INDIAN NUMISMATICS

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Introduction

The fascinatingly wide range which the historical studies and generalizations of D. D. Kosambi cover is known to all his readers and it is indeed a measure of his great versatility that in no other area was the relationship between his 'basic' discipline and history as direct as in the study of coins. A Professor of Mathematics all through his teaching career and an acknowledged original contributor to statistical and genetical studies," Kosambi did not, however, let statistics alone dominate his numismatic research; his papers on the subject show him to be equipped with not only the basic rigours of physically handling coins but also his capacity to use, in his attempts to buttress his statistical findings, an impressive mass of literary data, and his familiarity with the latest research on coins, Indian and non-Indian.

Despite the fact that Kosambi personally examined more than 12,000 coins of 'all periods', his focus, during the twentysix years that he spent in studying different hoards and also in elaborating the methods of his study, remained all through on 'punchmarked' coins. Reasons for it may be read through his repeated pleas for scientific numismatics, which require, as is clear from the following specifications which he laid down, a set of precise data: "The coins must have been cut with sufficient accuracy at the beginning so that their initial variation is not much greater than the changes caused by circulation. This excludes copper, pewter, and even billon coins of the ancient period.... Again, the circulation must be regular enough to have the proper ctlect, which excludes gold coins in general, almost always hoarded with the minimum handling, but liable also to be clipped or, in India, rubbed on the touchstone. Finally, the groups must have sufficiently large members with comparable history, i.e. should be members of the same hoard".

Hoards of punchmarked coins were available for study to many a scholar before Kosambi's time, and what primarily distinguishes him from his predecessors is not his use of a statistical method as such but a set of entirely different assumptions which led him to such a method. In dealing with the weights of coins, particularly of coin-groups, variations in which have important chronological implications, Kosambi did not proceed from a theoretical standard: "I submit the opinion that the *rati* was not used, even in ancient times, to weigh the coins, but rather the coins determined the choice of the seed, exactly as at present". When actual weights in a group are carefully analysed, variations in them cannot be easily explained away as aberrations from a theoretical standard, and Kosambi found in statistics—in the method suggested by the 'homogeneous random process'—a way of tackling the problem.[5] The statistical part of Kosambi's studies may be incomprehensible to many of us, but the assumptions underlying it will not. In considering the weight-standards represented by coin-groups Kosambi started by pointing out that, although the possibility that in antiquity the weight-standard of a group was more homogeneous than the percentage of alloy, there was an 'unavoidable variation' even in coins newly minted; that the rate of such variation, among individual coins would go up, because of the wear caused by handling, after they had been put into circulation; and that in the coin-group as a whole "the decrease in the average weight and the increase in the variation are each strictly proportional to the length of time the coinage has been in circulation". A hoard does not necessarily consist of a single group, but the above assumptions would apply as effectively to disparate groups represented in a hoard as to a single group for the purpose of determining the chronological history of each group. In fact the hoards studied by Kosambi were all of composite character, where demarcation between the groups led him at a subsequent stage to speculate on their absolute chronology.

Once it is possible to establish the relative positioning of the groups in a hoard, the natural concern of a numismatist would be to speculate on their circulation history as also the history of the making of a hoard, namely, whether a hoard is deposited at one time or in successive stages or whether or not the hoard indicates the volume and variety of coins in circulation at a given point of time. This concern is underlined in Kosambi's statement: "The main purpose of a coin is not to carry a legend, portrait or cult marks but to put into circulation a piece of metal cut to a standard weight".[6] Here too the rate of decrease and the range of variation in such decrease would be useful indicators, but Kosambi added two more dimensions to this approach. The first is based on an assumption—and the assumption has all his sound reasoning to back it—-that the reverse marks on punchmarked coins were put by traders or traders' guilds, and the fact, demonstrated by him, that the greater the number of reverse marks the less the weight, would be a calculable measure of the'length of the circulation period of individual coins in a group. Kosambi calculated the interval between two reverse marks to have been of twelve years' duration. The second dimension is the consideration of the absorption rate of coins. Coins tend gradually to disappear in the process of circulation. Broadly "Speaking, this rate of absorption is proportional to the number of coins in circulation. In considering the circulation history of coins represented in a hoard this assumption is important, because, as Kosambi could show, "The number of coins per reverse mark decreased in a very regular geometric progression".

But statistics, according to Kosambi's own admission, "by itself cannot group the coins; it is of use only in discrimination between the groups". So from this initial 'discrimination between the groups' he proceeded on to the minting history of each group. It was as such necessary to probe into the significance of the symbols on the coins, which, because of the regularity in the pattern of their occurrence, were considered within the range of a sensible explanation. With his characteristic rneticulousness Kosambi waded through a vast mass of literary texts ranging, in variety, from the

Buddhist *Aryamanjusriilakalpa* to a fifteenth-century *Phalajyotisa* text, and if the mystery of the symbols is considered to still remain unresolved, the attempt can be justified in Kosambi's own language: "All the foregoing has been written only to point out some neglected possibilities, and to show that as mere conjecture goes, a novice can compete with veterans". His attempts to assign different groups in the Taxila hoards and the Paila hoard to specific rulers and dynasties of Magadha and Kosala were largely based on his own reading of the meaning of the symbols, but it is needless to accept that it was all work of 'mere conjecture', as in all cases specific attributions came only after rigorous grouping of the coins in the hoards had been made. And secondly, in no such cases did Kosambi let guesses transcend the limits of his assumed chronological framework—a framework strengthened by parallels from outside India.

