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On individuals, aggregates and mute social Groups: Some Questions on Methodology

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ON INDIVIDUALS, AGGREGATES AND MUTE SOCIAL
GROUPS: SOME QUESTIONS OF METHODOLOGY

As the great Byzantinist that he was, Dionysios Zakynthinos was profoundly concerned with the essence of Byzantium and its particularities as a medieval society—a medieval Mediterranean society. To define it, he looked not only at the structure of the state, the economy, intellectual and artistic life, but also at elements such as the family and the place of women¹. Some of these components of Byzantine society include social groups that may be called mute, in the sense that they have left little record of themselves. Among them, some have long been a topic of inquiry for Byzantinists. The study of others has been accelerated in the past decades, undertaken by scholars moved partly by the publication of new sources and partly by a certain re-orientation of scholarly interest that engages historians generally, including those of ancient and medieval societies. The study of such groups poses certain methodological questions, and involves certain methodological dangers; although most of my examples will be drawn from Byzantine history, the questions and dangers are not specific to our field, but more general to the investigation of «mute» social groups.

The topic at hand is the difficulties and pleasures of trying to investigate the dynamics, realities and perceptions

1. See, for example, «Τὸ Βυζάντιον μεταξὺ Ἀνατολῆς καὶ Δύσεως», in *Byzance: État-Société-Économie*, London, Variorum Reprints, 1973, Study III.

of social groups which, for one reason or another, did not leave us much of a record of themselves, whether by themselves or by others. In some cases, their social role did not include that of record-keeper or ideologist for that society (to some extent this applies to women); in others, the documentation which members of such groups may have produced has not survived given the accidents of history — and also given the lack of interest of subsequent societies, which dictates what has survived and what has not. Such is the case of the merchants. I will not discuss here what value Byzantine society placed on their profession. It is certain that they kept accounts and that they made contracts, some of which have been published, while the terms of others can be reconstructed through records of disputes². What else they wrote we do not know, with the exception of letters³. In any case, for those documents which we know they had, the non-survival is a matter of the lack of interest of subsequent collectors; contracts are rarely beautiful, they have no theological or philosophical content, they do not contain quotations from the classics; so no one collected them. The destruction of the Byzantine state in 1453 also destroyed much of the documentation that had no aesthetic value; thus it is that the level of documentation for such matters is very low for Byzantium and significantly higher for Crete, where the Venetians carried notarial registers to safety at the time of the Ottoman conquest. On the other hand, some records of tax dues have survived, although they tend to refer to monastic properties.

The historian, therefore, is forced to manipulate data in order to reconstruct reality. Certainly, we always manipulate

2. For accounts, see P. SCHREINER, *Texte zur spätbyzantinischen Finanz- und Wirtschaftsgeschichte in Handschriften der Biblioteca Vaticana*, Città del Vaticano 1991; for contracts, see D. SIMON. SP. TROIANOS, «Dreizehn Geschäftsformulare», *Fontes Minores II*, 1977, pp. 262-295.

3. See J. DARROUZÈS, «Lettres de 1453», *RÉB* 22 (1964), pp. 72-127.

data, but for some topics the manipulation is more evident than for others. Let us examine some of the methods one can employ, and some of the difficulties involved in each.

I. Quantification and the seduction of numbers. This method involves an effort to discover hidden information by the statistical analysis, whether sophisticated or simple, of particular groups or activities (the production of manuscripts, for example). I will stay with the social groups. This method of analysis typically constructs a data base (whether with the use of the computer or not is immaterial) which expresses the characteristics of a group through a series of numerical or statistical relationships: for example, the proportion of men and women in a given population, of the correlation between wealth and life expectancy, or sex ratio and wealth, or any other categories⁴. When these relationships are elevated to the level of tables, they can form a data base consisting of information quite different from that explicitly present in the sources, and this can then be used for further descriptions of the group.

The analysis through statistics has been applied not only to mute social groups but to highly audible ones as well. As example, I bring A. Kazhdan's *Sotsial'nyi sostav gospodstvujuščego klassa Vizantii XI-XII vv.*, published in 1974. The aristocracy was studied as a group, and the prosopographical data were subjected to statistical analysis, out of which (in conjunction with other sources) conclusions were drawn regarding the structure and development of the aristocracy. Thus, for instance, a table shows the incidence of various

4. Statistical studies of western medieval society abound. See, for example, D. HERLIHY and CH. KLAPISCH-ZUBER, *Tuscans and their Families: A Study of the Florentine Catasto of 1427*, New Haven and London, 1985; or Y. BESSMERTNY, *La vision du monde et l'histoire démographique en France aux IX^e-XV^e siècles*, Paris 1991.

types of names, in the civil and military aristocracy, and from that were drawn suggestions about the professional background, place of origin and to some degree the self-identification of these two groups⁵.

