pp. 244-51, who as a Marxist cannot accept that the empire benefited from essentially privatizing its land, has suggested that soldiers received no land but were directly supplied by the state. He fails to see that the state would only have aggravated its problems by shifting from payments in coin to payments in kind, because it would simply have incurred the extra costs of transporting supplies all over the empire.

<sup>48</sup> See n. 15 above.

<sup>49</sup> Mark Bartusis, *The Late Byzantine Army: Arms and Society, 1204-1453* (Philadelphia, 1992).

<sup>50</sup> Those published to date, all in Vienna, are: vol. I, J. Koder and F. Hild, *Hellas und Thessalien* (1976); vol. II, F. Hild and M. Restle, *Kappadokien (Kappadokia, Charsianon, Sebasteia und Lycandos)* (1981); vol. III, P. Soustal, *Nikopolis und Kephallēnia* (1981); vol. IV, K. Belke, *Galatien und Lykaonien* (1984); vol. V, F. Hild and H. Hellenkemper, *Kilikien und Isaurien* (1990); vol. VI, P. Soustal, *Thrakien (Thrakē, Rodopē, und Haimimontos)* (1991); vol. VII, K. Belke and N. Mersich, *Phrygien und Pisidien* (1990).

*Tabula* are primarily works of reference, for looking up individual sites; plenty of room remains for comprehensive regional studies, especially if they can combine geography with written sources and archeology. So far we have a few good studies of different Byzantine cities and of the islands, with some other works more limited in time span.<sup>51</sup> A book on Constantinople is on the way from Cyril Mango, and should represent a major advance in our knowledge of the best attested part of the empire.

Yet our evidence is adequate to allow a good many more regional studies. For the present, perhaps too little basic work has been done to permit satisfactory studies of all of Byzantine Anatolia or all of Byzantine Greece. But in view of the abundance of the sources it seems surprising that we still have no proper study of Mount Athos in the Byzantine period. The place is of obvious importance in itself, with a wealth of published documents and preserved monuments and objects, not to mention a living tradition continuous with Byzantium.<sup>52</sup> Other regions with archeological and literary sources that would allow them to be studied in depth - if not quite in such depth as Athos - are Cappadocia, Bithynia, Phrygia, and the Pontus.<sup>53</sup> Are these regions really less promising subjects than individual sites and monuments are?

In trying to give minimal attention to Byzantine art in my book, I was struck by the scarcity of general books on the subject. Aside from Cyril Mango's excellent survey of Byzantine architecture, nothing recent and comprehensive was available for most of Byzantine art, and the authors of most of the recent monographs made so few generalizations that little of general interest could be gathered from them.<sup>54</sup> Lyn Rodley's intelligent introduction to Byzantine art and architecture appeared after I finished

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<sup>51</sup> E.g., Clive Foss, *Byzantine and Turkish Sardis* (Cambridge, 1976), and *Ephesus After Antiquity: A Late Antique, Byzantine and Turkish City* (Cambridge, 1979), and Émile Janssens, *Trébizonde en Colchide* (Brussels, 1969); Elisabeth Malamut, *Les îles de l'empire byzantin, VIII<sup>e</sup>-XII<sup>e</sup> siècles*, 2 vols. (Paris, 1988), and D. Tsougarakis, *Byzantine Crete from the Fifth Century to the Venetian Conquest* (Athens, 1988).

<sup>52</sup> E. Amand de Mendieta, *Mount Athos: The Garden of the Panaghia* (Berlin, 1972), is mostly on the post-Byzantine period. I acknowledge that female scholars have a good excuse for not tackling the subject.

<sup>53</sup> On the Pontus, Anthony Bryer and David Winfield, *The Byzantine Monuments and Topography of the Pontos*, 2 vols. (Washington, 1985), is not meant to be a full regional study.