Kosambi did not make use of any data from archaeological stratification in his dating of punchmarked coins. No such data, apart from those revealed by easily dateable coins in some hoards, were available when he began his numismatic research, but even in his later articles there is no mention of dating suggested by stratigraphy. But it would be certainly wrong to accuse him of lack of awareness in this regard; what he suggested as far back as 1941-42 would show that he viewed archaeology as potentially of more comprehensive use than mere dating. Something could be done with a chart of findspots. but not in the accepted dilettantish manner. If the findspots are accurately marked with groups, and the *numbers* counted instead of just the occurrence of a single coin of the type, we would make better conjunctures. Age and distance might be shown by loss of *average* weight, and the numbers or at least proportion would increase as one approached the locality of issue. For this, however, will be needed not only better grouping of information but also far more information from new excavations and more thorough-going surface collections.... It would have been of value to know the stratification of the coins of the older Taxila hoard.

What is remarkable is that even without the aid of stratigraphy his method alone brought his dating close to the possible range within which punchmarked coins were minted and circulated. He may be said to have gone a bit off the mark when, he suggested that the oldest coins in. the Paila hoard "represent the last of the real ancient Iksvakus, to be distinguished from successors like Pasenadi" (the suggestion possibly deriving from his assumption that coinage in India could be as old as the eighth century B.C.),[7] or that the cast coins were chronologically-later than the punchmarked series. But nothing known from archaeology so far seems to contradict his findings that coinage appeared in. the south in the Mauryan period in the wake of early historical trade or that a hoard, such as the one at Bodcnayakanur, could contain coins minted much later than the Mauryan period and be deposited as late as the fourth century A.D.

In. trying to understand what Kosambi contributed to the study ot Indian numismatics, it should, however, be remembered that the chronology of the punchmarked coins was not his only concern. It. in his language, "every hoard of coins bears the signature of its society",[8] then what Kosambi was aiming at was to decipher this signature in. the hoards of coins as also elsewhere. His vast range of observations, even if we limit ourselves here to a few selected ones based on the study of coins, will reveal this nature of his concern.

(a) Coinage began, with the traders, a supposition deriving not only from the "philological relation of *pana — coin with pani, vanik*= trader"[9], but from the entire process of the evolution of coinage m India, as Kosambi saw it. The background was provided to him by several classes of silver pieces found in the DK area of Mohenjodaro. Although he was initially hesitant in considering them as precursors of later day regular coinage, the remarkable similarity between the class IV of the Mohen-

jodaro pieces and later-day coins, and also the identity between the Mohenjodaro D-class weight (approximately 54 grains) and the weight system of the punchmarked coins gradually convinced him of a connection between the two systems: "Even after the destruction of Mohenjodaro which is entirely a trade city as shown by its fine weights and poor weapons,. the traders persisted, and continued to use the very accurate weight of that period. The first marks were traders' marks, such as are seen on Persian sigloi, and the reverse of the punchmarked coins of the pre-Mauryan age. This is shown clearly by one coin. (which) is blank on one side like our Mohenjodaro pieces, but the other contains no less than thirteen small marks, similae. in type to those known as the later 'reverse' marks". "The king stepped in at a later stage as issuing authority whose marks were to guarantee fineness and weight."[10]

(b) Kosambi offered a startling theory about the economic history of Taxila on the basis of its two hoards. The preponderance, at Taxila, of coins assigned to Magadha—a phenomenon which contrasts sharply with the absence of Taxilan 'bent-bar' coins in Magadha or elsewhere— argues for a balance of trade in favour of Taxila. The stability of the Taxilan economy for more than two hundred years is further 'suggested by a regularity of circulation revealed through curves of weight-loss and absorption. It was this favourable trade balance which led to Magadhan conquest of Taxila, but a rigid bureaucratic control eventually 'strangled the long-established trade' and thus brought about its ruin.

(c) The way Kosambi characterized Mauryan currency, again on the basis of the composition of the two Taxila hoards, is no less startling; there was a 'far greater pressure upon the currency' than in the period of the Nandas. One positive symptom of it was heavy debasement ("Copper more than half the alloy!"); another was indifferent minting, expressed thro ugh greater initial variation.[11] Not satisfied with the phenomenon itself. Kosambi looked for its explanation in terms of greater bureaucratization, expansion in the army and proliferation of trading activities, which combined to produce an acute shortage of currency which had to be met by debasing it. Kosambi also cited modern parallels by demonstrating that during the Second World War a similar pressure on British Indian currency was met in an identical manner.

