Buchbesprechung: Money and Government in the Roman Empire [Richard Duncan-Jones]

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Money and Government in the Roman Empire

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This major study is divided into three unequal parts, with a series of appendixes. All of them are relevant to serious students of Roman government and economy, and the second and third parts in particular are relevant to numismatists. Portions of the whole have been anticipated in articles by the author, whose earlier books are fundamental to the study of the Roman economy. It is fair to say that this is the magnum opus we have been awaiting; and that it crystallizes the author's approach to numismatic evidence, for which we have had to be content with tantalizing hints in his prior work.

In spite of the length of this discussion, it has been impossible to consider all the implications of the work; given the venue I have attempted to keep focused on the strictly numismatic or methodological questions which underlie the larger themes.

Part I. The Economics of Empire

Duncan-Jones presents a succinct and wide-ranging summary of the evidence available on surplus and deficit; money, prices, and inflation; the imperial budget; and tax cycles. In his view the Roman empire was not fully monetized, but the Roman *government* could not do without cash, and even revenues collected in kind were converted to cash. There is no evidence that the government did, or could, borrow; and yet there is constant evidence of 'openhandedness', for withholding imperial generosity ran the risk of unpopularity. Apart from regular forms of taxation, the emperor could count on various rather unequal resources: seizures of property (which was then converted to cash); special levies, such as the aurum coronarium; precious metal from temples, statues, and the like; and sales of goods. Particularly with regard to the first and last of these, it is slightly troubling to see the evidence of the *Historia Augusta* cited uncritically; in any case sale of houses, furniture, carriages and (!) dwarves must have had psychological and rhetorical impact that far exceeded any benefit to the imperial treasury.

Inflation, the bugbear of modern times, does not seem to have been significant during the period surveyed. The use of ancient prices is notoriously difficult, since only exceptionally high or low prices tend to be quoted, and the subject was rhetorical in the extreme. A survey of prices drawn from papyrological sources – including wine, donkey hire, harvesting and digging – leads to the conclusion that the compound rate approximated 1% per annum (p. 29), and this conclusion is supported, over the long term, by adjustments in military pay.

More problematic, it seems, is the relationship between coin output and price movements (pp. 29–32). Tables 2.1 and 2.2 chart donkey prices against coin output,

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as measured on the basis of Milne's hoard 17 – one of those described by Milne himself as «purchased ... in the Cairo market ... and having less scientific value ...»[1]. The hoard is unique in its distribution of coins over so long a period, and the need to employ such evidence shows how far we are from being able to measure, with any precision at all, a rather sophisticated economic phenomenon. Yet this is a foretaste of things to come, with slender and unreliable evidence being preferred to silence.

Chapter 3 (pp. 33–46) addresses the imperial budget, of which the major components are identified as the army (including salaries and discharge costs); civilian employees (citizen procurators, senatorial employees, non-equestrian procurators); the emperor's household; distributions to soldiers and civilians; building; gifts; foreign subsidies and gifts. Of these, attempts at quantification are made for the army, civilian employees, handouts, and building; and for these categories comparisons are made between c. A.D. 150 and ca. A.D. 215. Arbitrary figures are built-in to Table 3.7 (p. 45) for the others, and the fact that these figures alone vary by a factor of 100% for the earlier date and 50% for the latter should give some indication of how approximate the results are. In both cases military spending is calculated at between 71% and 77% of the total.

It need hardly be emphasized that these figures *are* approximations based on slender evidence; and for them to have any meaning at all requires the assumption that imperial expenditure was entirely monetary. For this proof is wanting. In fact the area of financial obligation most likely to have been met non-monetarily is the military one, where supplies are known to have been requisitioned locally. The need for cash was substantially reduced by extensive deductions against the stipendium.

Chapter 4, on taxes and tax cycles, is a summary view of a system that incorporated a wide and ever-expanding variety of taxes but in which receipts often fell short of target and which was always open to abuses. It is a kind of coda leading up to section II, on the coin evidence.

Part II. The Coin Evidence

Chapters 5 and 6 form the core of the book. The author founds his analysis on a collection of hoards that is far greater than any heretofore examined for the imperial period (summarized in Appendix 10, pp. 261–268). Of the hoards 61 are mainly gold, another 169 mainly silver. A further 44 find mention in the text without being employed in the 'main analysis'. The threshold for inclusion is a value of 400 sestertii (though in practice AE hoards are not used); thus at minimum the hoards include 100 denarii, 4 aurei, or some combination of the two.

The author questions – rightly, in my view – the traditional categorization of Roman imperial hoards as 'circulation hoards' or 'savings hoards'. However useful

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[1] J. G. Milne, University of Oxford. Ashmolean Museum. Catalogue of Alexandrian Coins (Oxford 1933), Table of hoards (at end of volume).

these terms may be in describing the plausible behavior of ancient hoarders, they are not very easy to apply and are not useful for analysis. He goes on to make some generalizations:

1) there is some tendency toward clustering of hoard sizes in even multiples of sestertii.