<sup>54</sup> Cyril Mango, *Byzantine Architecture* (New York, 1975), is the exception. The unmet need for such general observations shows in the enormous influence on secondary literature of the short introductions in Mango's sourcebook, *The Art of the Byzantine Empire, 312-1453: Sources and Documents* (Englewood Cliffs, 1972).

writing; even though she writes primarily for students rather than scholars, art historians need to pay more attention to the sorts of general issues she raises, however briefly, at the ends of her chapters.<sup>55</sup> Attempts to relate developments in art to other developments in social and cultural history have been more frequent, but mostly sporadic and superficial.<sup>56</sup> Before Byzantine art can be integrated into Byzantine history, we need a grasp of what the general developments in art were.

The history of culture brings us to literature. As Kazhdan has noted, no one has yet written even a partial history of Byzantine literature as literature.<sup>57</sup> Fortunately he and Lee Sherry have begun to write such a history (they start only with the mid-seventh century, in accordance with Kazhdan's view that the Byzantine period began then); this book may well have a major impact. For the present, however, to say that a Byzantine text has literary merit is still a controversial statement, even among Byzantinists. Most of those who work on Byzantine texts study them for a purpose that is not literary, and seem scarcely aware that a literary approach is possible. The usual assumption is that all Byzantine texts are artificial, formulaic, affected, and vapid. Art historians are particularly prone to believe this, because it is particularly true of the few texts that describe art.

Trying to find literary value in texts that have none is not the way to rehabilitate Byzantine literature. Yet that is the tendency of a recent insistence on taking Byzantine formal rhetoric seriously. Some scholars contend that the Byzantines must have admired and believed their court oratory - though strangely the same scholars refuse to accept the Byzantine theologians' explicit statements disparaging rhetoric and emphasizing content.<sup>58</sup> Similarly ignored is St. Augustine, who thought himself more miserable than a beggar in the street "on a day when I was preparing to recite the praises of the emperor, in which I would tell many lies, and be praised by those who knew I lied."<sup>59</sup> Presumably the modern approach would be to believe the orator sent by the people of Epirus to Valentinian I (364-75) to praise and thank their prefect; the emperor, unaware of modern scholarship, asked the speaker what the Epirotes really thought, and

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<sup>55</sup> Lyn Rodley, *Byzantine Art and Architecture: An Introduction* (Cambridge, 1994).

<sup>56</sup> An honorable exception is Anthony Cutler and John Nesbitt, *L'arte bizantina e il suo pubblico*, 2 vols. (Turin, 1986).

<sup>57</sup> Kazhdan, *Studies* (as in n. 2 above), p. ix.

<sup>58</sup> See my remarks in "Imaginary Early Christianity," (as in n. 15 above), especially referring to Averil Cameron's *Christianity and the Rhetoric of Empire* (Berkeley, 1992).

<sup>59</sup> Augustine, *Confessions*, VI.6.

received the answer that the prefect was a murderous tyrant.<sup>60</sup>

In some other genres, however, the Byzantines expressed themselves with more sincerity, and often with freshness and originality.<sup>61</sup> There are signs that Byzantine verse, and particularly verse romance, is now receiving a more sympathetic hearing.<sup>62</sup> But the recent interest in Byzantine saints' lives has led to their being treated more as anthropological treatises, which they certainly are not, than as literature, which many of them are. Byzantine historiography, a branch of literature to which many educated Byzantines devoted their best efforts, continues to be underrated. Among many books that need to be written, I have taken the Byzantine historians as my own next research project.