(d) Kosambi appears to have been the first writer to have commented upon the significance of the paucity of indigenous coins in the post-Gupta period. This, in his opinion—an opinion supported in subsequent writings on the period—is a pointer to a major change in the economy: "The self-contained village was hereafter the norm of production. Taxes had to be collected in kind, for there was not enough trade to allow their conversion into cash ... the Chinese pilgrim states that Indians rarely used coins for trade. This seems confirmed by the absence of coins struck by Harsha.[12] which contrasts with the tremendous hoards of punchmarked coins that had circulated under the Mauryas''.[13]

It may not be possible to subscribe to all of such formulations by Kosambi; but perhaps Kosambi himself did not believe that his formulations represent the final truth; through them he was aiming to focus on areas he would have liked a numismatist to venture into. So far there has been no follow up of Kosambi's approach in numismatic studies, and this appears to be due more to a general lack of awareness of the possibilities and problem-areas indicated by him'than to the fact that no statistician of his standing has evinced any interest in the study of coins.[14]

The essays in this collection have been arranged in the chronological order of their publication, the justification for which is the frequent reference which Kosambi used to make, for purposes of cross-checking, and also to avoid repetitions, to his earlier articles. The changes introduced to the original are mostly for the sake of typographical uniformity. Grateful acknowledgements are due to the following for the permission to reprint the articles: No*, i. *i.* 5, 6 *(Current Science],* Nos. 3-4 *(New Indian Antiquary),* Nos. 7, 11 (Numismatic Society of India), Nos. 8-10 (Asiatic Society, Bombay), No. 12 *(Scientific American).*

Acknowledgement is due to the Archaeological Survey of India, New Delhi, for the photograph reproduced on the jacket of the book.

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NOTES AND REFERENCES

1 For discussions on Kosambi as a historian and on his approach see D. N. Jha, 'D. D. Kosambi' in S. P. Sen (ed.). *Historians and Historiography in Modern India* (Calcutta, 1973), pp. 121-132; D. Riepe, D. D. Kosambi: Father of Scientific Indian History' in R. S. Sharma (ed.), *Indian Society: Historical Probings* (In Memory of D. D. Kosambi), (Delhi. 1974), pp. 34-44.
2 For a brief biographical sketch of Kosambi and a complete list ot his works see V.V. Gokhale, 'Damodar Dharmanand Kosambi'. in R. S. Sharma, op.cit. pp. 1-15.
3 This refers firstly to the fact that Kosambi was extremely meticulous about grouping coin symbols and weighing the coins correctly. This is behind his severe criticisms of the lapses of U'alsh and others. Secondly, his technical expertise, at least in two respects, ought to be highlighted: (/) his ability to clean coins, when necessary and (11) his ability to offer a satisfactory explanation for the presence of copper on tiie surface of newly unearthed silver coins.
4 D. D. Kosambi. *An Introduction to the Study of Indian History* (Bombay 1956) p. 164.
5 The basic observations with winch Kosambi prefaced his works on punchmarked coins are to be found repeated in his articles. It is curious to see it suggested in a recent work by R. Laing *(Coins and Archaeology.* London, 1969. pp. 100-102). that they were formulated as a part of the application of the 'homogeneous random process' as early as 1938 by A. A. Hemmy, and what Kosambi did was merely to
make Hemmy's method 'intelligible to the layman' (ibid. p. 106). Whosoever has read Kosambi's scathing criticism of Hemmy's method, will realize the absurdity of the claim made on Hemmy's behalf. In fact the exposition of the statistical method and the curve illustrating it—as they are available in Laing's book—800 arc all based on Kosambi, more specifically his paper 'Scientific Numismatics'.
6 Kosambi, *Indian History,* p. 163.
7 Although P. L. Gupta and others also suggest *c.* 800 B.C. as the-possible date of the origin of coins in India (A. K. Narain and Lallanji Gopal, cd., *The Chronology of Punch-Marked Coins,* Varanasi 1966, p. 5), archaeological evidence would place the punch-marked coins in the sixth century B.C. at the earliest (S.C. Ray, 'Archaeological Evidence on the date of Punch-Marked Coins', ibid. pp. 26-38; S. P. Gupta, 'C-14 Dates Determining the Chronology of NI3P Ware and Punch-Marked Coins', ibid. pp. 39-43. S.P. Gupta's suggested dates start from the middle of the fifth century B.C.).
8 Kosambi, *Indian History,* p. 174.
9 Ibid. p. 170.
10 Ibid. p. 170.
11 Ibid. p. 168.
12 This statement may not be entirely correct (see D. Devahuli. *Harslia: A Political Study,* Oxford University Press, 1970, pp. 238-243), but that would not substantially affect Kosambi's argument.
13 D. D. Kosambi, 'The Decline of Buddhism in India . *Exasperating Essays* (Poona 1957) P- 65
14 One may. however, cite such evidence as a paper by S. P: Hazra ('The Weight for Raktika for Punch-Marked Coins', *The Journal of the Numismatic Society of India,* XXXII, Part II, 1970, pp. 131-143) in which statistical calculations have been used. It will, however, be clear from Kosambi's papers that in his use of statistics lie had an altogether different purpose in view.

1
A Statistical Study of the Weights of Old Indian Punchmarked Coins

THE PUNCHMARKS on old silver coins found in India have presented an unsolved riddle which has been attacked by a classification of the *obverse* marks, The efforts of Messrs. Durga Prasad,[1] Walsh and Allan'[3] in this direction will be valuable to future scholars, but as yet lead to no conclusion. The first two have paid some attention to the reverse marks also, while the third sometimes ignores them; the reason for this partiality to the obverse is that a group of five marks occurs systematically there, while the reverse may be blank or contain from one to sixteen marks.