2) 90% of imperial hoards contain coins of only one denomination. The author makes something of the fact that «this is in complete contrast to the 'money carried by inhabitants' of Pompeii», and from this he concludes that «patterns of private coin-ownership are different from the single-denomination hoards in the main sample». But one wonders whether the conclusion has any meaning: just as the legendary John D. Rockefeller carried around a supply of dimes for distribution, most individuals have historically separated their ready cash from their concentration of wealth. Hoards – particularly the larger ones – can hardly be compared usefully with the pocket change carried by inhabitants of Pompeii or anywhere else.

3) The total number of hoards of gold is only 27% of all hoards studied here, but they make up ¾ of the face value. Again, in stray finds gold constitutes about 70% of the face value in these same hoards. Now since the basic gold unit was 25 times the face value of the silver one, and 100 times the sestertius, this should hardly cause surprise; and the coincidence of percentages, if not wholly adventitious, is meaningless. As has been noted elsewhere, it is a mistake to treat modern recoveries as if they were ancient losses.[2]

Even this massive amount of evidence leads to some equivocal results and (see below) no clear conclusions. For example, at p. 77 the author states, «Interpreting hoards as a barometer of insecurity becomes even less convincing when its logic argues more insecurity under Claudius than under Nero or Vespasian, and more under Hadrian than under Antoninus Pius.» But in almost the same breath (pp. 77–78) comparison is made between the number of hoards in RRCH and the number known from Italy of the empire; here «the contrast is probably linked to differences in levels of security between the Empire and the turbulent years of the late Republic». In fact what is being compared is not security vs. insecurity, but conditions obtaining in late Republican Italy and in those provinces where modern recoveries tend to be concentrated. These are the northern frontiers, which, in the author's view, were characterized by «sudden redeployment of army units, high rates of crime and brigandage, and even sudden death of army personnel»; any or all of these will have added to the number of hoards lost in antiquity to be recovered in modern times.[3] When the subject is taken up again in another context (pp. 82–83) the author admits that «there was probably a tendency for continuous occupation of Roman urban sites to remove coin buried there before our hoard-

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[2] See usefully (on this and many other points) C. J. Howgego, The Supply and Use of Money in the Roman World, JRS 82, 1992, pp. 1–31, at p. 3.

[3] See Howgego (above, n. 2) p. 3, citing M. H. Crawford, Coin Hoards and the Pattern of Violence in the Late Republic, PBSR 37 (1969) pp. 76–81.

records begin»; if this is correct the effect will have been most profound in Italy. Moreover large categories of hoards probably have not survived to any significant extent: those buried in secure regions by civilian or soldier; those buried in insecure regions by people living in family groups; hoards accumulated by way of trade.

Though much of the evidence used by the author is actually presented only in the following chapter, the summary attached to chapter 5 (pp. 84–5) offers the major conclusions of this section. The most important of these is argued as follows:

1) for most of the period, finds correlate reign by reign with civilian handouts (congiaria);
2) congiaria were typically matched by donativa;
3) the civilian handout, confined to the urban plebs, would not show itself over the empire;
4) the find-pattern [i.e. the correlation in (1)] thus becomes intelligible only if most hoards originate from the second type of handout, that is army donatives.

If sustainable, this novel way of looking at hoarding would assuredly be preferable to the 'barometer of insecurity' approach. Points 1) and 2) deserve examination, because the interpretation derives from them.

1) To the naked eye the postulated correlation between hoards and congiaria is not so apparent, and both the evidence admitted and the method of analysis are defective:
   a) The author's association of silver and gold hoards with congiaria or donativa is foreordained by his selection of material. He has excluded a vast number of hoards by establishing a threshold of 400 HS (small savings); obviously it is not inviting to associate such small accumulations with sudden aggregations of cash. It is simply asserted that these smaller hoards would not affect the overall picture, but the effect would be to smooth whatever hoarding curve there is.
   b) He has accepted, as the byproduct of congiaria or donativa, large hoards that include mixed denominations or even admixtures of provincial silver. For example, it is hard not to believe that so diverse a lot as the Sakha hoard (his no. 107), from Egypt, resulted in part from the rediscovery of an earlier hoard in antiquity.[4]
   c) The association of individual hoards with particular events is sometimes very loose. The same Sakha hoard is linked to the accession of Hadrian (p. 86 n. 12), though it has no denarius as late as the issues of Trajan with PARTH and therefore closes with coins struck no later than 116. Five hoards are associated with the donative of 136 (p. 88 n. 13). But the Casta-