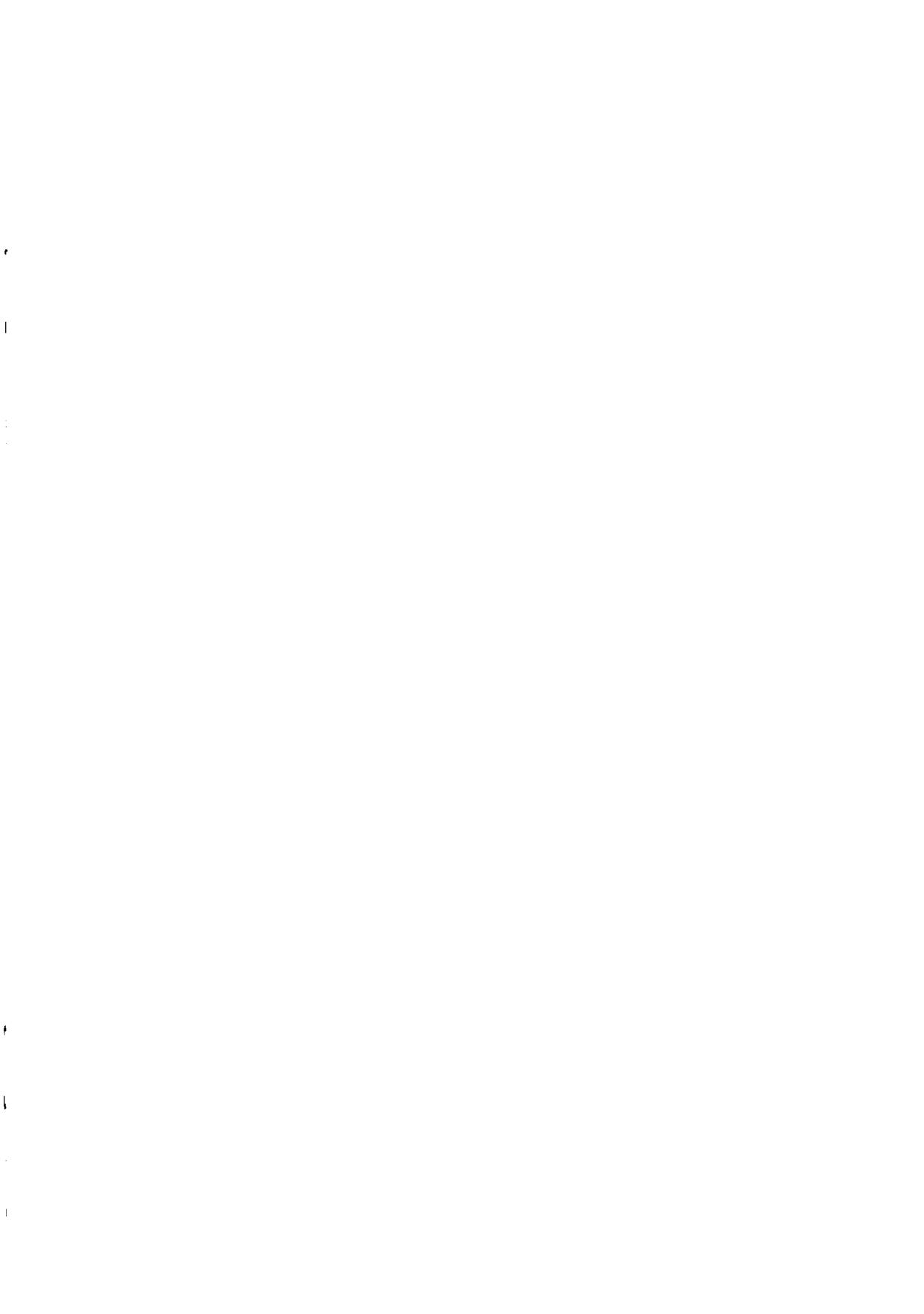
Let me add, in closing, one encouraging observation. A considerably larger share of the books and articles written on Byzantium today say something new and valuable than seems to be the case in most other fields. Even though many of the publications I encountered in researching my book were not directly useful for my purposes, something in most of them would be useful for some other purpose - I mean for a real scholarly purpose, not simply to list in a bibliography for the sake of completeness. This is the good side of the underdevelopment of Byzantine studies. In comparison with the thousandth study of Homer or Chaucer, all Byzantine research breaks new ground.