If statistical analysis is useful in the case of the aristocracy, it becomes virtually mandatory when the information we have is extensive and recalcitrant. Such is the case of the peasantry, in the 13th to 15th centuries. The Byzantine peasantry is a group whose existence no-one has denied — which is not the case with some other social groups. Its history has been much studied and reconstructed from the legislation, papyri, archaeology, saints' lives, and various other sources. But for a particular time, and for specific places, we have information of the kind I call extensive and recalcitrant. This is the information found in monastic documents, primarily *praktika*, and consists of entries of the laconic type: for example: «Theodore Alvanites, has: wife, Siligno; sons, Nicholas and Ioannes; daughters, Theodora and Maria; 2 oxen; 5 cows; 1 donkey with its foal; 50 beehives; 30 goats; 3 walnut trees; another half walnut tree; 3 fig trees; half a modios of vineyard near the priest Mezinis; another half modios near Ypatios; another near this one, of a half modios; a fallow vineyard of a half modios, which he has bought from a paroikos of the monastery of Alypiou; another, near Ioannis Kofos, of half a modios. His tax is $3\frac{1}{3}$ nomismata»⁶. By itself, this piece of information is just about worthless. It gives us a few data about one family, which may or may not be typical, and there is no way for us to know what it represents. If this were the only piece of information we had, we might think that the Byzantine

5. Cf. A. KAZHDAN and G. CONSTABLE, *People and Power in Byzantium*, Washington, D. C., 1982, p. 170.

6. P. LEMERLE, A. GUILLOU, N. SVORONOS, D. PAPACHRYSSANTHOU, edd., *Actes de Lavra, II, de 1204 à 1328*, Paris 1977, p. 254, l. 566-569.

peasant family had a nuclear household, with four children, a perfect sex ratio, and a fair amount of property consisting of animals, bees, vineyards, trees, but not arable land. The picture would have been entirely different if the one surviving piece of information were «Irene of Vamvakorravdes, has son, George; 2 cows; tax, 1/6 of a nomisma» — an entry from the same *praktikon*.⁷ But when we have comparable information about many other families, the whole issue changes. Ostrogorsky used such documents to write well-known studies of the peasantry which, while mostly studies of institutions, also incorporate a very rudimentary statistical analysis. His main argument remains impressionistic: «très souvent», «plus rarement», «généralement», «essentiellement» are recurrent terms.⁸ Then came a number of studies which relied on greater degrees of quantification. The entries I mentioned can be deconstructed and reconstructed according to categories of information: name of head of household, monastery, village, names and relationships of the other members of the household; the property broken down into its constituent parts, status of the peasants. In one study of peasant society in Macedonia 63 variables were used.⁹ This number of pieces of information, multiplied many times over for all peasant households is impossible to use impressionistically: in an impressionistic view, what remains in the memory is the outliers, the exceptions, the household with eight children, or with none, not the one with two or three.

With the use of statistical analysis, one can get closer to reality: one can establish averages, which are unreal; frequencies, variations and deviations which are more real: and corre-

7. Ibid., p. 244, l. 416-417.

8. G. OSTROGORSKY, «Les praktika byzantins», *Bsl* 9 (1948), pp. 203-306.

9. A. LAIOU-THOMADAKIS, *Η αγροτική κοινωνία στην ύστερη Βυζαντινή εποχή*, Athens 1987.

lations between various categories of information, this being the most useful aspect of statistical analysis, for it permits hidden information to emerge¹⁰. But it is also the most complex, and therefore prone to risk. There is no reason to dwell here on the simple errors one can make in statistical analysis. Supposing no such errors are made, what can the scholar hope to find?