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[4] S. H. Weber, An Egyptian Hoard of the Second Century A.D. ANSNNM 54 (New York 1932). In addition to imperial silver from Arabia (2) and Cyrene (3), the hoard contained silver of Rhodes (12), Argos (2), and Sicyon (5) as well as the Achaean League (13).

gnaro hoard included none of Hadrian's late types (e.g. the 'Provinces', RESTITVTOR or ADVENTVS series), and indeed none with obv. legend HADRIANVS AVG COS III P P, not to mention no coins at all of Aelius; in short, it closes too early. At the other extreme is the Mont Moselle hoard, associated with the same donative: it includes no less than 10 coins of Antoninus Pius, at least one of which has M. Aurelius as Caesar.[5] It need hardly be added that the fluid chronology of the Hadrianic coinage itself precludes very precise dating either of hoards or of the congiaria they document.

d) The evidence itself is uneven. Many of the hoards cited are compromised by incompleteness, a problem familiar to students: the freshest (and therefore latest) coins may have been removed, as well as some of the rarities. In many cases Duncan-Jones's tables reflect only maximum sizes for the hoards, in others the minimum. At one end we have the Larnaka hoard (his no. 172), for which he gives 783 denarii, the number acquired by the ANS in 1910–1911; these were surely part of a larger lot, and only 451 can be identified today. The Prelasko hoard (his no. 173) is listed with 700 coins, but «Die Anzahl der Münzen wurde von 600 bis auf über 700 geschätzt», of which 578 are recorded.[6] For Briglands (no. 175) 179 are recorded here and listed in the original report,[7] but earlier scattered discoveries from the same find escaped record.

Moreover the hoards themselves have been very differently reported. It is one thing to be dealing with the Erla hoard, published in full, or even Reka Devnia, of which about five-sixths has been accurately recorded; it is quite another to be dependent on mere counts of coins (such as those provided by Gerov, Mirnik and his sources, or CTMAF, and especially, in the tabular listing, to suggest a precision that is not possible.[8] Worse still, from the user's

[5] S. Bolin, State and Currency in the Roman Empire to 300 A.D. (Stockholm 1958) p. 345 (Table 5, no. 19 with note).

[6] FMRS II no. 353, pp. 86–110.

[7] A. S. Robertson, A Hoard of Roman Silver Coins from Briglands, Rumbling Bridge, Kinross-shire, Proceedings of the Society of Antiquaries of Scotland 90, 1956–7, pp. 241–246, a citation not locatable through this volume.

[8] Duncan-Jones relies on B. Gerov, Die Einfälle der Nordvölker in den Ostbalkanraum im Lichte der Münzschatzfunde I. Das. II. and III. Jahrhundert (101–284), ANRW II.6 (1977) pp. 110–181; I. A. Mirnik, Coin Hoards in Yugoslavia, BAR International Series 95 (Oxford 1981); and the volumes of the Corpus des trésors monétaires antiques de la France. To take one example from each, Duncan-Jones's citation of the Berznovo Hoard (his no. 141) derives from Gerov p. 151 no. 35 and in turn from BIAB 26 (1963), p. 257. D-J. gives 500 denarii in his listing, but only ca. 250 are said by Gerov to be in Pleven; the BIAB report itemizes only 204. The Osiek hoard ('Osijekn', D.-J. 140) is given as 2500 denarii on the basis of Mirnik, p. 56, but Mirnik gives 2500+; in fact only 1286 coins can be identified on the basis of Wruck's report (Osiek: W. Wruck, Der Denarfund von Osiek (Mursa), Deutsche Münzblätter N.F. 12, 1937, pp. 289–293, 311–317). Virtually all citations of round numbers from CMTAF are approximations (for example D.-J. 10, St.-Georges, given with 70 aurei, is «au moins 70», 11, Lentilly, with 200, is «environ 200»; and so on.

point of view, is tantalizing reference, without further documentation, to the long-awaited bibliography of British coin hoards by Prof. Robertson.

2) It is particularly troubling for the argument that we know so little either of the dates of donativa or of the manner in which they were paid. A number of factors suggest that donatives at least were not paid in cash on the spot: for one thing the camp chest would hardly have been sufficient, under normal circumstances, to meet the burden.[9] And Millar has pointed out the difficulties in supposing that cash was transported to the armies on each occasion of a donativum.[10]

In any case the facile assertion that «congiaria were typically matched by donativa» will not bear examination. The coins themselves provide most of the evidence for congiaria, but they fail us almost entirely with respect to donatives, for which we are dependent on the spotty literary evidence. The following table, based on the summary of congiaria given by van Berchem[11] and on the evidence for known donatives, shows how misleading it is to infer a donative from a congiarium:

[9] One has the impression that the evidence has been stretched to or even pushed beyond its limits here. «The reign [sc. of M. Aurelius and L. Verus] began with an enormous and presumably extravagant donative, which gave the Praetorians HS 20,000 each, and proportional amounts to the other troops.» The footnote cites *HA Marcus* 7.9, 'corroborated by Dio 74.8.6.' Examination shows that what Marcus and Verus did in 161 was «vicena milia nummum singulis ob participatum imperium militibus *promiserunt* et ceteris pro rata» according to the *HA*; and the citation from Dio comes from his account of the accession of Pertinax, where Pertinax (who had given only HS 12,000) claims to have given as much as Marcus and Lucius.