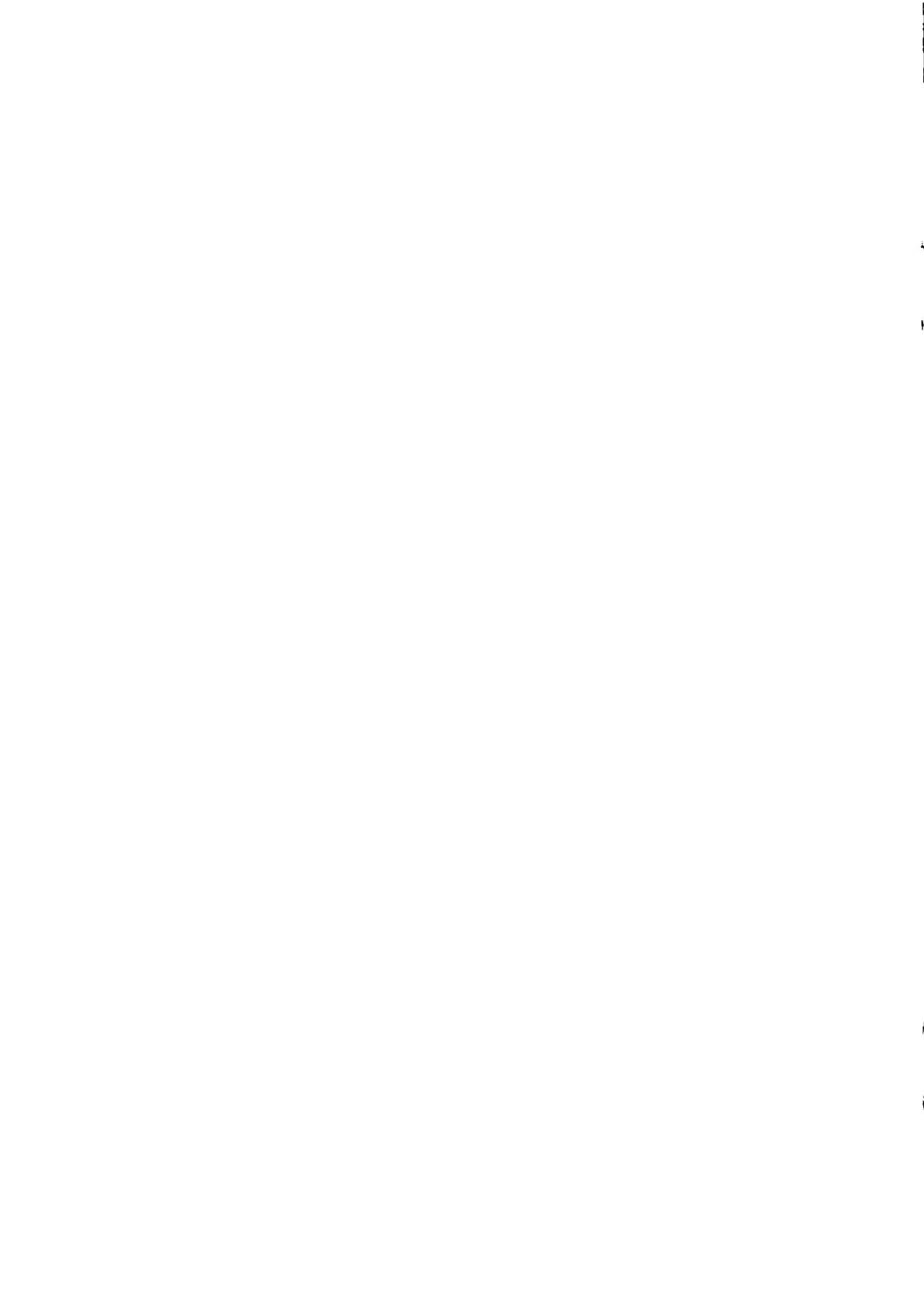
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<sup>60</sup> Ammianus Marcellinus, XXX.5.8-10.

<sup>61</sup> See now A. R. Littlewood, ed., *Originality in Byzantine Literature, Art, and Music* (Oxford, 1995).

<sup>62</sup> See R. Beaton, *The Medieval Greek Romance* (Cambridge, 1989).





Warren Treadgold is Professor of Late Ancient and Byzantine History at Saint Louis University. He is the author of *A History of the Byzantine State and Society* (Stanford, 1997), *Byzantium and Its Army, 284-1081* (Stanford, 1995), *The Byzantine Revival, 780-842* (Stanford, 1988), *The Byzantine State Finances in the Eighth and Ninth Centuries* (New York, 1982), *The Nature of the Bibliotheca of Photius* (Washington, D.C., 1980), and a number of articles on Byzantine history and literature.