One can find out about aggregates; about how a population behaves on the whole; not about how one particular family will structure itself and its property and its relations, but about how the group can be expected collectively to behave. That is, one loses sight of the tree and gains a small forest. The advantages have been established in areas other than the history of the peasantry. N. Oikonomides' studies of literacy in Byzantium use quantification in order to escape the vague impression left by the documents, where the fact, for example, that two women were literate in Smyrna in the thirteenth century, may stand out in one's mind, until we realise that this represents 10% of the total number of women, to be contrasted to 69% for the men. Subcategories make it possible to see the role not only of gender, but of social class and of town and country¹¹. Similarly, subcategories make it possible to see variations in the property patterns or migration patterns of poorer and richer peasants, or from one village to another. So there is much that is useful in quantification; it is partly through quantification that Jac-

10. See, for example, K. V. HVOSTOVA, *Osobennosti agrarnopravovykh otnošení v pozdnei Vizantii XIV-XV vv.*, Moscow 1968, who uses correlations to establish the basis of rent.

11. N. OIKONOMIDES, «Literacy in Thirteenth-Century Byzantium: An Example from Western Asia Minor», in J. S. Langdon, S. W. Reinert, J. S. Allen, C. P. Ioannides, edd., *TO EAAHNIKON, Studies in Honor of Speros Vryonis Jr.* New Rochelle, N.Y. 1993, I, pp. 253-265; and his «Mount Athos: Levels of Literacy», *DOP* 42 (1988), pp. 167-178.

ques Lefort is doing important work on the landscape and population of rural Macedonia¹². But what are the dangers?

1. *The mindlessness of numbers*. This is an egregious danger. It consists of presenting numbers or proportions as if they had an absolute significance. To say that 50% of a universe shares certain traits is impressive; but if one then finds out that the universe consists of 5 units, the original statement is meaningless. Similarly, to say that there are 250 cases of something sounds considerable: but what if the universe consists of 250,000 cases? This is an embarrassingly simple problem which I would not have mentioned except that it crops up with some frequency. A much more frequent – but more sophisticated – error is not to provide tests of the statistical significance of estimates. These are objective problems, connected with the concept of sample.

2. *The invisible hand*. This is the hand of the scholar, who decides how to deconstruct the data; how to choose the groups; what analytic categories to use; what relationships and correlations to seek to establish. Thus, for example, if one decides to look at all the properties of a certain landlord together, instead of looking at single villages, one has already made a qualitative judgment, which is that the landlord intrudes significantly into the life and economy of rural life, and that his identity is an important factor, more important than village organisation. Whether this is factually true or not, is not at issue here. The point is that the choice already biases the inquiry, and that this categorisation may obscure certain other important characteristics. In more general terms, a great deal depends on how and where one establishes the

12. See, for example, J. LEFORT, «Radolibos: population et paysage», *TM* 9 (1985), pp. 195-234.

boundaries between groups, categories or subcategories, surely a subjective process.

3. *The visible hand*, if only we allow ourselves to recognise it. I am speaking here of the prior selection, made consciously or unconsciously by the sources themselves. A statistical analysis of the *Peira* is of limited usefulness, if this is a compendium compiled with a specific purpose in the mind of the compiler. Similarly, the statistical study of the literacy of women may be dealing with a pre-selected group, to the extent that much of the information comes from witnesses to acts, and women did not routinely act as witnesses.

4. *The invisible factor*. This works in a number of ways. In terms of statistics, one may speak of the outliers — exceptional cases whose eccentricity may be so pronounced as to bias the entire sample. But this is easily controlled. More important is the factor whose explanation lies outside the statistics: a natural catastrophe, or a predator whose passage may influence a whole population, and whose presence cannot be deduced or predicted from statistical analysis. Even in the natural sciences, where aggregate populations and predictable laws would seem to rule, eccentric individuals and events may be of seminal importance: a recent article points out that the major features of planets, for example, «are set by unique events — mostly catastrophic»¹³. This is even more observable in the study of animal populations and their evolution. It has been noted that a single predator can so disrupt the demography of a species as to make its recovery a very slow and problematic prospect¹⁴. A comparable example in our case might be the Catalan invasion, whose progress through Ma-

13. STEPHEN JAY GOULD, «The Horn of Triton», *Natural History* (December 1989), p. 26.

14. *Ibid.*, pp. 21-22.

cedonia left ravages that are visible in the disrupted populations of a few villages of the Chalkidike even ten years later¹⁵.

What we have here is a particular form of the difficult and perennial question of aggregate and individual, and whether we learn more from studying one or the other.