This hardly argues for instant cash settlement any more than a «British gold hoard which ends with coin of 159/60» and a «Syrian hoard which is mainly gold» are likely to reflect the event. The British hoard is the well-known one from Corbridge; the Syrian one (from Acre), not published in detail but noted in CH 7, no. 243, consisted of 38–40 aurei (3 as old as Nero, 29 Antiochene tetradrachms and a denarius of Vespasian.

We leave aside here the matter of Vegetius 2.20, where it is implied that half the donative was regularly left on deposit *apud signa*.

[10] F. Millar, Les congiaires à Rome et la monnaie, in: Nourrir la plèbe. Actes du colloque tenu à Genève les 28 et 29.IX.1989 en hommage à Denis van Berchem, ed. A. Giovannini. Schweizerische Beiträge zur Altertumswissenschaft 22, 1991, pp. 143–157.

[11] D. van Berchem, Les distributions de blé et d'argent à la plèbe romaine sous l'empire (Geneva 1939) pp. 141–161. See also the article 'Liberalitas' by Barbieri in de Ruggiero, Dizionario epigrafico IV pp. 838–892.

	Cluster	Date	Congiarium?	Donative?
1.	–	139	LIB I	no
2.	–	140	LIB II	no
3.	–	144	LIB III	no
4.	145	145	LIB IIII	yes
5.	–	148	LIB V	no
6.	–	151	LIB VI	no
7.	–	152	LIB VII	no
8.	158	158	LIB VIII	no
9.	161	161	LIB VIIII	
10.			LIB I	yes
11.	165	165	LIB II	yes
12.	166	166	LIB III	yes
13.	–	167	LIB IIII	no
14.	169	169	LIB V	no
15.	175	175	LIB VI/Com. I	no
16.	–	177	LIB VII/Com. II	no
17.	180	180	LIB III	no
18.	182	181	LIB IIII	no
19.	–	183	LIB V	no
20.	186	186	LIB VI	no
21.	187/8	–	no	no
22.	–	190	LIB VII	no
23.	–	192	LIB VIII	no
24.	193	193	LIB I (twice)	yes

Twenty-three congiaria are documented, but only six donatives (nos. 4, 9–10, 11, 12, and 24); yet there are twelve 'clusters', one of which (no. 21) does not correspond even to a congiarium. Particular occasions – accessions, marriages, tirocinia – seem to have stimulated both events, but the congiaria were much more frequent (and, one supposes, much more of a financial drain).

It would be attractive to have some less nebulous concept than the 'barometer of insecurity' to explain hoarding patterns, but the case remains to be made. The case for impact of either congiaria or donativa on hoarding patterns is weak, and this must be dismissed as a component of any subsequent argument.

Briefer sections treat the Egyptian evidence and hoards beyond the frontiers. With respect to the former, the clustering of the hoards may be illusory since hardly any of the hoards is controlled. For the latter the only satisfactory interpretation seems to be payment to mercenaries or foreigners serving in the army: the accumulations in general have much the look of hoards from inside the empire, even if their chronological pattern diverges slightly; and we can document from other sources the increasing presence of foreigners in military service.

Part III is concerned with money and money supply, and is divided into 10 chapters, some of which have been published before in slightly different form; these occupy nearly two thirds of the book.

The introductory chapter is provocative in the negative sense of the term. The characterization of reverse types as 'conventional' is only true in a relative sense, and one wonders what exploiting the propaganda potential of coinage to the full might really mean. No one would be surprised if the emperors did not often take the initiative in deciding the details of coinage, or to learn that the author thinks the officials responsible for these decisions to have been conservative; but it is prima facie unlikely that an inefficient bureaucracy is responsible for the incompleteness of Hadrian's 'Province', 'Restitutor', and 'Travels' series (p. 98), or to suppose that the emperors were indifferent to the visual content of their coinage. Nor is it necessary to suppose that «the realities of monetary exchange could not penetrate the upperclass indifference shown by the historical sources» − a more economical view is that coinage was simply taken for granted.

The reduction of the Roman coinage to a mere economic device, governed by generalized rules of behavior, comes through most clearly in this first chapter, and the author's insensitivity to any of its other aspects is the key to many of the weakness of approach that emerge in subsequent chapters. Roman coinage is not viewed but measured; the intellectual, artistic, administrative and technical processes involved in its creation are entirely submerged by its behavior − or rather, its behavior as recoverable from the evidence at our disposal, and particularly its behavior in hoards.