The most important qualities of the coins in the ancient days wore undoubtedly the weight and the composition. The latter has received very little attention, a coin or two being sampled from each new lot. The former is given as a rule, for every coin, bui the statistical study of a coin-group by weight does not seem to have been attempted.[4] The resulting confusion as to what standard of weight actually existed can be seen by consulting any of the above works; even Rapson[5] found" documentary evidence too self-contradictory for use.

For the basis of a preliminary study, I took Walsh's memoir[6] on two Taxila hoards as fundamental. The work is full of oversights and mistakes, as I have shown in a note to be published in the *New Indian Antiquary."* Nevertheless, it is the only sizeable mass of data available to me, and I take all figures from Appendix XI, with the hope that no error of any importance enters into the weighing. Excluding the 33 long-bar coins which approximate to Persian sigloi, and the 79 minute coins, all the rest, to a total of 1059 coins which seem meant to represent the same amount of metal, average 52.45 grains in weight. The 162 later coins (Appendix XII) of a single coinage average 52.72 grains. But the standardization of weights was not the same, as.is shown by applying the *z.* test to the variances of the two lots.

But even the main hoard of 1059 *karsapana* is not homogeneous. So, I classified them by the number of reverse marks and found the following data, in which the 64 double obverse coins have been omitted.

In Table 1.1, *n* is the number of coins with the number *x* of reverse marks given at the column head, and *m* the average weight in grains.

TABLE 1.1

		x—0	1	2	3	4	5
Square	*n*	224	128	132	85	64	46
	m	53.26	52.93	52.74	52.47	52.53	52.17
Round	*n*	58	34	29	28	25	10
	m	58.35	52.84	52.75	51.90	52.29	51.67

		6	7	8	9	10
Square	*n*	21	25	10	9	8
	m	52.03	51.67	51.40	51.47	51.01
Round	*n*	13	8	9	3	3
	m	51.82	52.23	51.23	50.10	51.20

One coin in the square 10-reverse-mark class has been omitted, because it has a decidedly different history from that of the rest.[8] There exist coins with as many as 16 reverse marks, but counting the number of marks becomes difficult, and the total not tabulated being 15 square coins and 7 round, the table given below will represent substantially the most reliable portion of the data available to us.

It is seen at once that there is a regular drop in average weight with increase in the number of reverse marks. In fact, for the square coins, the linear regression can be fitted accurately enough by eye and is found on calculation to give the formula $y = 53.22 - 0.212x$, where y is the average weight in grains and x the number of reverse marks. For round coins, the fit is not so good, though still satisfactory, the regression being given by $y = 53.1 - 0.214x$. That is, practically the same line serves for both (Fig. 1.1).

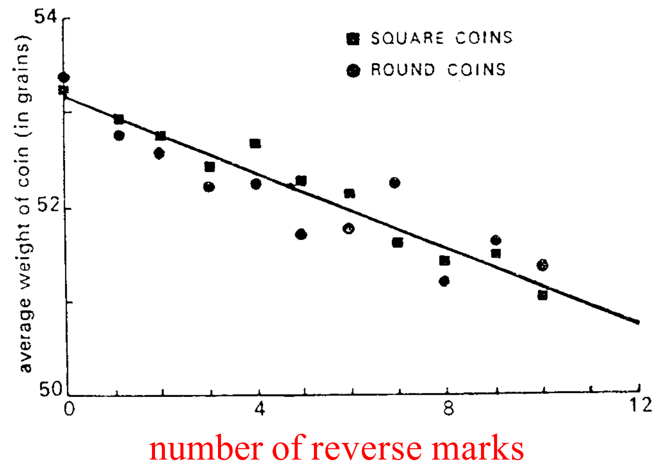
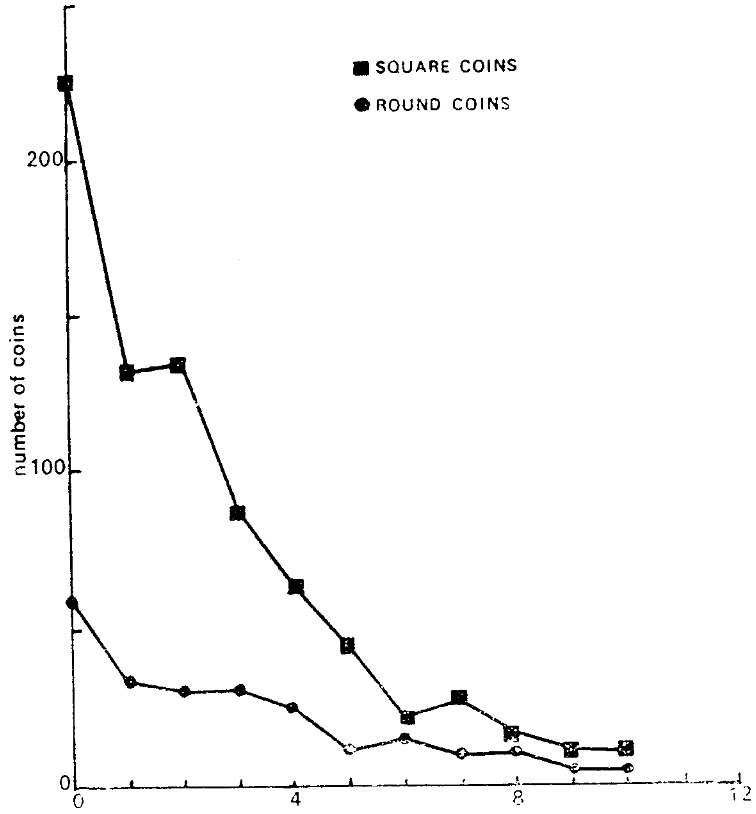