II. Individuation and the seduction of pathology. This brings me to a second method, which I will illustrate primarily through the history of women. It is a more vexed topic than that of the peasantry. After all, agrarian history and the history of the peasantry are topics of venerable ancestry and therefore respectability and, although they gave rise to passionate debate in the past, the debate is relatively muted today. The history of women is another matter, partly because the sources are more limited: partly because there are strong ideological statements from the Byzantines themselves; partly because there are current ideological and political positions, which occasionally cause voices to rise. With regard to this topic, the pre-selection of the sources (the visible hand) is rather evident — the example of literacy which I have already mentioned is a case in point. Besides, we have an evident disparity between the sources bearing on individuals and those few sources which allow us to look at aggregates. That is to say, the information we get from the two kinds of sources is of a powerfully different nature. We do know a fair amount about a few kinds of women: members of the high aristocracy and saints. Not only do we know a fair amount about them, but we also have poignant stories: the story of St. Mary the Young, just about beaten to death by her husband; Anna Comnena, who adopted every stereotype about women and

15. D. JACOBY, «Phenomenes de demographie rurale a Byzance aux XIII^e, XIV^e et XV^e siècles», *Études rurales* 5-6 (1962), pp. 163-186. P. KARLIN HAYTER, «Les Catalans et les villages de la Chalcidique», *Byzantion* 52 (1982), pp. 244-263.

cursed her fate for not having made her a man; the nebulous figure of the sebastokratorissa Irene, whose life and deeds have aroused much commentary and interest¹⁶. So we can study particular individuals fairly well. But what can we extrapolate from their lives? Whereas one scholar might derive from the story of St. Mary the Young the idea that Byzantine women were normally downtrodden, and suffered grievously at the hands of the master of the house, another might point out instead that it was Mary who had the management of the household, she who had full control of her property to the point of alienating it all, she who contracted loans and she who repaid them. In other words, here the maverick individual, whereas not altering the course of the history of women, may very well alter the course of our perception very significantly.

How, then, do we get from the individual to the aggregate?¹⁷ At one level, we can analyse peasant women in the same terms as the males — with the results, advantages and problems that have already been mentioned. We can also use a finite but not inconsiderable number of sources with a very different tenor. These are documentary sources: contracts and donations on the one hand, and on the other, cases which have come to us because they are contained in the various legal sources, whether from lay or ecclesiastical courts. In this latter category, we come up against a fallacy that was powerfully pointed out by Pierre Bourdieu¹⁸. The fallacy lies in the

16. On Irene, see S. D. PAPANIMITRIU, *Feodor Prodrom*, Odessa 1905, pp. 24-27; V. G. VASILJEVSKIJ, «O sebastokratorisse Irine», *Zurnal Ministerstva narodnago Prosvesčeniye* 28.5 (S. Petersburg 1893), pp. 179-185; S. PAPANIMITRIU, «O πρόδρομος τοῦ Μαρξικανοῦ κώδικος XI 22», *Viz. Vrem.* 40 (1964), pp. 102-163; E. M. and M. M. JEFFRIES, «The Comnenian Background to the *romans d'antiquité*», in their *Popular Literature in Late Byzantium*, London, Variorum Reprints, 1983, Study X.

17. For a recent attempt, see A. P. KAZHDAN and A.-M. TALBOT, «Women and Iconoclasm», *BZ* 84-85 (1991-1992), p. 391-408.

18. PIERRE BOURDIEU, «Les stratégies matrimoniales dans le système de reproduction», *Annales* 27 (1972), p. 1107.

following: where we have such sources, we might be led to consider that they are a mirror of reality. But normally, social relations, especially in the realm of private life, are resolved informally, and never reach the point where they become documented — except these days by anthropologists. Thus what the documentation provides is a mirror of *distorted* reality, in effect a mirror of the *failures* of the system, not of the system itself. Bourdieu has called notarial acts, the bread and wine of social historians, as well as of legal historians to whom he refers, «simples enregistrements des ratés du système (actuels ou potentiels)». Yet this problem can be mitigated if we are aware of it. In the end, aggregates do tell a story. When we find that the proportion of judicial cases involving women ranges from 35% to 71% of the total, and that of those the majority involve the disposition of property, certain important conclusions may be drawn regarding the relationship between women and property in Byzantium, or even women and the court system. Most important, in this as in so many other things, is indirect information, which the sources have no direct interest in telling us, but which they provide in the course of other information. Furthermore, what one can try to establish is the parameters and boundaries of the system: situations which do occur, and whose importance can only be gauged by a reading of other sources.