The hoards are abundantly documented in the appendix. Chapter 8, on Chronology of mint output, comes to the reasonable conclusion that hoards provide a reflection of variations in mint output, and that they can provide an 'index' to relative output by reign, once wastage is estimated. The problem of wastage is taken up later (chap. 14, pp. 193–212); here it is concluded, not unreasonably, that gold and silver output parallel one another.[12]

Chapter 9 is an analysis, reign-by-reign, of the gold and silver coinage of the emperor from Vespasian and Hadrian through Antoninus Pius (Titus and Nerva being omitted because of poor evidence, Domitian as an exceptional case) followed by a look at internal fluctuations in the coinage of Septimius Severus. Up to this point the author has gathered the evidence of hoards and made some potentially useful observations. From this point on he crosses the line between evidence and extrapolation, and if the first few chapters have produced controversial results what follows is likely to shock.

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[12] This is not the place to question in detail the autor's precise figures for wastage; here he lays out the ground for his figures. But the evidence available is surely insufficient to support arguments requiring precision in this regard. For an example of a better-documented series, see A. Davesne and G. LeRider, Gulnar II. Le trésor de Meydancikkale (Paris 1989) pp. 263–266, on dated coins of Tyre.

Here we are introduced to the concepts of 'types per year' and 'coins per type', which are simple enough to calculate but not so easy to compare period by period. The application to the Severan period is difficult: the chronology for Geta, for example, is inconsistent with that of Septimius and Caracalla; and the eastern mints, which are not discussed in detail, upset any calculation.[13] Whether these concepts have any validity is itself a question, for the annuality of coin types is unfixed for any period in which the coins themselves are undated: obviously the longer a type was struck, the fewer the 'types per year', and the one is not necessarily calculable from the other. Numismatists tend to stretch out the striking of types to fill the time available for their production, but there is no certainty at all that (for example) the coins of imperial women were in fact struck continuously. The failure to confront the coins of the females is a great weakness of the whole work, not only for the Severans but for the second century, in which their coins constitute ca. 30% of the silver struck.

There are certain other new concepts:

The type/die equation. At p. 147, «coin types, whose frequencies are built up from die frequencies, are taken as a proxy for dies». This sneaks in the use of a constant relationship between dies and output, but more of this anon. Then the «original matrix of coin types, from existing catalogues, is used as a third coordinate, in conjunction with the number of types and coins of that reign in a given hoard-sample». The existing catalogues in question are Cohen (!) and RIC, in spite of the obvious inadequacies of both; the justification that «the Cohen material endorsed by RIC[1] must represent a basic matrix of coin-types» is simply dazzling. In fact the vast bulk of Cohen's material, however questionable, is 'endorsed' by RIC[1], which is to say it was taken over uncritically, only the most obvious anomalies being remarked or excluded.

Negative binomial k. This method is advocated, and used, in preference to more familiar procedures, not because it is easier — it is acknowledged to be otherwise — but because it seems to give results for Crepusius. The consequence for the reader is that none of Duncan-Jones's calculations are readily replicable by any method that depends on knowledge of the distribution of die duplicates, e.g. Esty or Good. Only Carter's method, which requires only raw counts of dies and specimens, can be used for comparison.[14]

[13] Despite the confident assertion, p. 135, that they constitute 5.7% of the Severan denarii at Réka Devnia, they cannot in fact be identified from that report with any confidence. The report singles out coins of 'style oriental' or 'style syrien' but there is of course no guarantee, in the absence of illustration, that all these identifications are correct. The possibility of confusion exists wherever a single Cohen number may include coins of Rome or the east — about 50 cases in all. N. A. Mouchmov, Le trésor numismatique de Réka Devnia (Marcianopolis) (Sofia 1934).

[14] What is called here 'Carter's method' is that laid out in G. F. Carter, A Simplified Method for Calculating the Original Number of Dies from Die-link Statistics, ANSMN 28, 1983, pp. 195–206. For a review of such methods — most of which produce results of the same order of magnitude, and whose theoretical soundness the reviewer would not presume to judge, see W. W. Esty, Estimation of the Size of a Coinage: a Survey and Comparison of Methods, NC 146, 1986, pp. 185–215.

Die counts. The author enumerates the relatively small number of die studies of imperial coins (p. 151),[15] then reports his own: of five attempts, only two were more or less successful (denarii of Crispina and Aelius Caesar); and one of these is discarded because the diffusion of the type in the hoards proved to be so uneven that its importance remains difficult to establish (p. 151 n. 29). This should have sounded a warning bell, but did not, and the author presses on with his analysis of Aelius.