Fig. 1.1 Line of regression given by y= 53.1 - 0.214x.

The second result concerns the number of coins in each group. For simplicity, taking the sum y of both round and square coins with a given number x of reverse marks, the drop in number is exponential (Fig. 1.2). That is, the regression is given by $y = 283.86\, e^{-x/3}$. This was obtained by taking the logarithm of the number of coins with each .v, and fitting a linear regression. The divergence between the formula and the observed number is not significant by the x^2 test, and the calculation obtained from the above table serves also for the omitted coins, giving, for $x = 0$ to 16, a value of x^2 with p near 0.2; on the whole, a just tolerable fit.



number of reverse marks

Fig.1.2 Regression curve given by y=283.86 $e^{-x/3}$.

These two results are quite startling. They show that the reverse marks—irregular as they might appear—were not distributed at random, for had they been so distributed, we should have obtained a Poisson distribution or something of the sort for the number of coins as a function of x, and the linear regression for weight would not have fitted so well. The only hypothesis that can account for our results is that the *reverse marks are checking marks stamped on by contemporary regulations or controllers of currency, at regular intervals.*

If accepted, this means that among obverse marks there might exist some symbols that specify the date of issue of the coins. This would, possibly, account for the fifth variable symbol found on the obverse. Even now. we have a sixty-year cycle with a name for each year, and there certnmlv existed an older 12-year cycle, still extant in Chinese and Tibetan tradition, which was converted into a sixty year affair by associating twelve years with each of the five elements. This could account for one or two of the five obverse marks. One obverse mark is fixed: the sun symbol. If it is not votive, it might be a symbol of the metal itself. The next commonest mark is some form of the wheel, with (usually) six points of varying design. This *sadaracakra* is, in my opinion, not to be interpreted as a symbol of any deity, but as representative of the issuing authority, the *cakravartin* or king. The form of the points of the wheel, with perhaps one of the extra symbols, might be the ruler's personal monogram. This is borne out by the fact that in a few cases where the six-pointed wheel does not occur, we invariably get (with two exceptions) small homosigns in their place.[9] That is, when the issue was not authorized by a king, it was authorized by a council of some sort.

Leaving these doubtful conjectures, we can use groupings by obverse marks for the purpose of weight analysis and compatibility tests, in particular the *t* test and the *z* test.

Even in modern times, a certain, amount of currency will be lost each year due to damage, hoarding, melting down. etc. This should, in stable times, be proportional to the actual number of coins in circulation. But when the coin does not represent full value in metal content, being just a token coin, with a rigorous control of weight bv the examiners of currency, the formula for the number of coins surviving *t* years after issue would be given by

$$y=ae^{-bt}\left(\frac{1}{\sigma\sqrt{2\pi}}\int_{m_i-r}^{m_1+r}\exp\left\{-\frac{(x-m)^2}{2\sigma^2}\right\}dx\right),$$

and $$m=m_1-tm_2,$$

where $$\sigma^2=\sigma_1^2+t\sigma_2^2.$$

Here *a* is a constant of integration, essentially the number of coins minted. The legal weight, as also the average of freshly minted coins, is taken as m_1, the variance at the mint as *a1*^{2}. The average loss of weight per year is *m2* and the variance of this annual loss, *a2*^{2}. The legal remedy, i.e., the weight by which a coin may exceed or fall below the legal standard, is called *r* in the formula.

When the coin is a source of metal, the first factor would account for most of the currency in circulation, particularly as the variances with modern techniques of minting are very small. But with a token coin, and in any case after the passage of a greater number of years, the second factor would begin to dominate, and the coin withdrawn rapidly from circulation by those who check the currency. The phenomenon is similar to that often seen in biology, where a gene or a culture of bacteria shows exponential *growth* till a threshold value is reached, when the situation changes entirely, the growth makes its own surroundings lethal, and further growth is either inhibited, or the whole of the variate vanishes altogether.

NOTES AND REFERENCES

1 *Journal and Proceedings of the Asiatic Society of Bengal,* New Scries, 1934, 30, Numismatics Number.

2 *Memoirs of the Archaelogical Survey of India* No. 59, 1939.

3 *Catalogue of Indian Coins in the British Museum* 'Ancient India' 1930.

4 The work of A. S. Hemmy, *Journal of the Royal Asiatic Society,* 1937, pp. 1-26, must be dismissed as mere trifling with an important subject.