As for the individual approach to the history of women, a well-known case allows one to show both its usefulness and its limits. The story, in a nutshell, is as follows: Around 1028, in Constantinople, there erupted a scandal among the upper class. A young aristocrat, Imerios, seduced a young girl, of the same social class, slept with her in the house of her father, and made her pregnant. The couple wished to marry, and made their intention known to a priest, in church. But the man's father did not give his consent, and the matter was brought to court in a case that lasted for some time. In the

course of it, the emperor intervened three times, once ordering the young man to marry another woman. The court debated at self-satisfied length the various punishments to be inflicted on the young man, eventually taking the admirable decision to fine him: 210 gold coins would go to the girl, and 150 to her father, as compensation for *injuria*. The case is long, circumstantial, and lovingly described; it has given rise to much modern commentary¹⁹. The point for us here, is that one could treat it in unsubtle and erroneous ways: for example, try to count the times seduction appears in the *Peira*, and from that extrapolate the incidence of seduction in Constantinople. Or one could take the resolution of the case as the normal way such questions were handled. But that would be patently absurd. The case is clearly pathological, not only because imperial interference three times is unusual, but also because it took a great deal for a family to bring such a case in the open, as Eustathios Romaios himself comments in another instance²⁰. So we learn nothing about usual behavior from the overt facts of the case. What we do learn is more subtle. We learn that such encounters were possible; that the father's will could block any love match; but that this principle was contested; we learn that a woman could get compensation for deflowering — but that social class played a tremendous role in the way such cases were handled. And we are reminded, through Eustathios' rhetorical flourish at the end, regarding the destruction sown by those who deflower virgins, of the great value of virginity even in the somewhat loose eleventh-century Constantinopolitan society. We finally

19. See, in the last instance, A. LAIOU, «Sex, Consent and Coercion in Byzantium», in *eadem. Consent and Coercion to Sex and Marriage in Ancient and Medieval Societies*, Washington, D.C., 1993, pp. 149-154, with the earlier bibliography.

20. In a different area, the paucity of sources has elevated the musings of Ke-kaumenos to the level of ideological statements typical of Byzantines. But it is at least as likely that his are idiosyncratic, or pathological, views.

learn about legal and real condemnation: the man is punished, but the social condemnation of the woman seeps through the case. The overt aspects of this story have little value for the general history of women; it is the subtext, easily corroborated by other stories, that has some general application.

For the history of women, then, I am arguing for a weighted reading of the sources, and a concurrent study of aggregates and individuals. The aggregates can show us inheritance patterns, property patterns, dowry patterns, literacy patterns. The individual cases can show us the limits of the system, and what a particular situation could mean for particular people: the pathological can show us where the stress points are in a system, the response of a group when it comes under stress.

III. **The comparative mode.** This is one of the oldest methodologies that have been applied to Byzantine social history, and can lead to illuminating results. Byzantinists have used this approach, often explicitly, at times implicitly. They have used it more than their counterparts who study western medieval history, partly because Byzantinists usually know more about other societies than scholars of other disciplines know about Byzantium. This is an issue which stems from the history of our discipline. Secondly, it is a well known fact that historians of certain other societies have at their disposal much more concrete documentation at least of a certain type than we do. This allows them more detailed analysis. The combination of the two factors, and of others, has resulted in the fact that some ancient as well as western medieval societies are better known than Byzantium, and scholars have developed persuasive (at least for a generation) models of how such societies function (I use the word «model» in a loose sense). It is fairly common for Byzantine social historians either to test their own views of Byzantium against such mo-

dels, or to compare what they know of Byzantium with what they know of other societies. The issue of Byzantine feudalism, for example, was treated for a long time within the framework of categories and models developed by western European scholars for western European societies in the first instance, and then generalised.

The comparative approach is a very useful one. It permits the historian to do at least three important things: to fill in the holes of the documentation, even if in a tentative and hypothetical manner; to establish similarities and differences with a given model; and to look beyond the Empire, and see whether it can be inscribed, with all its particularities, into a more general system, whether that is medieval, or frozen late Antique, or geographic, for example, Mediterranean. Instead of dwelling on the benefits of such an approach, let me just say that without it we would not have had Ostrogorsky's *Histoire de la féodalité byzantine*²¹.