Of all the Hadrianic issues that might have been chosen, Aelius' is in some ways the worst, for it is a coinage whose dies' lives may have been shortened artificially by the untimely death of the heir-apparent. The calculations assume what remains to be be shown except in the most general way: that the representation of an issue in hoards bears a direct relation to the number of dies used to produce it. (It would be quite another matter to show that that relation, if it existed, was a constant.) While most would agree that hoards provide a good guide to numbers of coins put into circulation, they may not correspond to the number actually struck; and certainly not to the number of dies used to strike them.

A final theoretical objection is the author's practice of counting (and deriving estimates of output from) reverses. For example, the denarii of Aelius are used as the nexus between die projections and hoard representation: a sample of 147 coins with rev. TR POT COS II CONCORD yields evidence for 78 reverse dies (p. 152, Table 10.5); a sample of 62 coins with rev. TR POT COS II PIETAS, 31 rev. dies (ibid. Table 10.6). These figures, using the 'negative binomial k' method, project to 379 reverse dies for the two types.

Then it is observed that the Concordia and Pietas types taken together were 48% of the Aelian output (the figures used are 72 of the two types of 149 Aelian denarii total in the Réka Devnia hoard). Finally, taking the denarii of Aelius (on the basis of 10 large hoards) as 1.99% of Hadrianic silver output, we reach a calculation of 41,149 total reverse dies for the silver coinage of Hadrian (379 dies/.48 of total coins in Réka Devnia/.0199 as a fraction of all Hadrianic coins).[16]

Confirmation of this is said to come from a count of the obverses. Table 10.9, based on a different sample, provides the basis for this calculation, and for the observation of something like parity between obverse and reverse dies consumed. «The big imbalance that could be expected if obverses with their longer potential lifetime were used to the breaking point is not seen here, and production-runs were

[15] In fact the number of die studies is not so small: for Claudius there is H.-M. von Kaenel, Münzprägung und Münzbildnis des Claudius. AMuGS 9 (Berlin 1986); for the Severan period, A. M. Woodward, The Coinage of Pertinax, 1957, 84–96; and The Coinage of Didius Julianus and his Family, NC 1961, 71–90. Only the first of these is mentioned in the bibliography.

[16] One can quibble with the mathematics: the correct figures from Réka Devnia are 70 and 156, so rather than .48 the second element should be .448; or one can enlarge the sample of hoards, which yields a figure of .0177 for Aelius' silver as a fraction of all Hadrianic issues; then the result becomes 47,795 reverse dies, a total larger by one-sixth. But this is all mathematics, not numismatics.

evidently kept within the capacity of the shorter-lived reverse dies» (p. 155). This is seductive but ultimately not persuasive, and certainly not demonstrated by the evidence offered here. For only half of Aelius' reverse types of this issue have even been examined; who is to say that the others do not share all their obverses with the coins reported here? Although the author has hesitated to burden his work with documentation (p. xv), here it is necessary for proper evaluation. His work cannot be replicated and it is reported in such a way that it cannot really be used.

The importance, for the author, of obverse/reverse die parity becomes evident in his approach to the gold, which follows. At pp. 155–160 there is an analysis of nine Hadrianic reverse types, whose die totals, both observed and projected, vary considerably. The author is led to distinguish 'low-output' and 'high-output' reverses, and to propose 'modules' of 25 and 50 reverse dies; then, based on representation in hoards, to project die totals for the whole reign of Hadrian. Here it is worth illustrating how the projected die totals derived by 'Negative binomial k' vary from those projected by Carter's method:

	Coins	Rev. dies observed	D-J	Carter
1. Concordia TRIB POT	108	9	13	9
2. Pietas TRIB POT	48	6	[9]	6
3. Hadrian riding	96	26	53	31
4. 'Hadrian riding, P P'	52	10	18	11
5. Hadrian galloping	71	14	25	16
6. Jupiter standing	74	23	50	27
7. Jupiter seated	47	19	50	27
8. Wolf left	102	16	26	17
9. Wolf right	40	11	23	13
	638	134	258 [+9=267]	157

Obviously acceptance of the scheme of 'modules' depends on acceptance of the 'Negative binomial k' method, the argument for which depends on its successful application to the Crepusius coinage.