5 *Catalogue of Indian Coins in the British Museum,* Andhras, W. Ksatrapas, 1908, p. clxxvii et. *seq*.

6 *Mem. of the Arch. Sur. of bid.* No. 59, 1939.

7 See Article 3 *[Ed.]*.

8 One coin in the 3-mark round lot should also have been so omitted, bringing tin mean to 52.20, which would have fitted much better.

9 *Jour, and Proc. of the A. Soc, of Beng.,* 1934, Numismatics Number, p. 41.

2
On the Weights of Old Indian Punchmarked Coins

IN CONTINUATION of the work on punchinarked coins published in the July issue of *Current Science*[1], I have the following announcements to make:

(a) The weight variances of the Mauryan period are much greater than those of the earlier period, at least on the evidence of coins found at Taxila. For the later hoard, which is in almost mint condition, the variance is, in grain units, 5.65, whereas the variance for all the coins of the earlier hoard is 1.49, and for single groups of coins in the hoard, as low as 0.14, which compares favourably even with modern machine-struck coins.

(b) Proceeding on the assumptions that Walsh's descriptions[2] are substantially correct, and that my analysis (which makes the reverse marks periodic and regular checking marks) acceptable, it is found possible to arrange the main and most important groups of coins in the earlier hoard, in chronological order. These are: *B.b.1, A.1, C.1, D.2,* in Welsh's notation. The problem of assigning them to kings or dynasties is difficult on the basis of extraordinarily conflicting documentary evidence. But, as a tentative effort, I associate these coins in order wilh: Sisunaga II; the (later) sisunagas; the Nand or Nanda dynasty; and the Nava (= new, not nine) Nanda, Mahapadma, who is to be taken as the immediate predecessor of Candragupta Maurya. The documents .used are Pargiter's excellent collation of Puranic texts, the *Aryamanjusrimulakalpa,* the *Mahavamso.* the *Samanta-pasadika* and its Chinese translation, and some of the Jain tradition as reported in the encyclopaedia, *Abhidhanarajendra.* It is, of course, quite possible to give different interpretation and weightage to these texts, and to reconcile their great divergences in a different way.

(c) The coin samples are invariably skew-negative; and sometimes platykurtic because of a few badly underweight specimens which could be discarded by a certain criterion, based on the variance of the group itself, which I have had to use in the absence of any other evidence. But the skewness will always remain, and is in fact to be expected. The question now arises, does the z test apply to such distributions? If we assume that the frequency (probability) function ha; an expansion in weighted Herrnitian Polynomials about the mean value (surely not too restrictive an assumption), it is easily seen that *a sufficient condition for the distribution of the variance to remain the same as for a normal distribution is that all terms of even order, except of course the constant term, should be absent from the expansion.* This also ensures that all even-order moments are the same as for a normal distribution. So, it is clear that all tests based on variance alone— which excludes the t test, but allows the z test, Bchrens's test, and others of the sort— are valid for a skew distribution, provided there is no kurtosis. But it must be noted that these variances are to be taken about the usually known true or population mean; otherwise, the z test for skew populations is only a very good approximation for all but the smallest samples.

NOTES AND REFERENCES

1 *Current Science,* Vol. **IX,** 1940, **pp.** 312-314.

2 *Memoirs of ihe Archaeological Survey of India,* No. 59, 1939.

3
A Note on the Two Hoards of Punchmarked Coins Found at Taxila

MEMOIR NO. 59 of the *Memoirs of the Archaeological Survey of India,* by Mr. E.H.C. Walsh, C.S.I., M.A. (Retd. I.C.S.), (1939, PP- 10+164, with *xlviii* plates) deals with punchmarked coins found at Taxila. I approached this work with a view to seeing what systems of coinage-weights existed in ancient India, and of determining them by statistical analysis in case no such determination had been given in the memoir cited. In fact, I had made a certain amount of progress in the statistical work when it became evident that the report written by Mr. Walsh contained an astounding number of oversights and mis-statements which might completely invalidate the work I had undertaken. I publish this criticism (by no means comprehensive) of the Archaeological Survey Memoir No. 59 because others might take all its statements—backed as they are by official sanction and by Mr. Walsh's reputation as a numismatist—as consistent and authoritative.

The frontispiece reads: "An Examination of a **Hoard** of 1171 Silver Punchmarked Coins of the Older Class, Long-Bar Coins and Minute Coins Found in the Bhir Mound at Taxila in 1924 and a Hoard of 167 Debased Silver Punchmarked Coins of the Later Class Found in the Bhir Mound atTexila in 1912". The description is repeated on p.1 of the preface, but on p. *ii* the second and smaller hoard becomes 176. On the first page of the text, proper, the first hoard is reduced to 1167, and the second continues as 176. Turning to the tables themselves, we find Appendix XI headed on every page (pp. 100-153) as 'List of 1167 coins'. But on p. 153, the final number actually tabulated is after all, 1171. This is the same number that appears in the Plates. Appendix XII, the list of the later coins, has a subheading "The Hoard of 176 Silver Punch-Marked Coins..", but the actual number tabulated as well as represented in the Plates is 167.