But herein lie some problems. Of course, it is true that the discussion of the role of comparison in the cognitive process is a topic far beyond my competence. It is also true that at some level one very often employs comparison. The only thing I should like to do here is to bring attention to some of the traps which can affect our own studies. There is, first of all, the danger of assuming that there is a model, with clear and solid traits, which serves as the basis for the comparison, when in fact this is not the case. The «model» of western European feudalism is an example. What we thought we knew about it, and even the appropriateness of the term «feudalism» for western medieval societies, has been exploded by scholars in the last twenty years. With the deconstruction

21. For a measured comparative approach, which clearly shows the choices inherent in comparison, see D. A. ZAKYTHINOS, «Τὸ Βυζάντιον μεταξύ Ἀνατολῆς καὶ Δύσεως», and «La synthèse byzantine dans l'antithèse Orient-Occident», *Actes du colloque international de civilisations balkaniques*, Sinaia 1962, pp. 107-115.

of that model, what are we comparing to what? (The accent being on the second «what»). Secondly, there is the problem of choosing from a variety of societies or processes in order to draw one's comparisons: does it make sense to compare Byzantium to the Ile de France, or should one be looking toward Italy? The decision itself already assumes a prior process of comparison, which may well prejudge the issue. Thirdly, it is possible that there are enough particularities in both sides of the comparison so that the comparison does not hold. Given the historiographical and documentary situation, as well as the fact that Byzantium did not exist in a vacuum, some kind of comparative approach is, in my view, both necessary and useful. The very seductiveness of existing models, however, poses methodological dangers. One might choose one's model, gear the investigation toward discovering similarities with it, and then reach the unsurprising conclusion that similarities exist. Or, conversely, one may choose one's model, see that its constituent elements do not exist in Byzantium, and negate the existence of a group, or of a process, which may in fact have existed but in a form different from that suggested by the model.

This last difficulty describes, in an exaggerated fashion, some approaches to the history of the third social group I want to mention, the Byzantine merchant. The problem of sources is evident, and I will not rehearse it. The spectacular success of the Italian mercantile cities in the eastern Mediterranean after 1204 is also evident. The theory then developed that the western merchant had destroyed the Byzantine merchant, and furthermore that there is no evidence of involvement of the Byzantines in long-distance trade, especially the spice trade where the Venetians and Genoese made their fortunes. Ergo, there was no Byzantine merchant; ergo, there is a structural problem to be described, defined, analysed or explained away. But was one comparing the right

categories? Is the western merchant the Italian merchant? Is trade only long-distance trade? Is there another type of trade and another type of merchant that did exist in Byzantium? Relatively recent studies by M. Hendy, K.- P. Matschke, N. Oikonomides, E. Patlagean, A. Laiou and others have shown that such was, in fact, the case²². Once the problem is posed in those terms, the sources begin to yield information, with the result that there is now considerable study and analysis on Byzantine merchants and their activities both in the later period, where the documentation is relatively more abundant, and in earlier periods. While this work is far from finished, I think that we will discover first that there are activities different from those of the big Italian merchants who themselves were atypical of the «Western» medieval merchant. And secondly, we may discover patterns which define a different model. Comparison will then take place at another level.

The different methodological approaches I have described here are not each limited to a particular social group; though the study of each social group may depend more heavily on one or the other approach. Certainly the study of merchants, although starting in a comparative mode, must also be undertaken as a study of both aggregates and individuals. This is, of course, hardly a startling statement, since it is true of all «mute» social groups, indeed of the science of history generally. What the investigation of such groups does is to point up the need to be aware of certain rules in the treatment of aggregates and individuals. For aggregates and individuals behave differently, and therefore their evidentiary trace must

22. I refer here only to the recent study of E. PATLAGEAN, «Byzance et les marchés du grand commerce, vers 830 - vers 1030. Entre Pirenne et Polanyi», in *Mercati e Mercanti nell'alto Medioevo: l'area Euroasiatica et l'area Mediteranea*, Spoleto 1993, pp. 596-629. because of its methodological interest and particular relevance to our topic.

be treated differently. Aggregates portray the «mean», the central tendency of a system. Individuals can illuminate the exception, or the limit, possibly also encapsulate the mean; a judgment is unavoidable as to their significance and representativeness. Trying to deal with the laws of aggregation, as one must to do social history, has difficulties and pitfalls; but also great rewards, for it is a process of illuminating the past, and, in the case of our own discipline, bringing to the fore important aspects of the rich, diverse but highly articulated society that Byzantium was.