So far the only trap the author has avoided is extrapolation to numbers of coins; that comes later. But even now the reader has to beware, even if he has forgotten that a large number of reverse dies might well be used in combination with a much smaller number of obverses. Counting reverses without counting obverses – and apparently obverses have not been counted – raises the danger of seriously overestimating output; and Duncan-Jones's projected die totals are in any case far higher than those yielded by Carter's method (as noted above, the only one which can be employed for comparison). Finally, anyone who would work with the evidence of gold hoards in its present state must expect to be proved wrong: since

the author wrote the Trier hoard has come to light, and its sheer size (it may be the Réka Devnia of gold hoards) is bound to upset all the projections here.[17]

Even those who agree that we can use statistics to extrapolate numbers of dies from a given sample do not agree on how many coins those dies could produce; for what it is worth Duncan-Jones's figures are 22,000 coins/die for gold, 7,982 coins/die for silver. To call these 'obviously elastic' is an understatement (p. 165); the reviewer's skepticism at their use to produce a 'rough estimate' of Hadrian's gold output at HS 2,270 million and make it yield, among other estimates, 294.8 million denarii for M. Aurelius, may well be imagined. And how it could possibly be relevant to compare this with estimates of 600,000–1.2 million ducats per year at Venice in the 14th and 15th centuries is anybody's guess.

This review will add hardly anything new to the recent controversy over whether such calculations are simply wrongheaded.[18] The caution has to be here, however, for Duncan-Jones has lent his considerable prestige to a form of inquiry that is premature to say the least, and his results are likely to be gobbled up, digested, and spewed forth by historians as part of the historical picture of the second century. This should not be so.

Chapter 13 treats weight loss and circulation speed, and the author rightly complains (p. 180 n. 6) that few published hoards include reports of both weights and die links; few enough, in fact, include weights. The big Belgian hoard, which he prefers (p. 183) as a basis of calculation for gold wear, remains unpublished; and even the evidence of the silver hoards cannot be exploited to the full, for only 'coin of the emperor' is usable here. Of the four hoards used, it is worth observing that only one corresponds to the end-date projected by linear regression. A number of tables deal with AE hoards, employing only the evidence of the large Garonne and Puy-Dieu hoards; the picture here is slightly more consistent, suggesting a slow falloff, accelerating under Antoninus Pius, in the target weight of the sestertius from a Flavian 12 to the pound to an Antonine 12 3/4. Still, more needs to be done before one will want to put confidence in an annual weight loss of .002025 gm./year for the denarius.

Chapter 14 represents an attempt to deal with wastage and reminting. This phenomenon would be important to trace in detail, but in spite of the author's

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[17] The hoard has been described by K.-J. Gilles, Der grosse römische Goldmünzenfund aus Trier, Funde und Ausgrabungen im Bezirk Trier aus der Arbeit des Rheinschen Landesmuseums Trier 26 (1994) [= Kurtrierisches Jahrbuch 34 (1994)] pp. 9–24. The find comprised some 2,517 aurei; apart from 2 of Didius Julianus, one of Didia Clara, one of Septimius Severus and two of Julia Domna, the terminus is A.D. 166/7. There is also a new hoard of some 130 aurei from South Oxfordshire, quite comparable in structure to the large Corbridge find, which is now being studied at the British Museum.

[18] See for example T. V. Buttrey, Calculating Ancient Coin Production – Facts and Fantasies, NC 153, 1993, pp. 335–351; and Calculating Ancient Coin Production II – Why it Cannot be Done, NC 154, 1994, pp. 342–352. A different view is taken by Richard Reece in his review article The regional study of coin site-finds, JRA 7, 1994, pp. 480–490, at p. 488; and I have had the benefit of reading F. de Callataÿ's Calculating Ancient Coin Production: Seeking a Balance, forthcoming in NC.

efforts the question must still be regarded as open. The choice of Britain as the sample from which to calculate the progress of wastage – made simply because the evidence is abundant (p. 201) is probably misleading. The remoteness of the province and the barrier of the channel insured its isolation from the mainstream. British hoards tend to mix coins struck before 193 with those struck after much more indiscriminately than do hoards from the rest of the empire, for example; in either case the lifetime of the Trajanic and Hadrianic coins charted in fig. 14.7 and 14.8 was artificially prolonged.[19] Clearly purity was an issue, and the apparent sudden decline in the fineness of the Severan denarius was a factor in hoarding; this is less visible in gold hoards, but the evidence is slim and hard to interpret. As the author notes (p. 206), an increase in the total volume of gold in the 160s would have forced down percentage representation of earlier emperors. Duncan-Jones sees the gold issues of this period as «sizeable, but probably no more than this» and believes reminting is at issue.

Chapter 15 treats Change and Deterioration. The timing of this book is perhaps unfortunate, for it was published just as the work of David Walker has been exposed to serious questions.[20] Walker's analyses of silver coins have provided the basis for virtually all metrological discussion for the last two decades; not only was the sample vastly larger than anything that had gone before, but it provided a basis for a large scheme that purported to explain the relationships among Roman and provincial currencies, and indeed the very existence of the latter. Now it seems clear that Walker's readings were more irregular than the coinage itself, and were seriously affected by surface enrichment.[21] If this is so the whole of chapter 15 suffers, as well as any calculation based on denarii per pound of silver.