This sovereign contempt for mere arithmetic characterizes the entire work. We read on p. 15, "Class C, Bull-Hill Area contains lot coins.. .", but the table on p. 46 gives a total of 102 in that class. On p. 32, we again find 167 coins in the later hoard, of which, omitting five of a separate coinage, weights of 162 are given for various ranges. But the coins so given add upto 163; and on my own tabulation from Appendix XII, the 52-53 grain range contains one coin less, 53-54 two coins less, 54-55 two coins more than given by the Memoir.

Either the proof-reading has been faulty, or there is an unexplained notation On p. 141. the weight of coin 935 is entered in Clarendon type; on p. 157, the weights of coins 936, 944-5, 948, 956 are entered in italics. Serial number 1098 (p. 146) is itself entered in italics; weights of coins 11 loand 1114 (p. 147) have just one of the three figures for the weights in Clarendon. Coin 1167, which seems a Double Obverse coin from the plates and is labelled as such in Table A (p. 39) is boldly labelled Double Reverse in Appendix XI (p. 153). Now for my purpose, the typography of the description of the coins is quite insignificant, if only the weights have been accurately found and entered. But I have grave doubts even about this. Coin 839 weighs 52.2 gr in Table A (p. 37). But it has lost a grain to become 51.2gr in Appendix XI (p. 127). The weights of most of the coins have been given to 1/100 gr only; which is not at all objectionable, were it not for the fact that the weights of some have been given to 1/100 gr. Of the 33 longer-bar coins (p. 100),just one, i.e., No. 12, has a weight in the hundredths; it would seem unlikely that all the remaining 32 came out exactly to 1/100 gr. Of the 1059 coins tabulated on pp. 102-153 only 268 have weights given to 1/100 gr. Of these again, as many as 229 have the last figure 6; 32 have weights that end in 3; three have weights ending in 9: one each has a weight ending in 1,5, 8, 2. No coin that has the hundredth grain given in the column of weights has the figure 4 or 7 there. I obtain these on a quick cgunt, and a coin or two might have been miscounted, but the classification is substantially correct, and the overwhelming preponderance of the figure 6 in the last place is inexplicable. One would like to know the system of weights used, the approximate errors of the experiment, and the methods used for checking. At least, this is the procedure demanded from the average science student in the laboratory, and there seems to be no reason why the Archaeological Survey should not adopt that standard.

There are some other discrepancies in the tables that add to the reasonable doubt that—I hope—has been cast upon the reliability of the Memoir. I fail to see that Appendix VII. Table G (p. 90) is a table at all in any sense of the word; perhaps, a similar remark might be made, with less force, of Appendix VIII, Table H (p. 91). But it is shown on Plates X and XI, and Table J is supposed to contain only references to Plates. In Table G (p. 47), we find two classes E and F, with the extraordinary statement, "There is no class E or F. The coins at first entered under those classes, were found to belong to other classes, under which they have been entered". To one who does not claim to be a numismatist, it would have seemed obvious that these classes, having no existence, should have been omitted altogether, and the later classes relettered accordingly. Coin 320 (p. 108) is described as having a blank reverse, with I ind'stinct mark, which seems a contradiction in terms. Coin 1149 (p. 150) shows an extra unmatched entry in the column headed 'Number of Marks'. The descriptions of reverse marks on coin 831 (p. 126) and 675 (p. 120) seem highly questionable to me. I should, however, again like to remind the reader that I am not a numismatist, and that this is just a cursory examination. Apart from the fact that one does not expect such discrepancies in a work so sumptuously printed, priced at Rj. 24-10 (or 38 s), it is curious that the amount of time spent on the work should not have sufficed for a thorough checking The preface, dated roth February, 1938, says (p. i) that the work was begun in 1928. I have a few remarks to offer about the theoretical conclusions of the paper. We find (p. 32) about the later coins:

The coins are an alloy of silver and copper. The metallic composition of two of the coins, taken as example have been determined by the Archaeological Chemist in India, who 'is of the opinion that they are composed of an alloy of silver ancl copper and contain 40.3 and 75.3 per cent of silver and copper respectively. Itisobvious thai their composition is very irregular'. It may be due to this fact that the weights of some of these coins vary so much from the usual weight of the Punch Marked Coins.

It is difficult to understand how the extreme variability of 162 coins was determined by assaying just two of them; in addition, the quotation about percentages of silver and copper is very difficult to interpret?" showing that whatever the composition of the coins, the English composition of the source of the quotation has been even more irregular.

