

S. 432 Anm. 9: Die *Sextii* (mit kurzem *e*) sind mit den *Sestii* (mit langem *e*) nicht in Zusammenhang zu bringen. – S. 462: In der Inschrift ist nicht *a]uguri*, sondern *c.] v., cur.* zu lesen (vgl. die Addenda in EE IX, s. 593); außerdem ist *Claud* ohne Zweifel die Tribus, nicht der Gentilname (vgl. die anderen Inschriften der *Valerii* dieser Familie AE 1996,171 und bes. D. 1190). Wichtiger ist jedoch die Frage, ob man *ob ... amorem in cives* so deuten soll, daß der Mann in Lavinium tatsächlich ansässig war. M. E. ist dies nicht so (*in cives suos* wäre eine andere Sache, obwohl man auch hier *suos* gelegentlich auf das Subjekt, nicht auf den Geehrten beziehen könnte). An sich ist ja *civis* ganz neutral, 'Bürger (einer Stadt)', und der Mann wird von den [*sa*]cerdotes und dem *populus* für seinen *amor* gegenüber den Bürgern – aber m. E. keineswegs notwendigerweise seinen Mitbürgern – gepriesen.

Olli Salomies

RICHARD DUNCAN-JONES: *Money and Government in the Roman Empire*. Cambridge UP, Cambridge 1994. First paperback edition 1998. ISBN 0-521-64829-7. 300 p. GBP 19.95.

A reader not previously familiar with numismatics or economics (like myself) would be probably somewhat reluctant to read a book based on specific knowledge of material usually not very tempting to a "traditional" classical scholar. Reluctance, however, turns into relief and interest thanks to the instructions given by the author himself in the book's preface. If his advice is followed, this difficult and complicated topic becomes challenging and accessible to anyone interested in the financial structure of the Roman empire.

The great advantage of this book is its clear division into rather short chapters which are informatively titled and summarised, so that the reader can choose between more general and a very specific approach to Roman economics and the monetary government of the empire.

The first part of the book gives an overview of the Roman economical system, or rather the non-systematic nature of the methods to raise funds and control expenditure in the period from Augustus to AD 235. Despite difficulties in interpreting ancient sources (the figures concerning money surviving in historiography are shown to be mainly stereotypical and proverbial) D. offers us estimates of e.g. the imperial budget, army costs and price development. He compares wine prices, rates of donkey-hiring and wages for harvesting and digging (the information coming exclusively from the East) and makes interesting conclusions, for example that there probably was low inflation in the whole empire, but since a most of payments were made in kind and only the wealthiest section of Roman society was monetised, exact calculations are impossible.

As usual, taxation is discussed in a chapter of its own. Duncan-Jones' aim is to calculate how much revenue was gained in form of taxes from Egypt. The source material consists naturally of papyri, mainly tax and land lists. Duncan-Jones' calculations are well supported by the facts, but considering the far-from-perfect understanding of taxation in Egypt in general (e.g. new forms of taxes are found constantly), his conclusions should be taken mainly as guidelines. This is especially the case for poll taxes and other "personal" taxes.

The latter half of the book is dedicated to more specific discussion of money as a physical object, of coin hoards, minting and the circulation of coins, as well as to the analysis of coinage in a wider historical context. This part demands a lot from the reader (many figures and tables being offered to make the rather technical discussion more clear, but unfortunately for a non-professional they are sometimes more confusing than clarifying), but on the other hand reading selectively, i.e. the conclusions of each chapter, one gets a very good picture of the problems concerning money and money supply.

What makes this monograph especially attractive, is the great number of answers to practical and down-to-earth questions, for instance of who had money, where it came from and how it was used. This book should, of course, be read by every scholar who wants to familiarise herself/himself with the economical questions in imperial Rome as illuminated by numismatic evidence. But it is also most useful and even enjoyable to anyone who has ever had to consider money in the ancient world.

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RAIMUND FRIEDL: *Der Konkubinat im kaiserzeitlichen Rom. Von Augustus bis Septimius Severus*. Historia Einzelschriften 98. Franz Steiner Verlag, Stuttgart 1996. ISBN 3-515-06871-6. 417 S. DEM 140.

A new monograph on Roman concubinage is, perhaps surprisingly, not superfluous. As Friedl rightly concludes in his critical overview of earlier scholarship (13–21), many of the existing works (including the classic treatments by Meyer and Plassard) are totally obsolete while others are restrictive either in their choice of source material or in their subject matter.

F. has undoubtedly managed to produce the most comprehensive study so far on Roman concubinage in the early imperial period. His thorough familiarity with secondary literature (up to 1993) on e.g. law, epigraphy, onomastics, and social history is amply attested in the notes. In fact, it might have done little harm to save space by omitting some of the less important references. F's analysis of the primary material is sound and useful. He includes inscriptions not only from Rome (earlier studies on quasi-marital unions have concentrated on CIL VI) but also from Italy and the western provinces. This is a clear advantage, as in the imperial period the inhabitants of Rome by no means form a representative sample of "Roman society". North Africa, the Balkans, and the whole of the Greek East are excluded, though. I can understand the reasons but it means that there is still need for further investigation.

I am somewhat less satisfied with two other limitations. Firstly, F. excludes (111–2) all unions where at least one member is a slave (in legal terms *contubernia*). As F. himself shows (94–101, 218–20), the word *concubina* was not clearly defined in Roman everyday language, and in addition, many unions which could later be defined as *concubinatus* may have begun as *contubernia*. Thus, the exclusion is based entirely on formal juridic criteria, corresponding neither to the conceptual (from the Roman lay point of view) nor to social reality. This is potentially dangerous in a study which attempts to explain why people chose to live in concubinage.

Secondly, F. pays little attention to evidence after the early third century. Although I can appreciate his fear that the scope of the book might have expanded beyond control,