The final chapter is an exercise in mathematics that leads to some rather implausible conclusions, partly because of reliance on Walker's numbers. It is observed, for example, that the coins of Titus and Domitian struck under Vespasian are slightly heavier than those of their father, and that coins of Commodus struck

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[19] In four hoards terminating with coins of the period 208–213, the percentage of post 193 denarii is already high, if somewhat irregular: Bristol (D.-J. 198), 32.9%; Muswell Hill, (D.-J. 199) 52.5%, Much Hadham 69.8%, Darfield 2 (D.-J.) 202) 19%. Other later hoards from outside Britain show much lower percentages: Kecel 3.04%, Obererbach 3.35%, Tell Kalak (D.-J. 213) .89%. I do not mean to suggest a consistent pattern, merely that British hoards behave differently from others. Citations for hoards not in Duncan-Jones: A. Burnett, Much Hadham, Hertfordshire: 129 denarii and 36 sestertii to AD 211, in R. Bland, ed., The Chalfont Hoard and Other Roman Coin Hoards. Coin Hoards from Roman Britain 9. (London 1992) pp. 73–79; K. Biro-Sey, A keceli éremlelet, Cumania 9, 1986, pp. 27–71.

[20] D. R. Walker, The Metrology of the Roman Silver Coinage Part I: from Augustus to Domitian, BAR Supplementary Series 5 (Oxford 1976); Part II: from Nerva to Commodus (ibid. 22, Oxford 1977); Part III: from Pertinax to Aemilian (ibid. 40, Oxford 1978).

[21] K. Butcher and M. Ponting, Rome and the East: Production of Roman Provincial Coinage for Caesarea in Cappadocia under Vespasian, A.D. 69–79, Oxford Journal of Archaeology 14.1 (March 1995), pp. 63–77.

in the 170s are lighter than those of M. Aurelius.[22] The conclusion that different standards were simultaneously in use is hardly supported by the text adduced (Suet. *Dom. 2.3*). More evidence is required before we should abide this assault on common sense.

As noted above, this book has no clear conclusion, and many of its major observations have been questioned here. One is reminded, unfortunately, of the work of Bolin, now nearly four decades old, which essayed a similar theme.[23] It is hardly read today. I would not predict the same future for Duncan-Jones, but the books share the flaw that they attempt to view coinage solely as an economic phenomenon, whose behavior may be reconstructed according to an imperfect model based on even less perfect evidence. It is as if all we need do is record, count, calculate, and all will fall into place. Essential to the model is the generation of facts – e.g. coins per die – where there are none – and the extrapolation from limited amounts of published evidence when more could have been available.

The book has the great merit of pointing up the gaps in our knowledge, and, implicitly, how to fill them. Numismatists will always have to deal with defective evidence – hoards discovered under uncontrolled circumstances, and therefore susceptible to corruption or the suspicion of it; hoards seen too briefly to permit accurate recording. And it will be a long time before all are agreed on a suitably accurate method of analysis of coins, longer still until sufficiently large samples are available to be integrated into our studies. Better for now to admit the flaws in our knowledge than to spend our energies developing sophisticated means of guess-work.

Some steps can be taken now. Upgrading the standard of hoard reporting is the simplest: weights should be part of every report, die axes where appropriate, die links as they occur.[24] The evidence of hoards is often transitory, but they were (quite properly) the basis of this study, and how much better it would have been if each of them had been recorded in full. More generally it is time to confront the evidence of dies. Die studies are not the whole answer, but they will go a long way toward providing facts where we now only have guesses. The study of Crepusius is cited so frequently not only because it is a model, but because it has so much to tell us. His coinage, with its uniquely marked dies, saves a lot of labor – and we should take every possible shortcut – but it is time to bite the bullet and undertake die studies of imperial issues of controllable size. Perhaps the whole coinage of Trajan is not feasible, but the Four Emperors and Nerva should be; and by now the

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[22] The data comes from the Magura, Puriceni, Beck Row, Londonthorpe, and Little Brickhill, and Bletchley hoards; larger samples more suitable to the purpose could appropriately have been drawn from published collections: my calculations from the British Museum catalogue of coins not described as worn or damaged come to a mean of 3.25 for Vespasian, 3.26 for Titus, and 3.25 for Domitian based on samples of 122, 28 and 21 coins respectively.

[23] Above, n. 5.

[24] In this respect *Trésors Monétaires* and recent volumes of *Coin Hoards from Roman Britain*, rather than the various *Fundmünzen* series, are models for emulation.

abundance of material should render most second-century gold issues susceptible to examination by die, holding out the possibility of refining chronologies for the women.

Coins are not the answer to everything, but in the end such answers as they can provide will emerge only when they have been fully exploited. No other class of artefacts offers such an untapped resource, and if nothing else this book suggests not only the directions in which research should move, but methods that will be applicable when sufficient samples are available.

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