On p.16, Mr. Walsh comes to the conclusion that inasmuch as the heaviest seeds of the *Abrus precatorius (rati* or *gunja,)* average 1.86 grains the *karsapana* (he calls it *karshapaya,* p. 15) of 32 *raktikas* would have been much heavier than the coins actually found. This statement has an air of verisimilitude, as the weights of coins 113-1171 average about 52.4 grains on *my* calculation. But the average of the *gunja* seeds of 1.86 grains comes from Cunningham's experimental determination, checked by the current Indian goldsmith's *gunjas.* as well as by picking out the largest seeds of the sample obtained by our author (p. 16). But we find on the same page that the author obtained an average weight of 1.68 gr for the *rati* 'after excluding all small seeds'. On the basis of his own experiments, he would have obtained the weight of the coin of 32 *ratis* as 53.76 grains, and had the small seeds not been excluded, it is a safe guess that 32 times that average *rati* would have tallied very closely with the average weight of the Taxila coins, allowing for loss of weight by circulation; in fact, even now, the two are quite close. Nevertheless, we find at the end of the third paragraph on p. 16: "It is, therefore, clear, that at the present time only the largest seeds are used as weights, and Cunaingham's 'full weight' is correct, and, on present practice the theoretical and actual weights of these coins cannot be reconciled". The statement is quite true but hardly to the point. It is well known (cf. *Report of the Weights and Measures Committee* 1913-14, Simla, 1914) that at present, the weight most commonly used for the *tola* is the current British-Indian rupee of 180 grains. But inasmuch as the *tola* is to be 96 *gunja* in weight, the largest seeds would have to be used by a goldsmith or jeweller to give anything like a 180 (or 183.75) grain *tola.*

I take it that the weight of the *Abrus precatorius* seeds, as well as the weight of any group of coins struck at any one place under the same system, would tend to vary according to the normal (Gaussian) law, about a given mean value. The average is the only quantity we ever find in general reports, but what is of the utmost importance is the variance, though no numismatist seems to have heard of the term. It was my intention to test the variance of the *Abrus precatorius* seeds by experiment, calculate the variances for the groups of coins given in this and other memoirs, and to see whether any evidence exists for nonhomogeneity. This can be done by modern methods developed by statisticians, particularly R.A. Fisher, in connection with the theory of small sample. The *t* test and the *z* test would be immediately applicable; and I hope to publish, in another paper, my results on the system of weights used in our ancient coinage. Inasmuch as the work be highly technical, I publish as a separate note this criticism of one of my main sources. The errors pointed out here need not affect the statistical work provided the weights as entered are substantially correct. But the classification is sure to cause difficulties, if I have to ely on the authority of such numismatists for the actual classes, without rany confidence in their data.

4
On the Study and Metrology of Silver Punchmarked Coins

THE PURPOSE of this essay is to attempt a statistical analysis of the silver punchmarked coins, mainly those found in two hoards at Taxila, and described in the *Archaeological Survey of India Memoir* No. 59 by E.H.C. Walsh [3]. It is unfortunate that this *Memoir* should be the' foundation of the present study, because it is full of errors and oversights; in any case, it is the only description of large, approximately dated, hoards available to me, and I advise prospective readers to use it with caution and with my commentary on it [4]. Weights as well as classes are taken from Appendices XI and XII of the work; where these contradict statements made elsewhere in the work, or contradict themselves, the evidence of the plates in the volume was used. So far as I know, this statistical method [5], though quite, well known to professional statisticians, has not been employed for the study of punchmarked coins. Probably, it has not been used in numismatics at all, because the peculiar and so far insoluble problems raised by the punchmarked coins do not present themselves in connection with coinage systems in general.

For the coins here investigated no method except the statistical one will give anything like a definite result. The reader should not be misled by the superficial resemblance of statistical terminology to the language of the race-course. Even for the most accurate scientific measurements, say atomic weights, a probable error has to be given; the *t* and the *z* tests would have to be used in much the same way as in this work to determine whether two distinct sets of such measurements were compatible.

In the first section, I review the usual discussion of the symbols on the coins, and add my own pennyworth to the existing welter of conjecture. The second deals with the present knowledge of their weight-system. Then follow other sections of primarily statistical content, well diluted with guesswork, and a final one giving a very brief note on the mathematical theory and methods underlying the work.

I POSSIBLE INTERPRETATIONS OF THE MARKS

Non ragionaiam di lor ma guarda e passa - DANTE Inferno III, 51

The wide distribution and the great antiquity of punchmarked coins was known to the very first scholars who turned their attention to Indian numismatics. For the rest, there is hardly a detailed statement about their nature and the interpretation of the marks stamped on them that does not [2] contradict itself or is not contradicted by the statement of someone else. In all these utterances, one can, or is at least tempted to, read more about the writer's psychology than about numismatics; the motives seem to range all the way from an ingrained contempt for native craftsmanship to an intense Indian patriotism. Cunningham saw "no difficulty in thinking that they might amount as high as 1000 B.C." [i. p. 6].

D. R. Bhandarkar wants to push it further back: "Coined money must be considered to be existing in India as early as the middle of the third millennium before Christ" ffi, p. 71]. Allan finds no evidence that coinage in India is o!dT than the Nanda period, and states. "The period of circulation of punchmarked coins may therefore be put at the third and second centuries B.C.; that they continued in circulation later is most probable, and that they go back to the fourth century B.C. is possible" [2, p. *Iviii].* These statements span the limits of human credulity, in view of the fact that no coins have been found at Mohen-jodaro; and that the earlier of the hoards I mean to analyse was closed before 300 B.C. and contains many very badly worn coins.

The main difficulty in dealing with these coins arises from the fact that if their symbols represent a legend, no one has succeeded in reading a single one. except perhaps the Taurine as the Brahmi ma; at the root of this is the absolute lack of relevant documentation. In the three authorities I take as the best (Allan, Durga Prasad, Walsh) one can find evidence adduced from finds at Mohenjodaro, the *Jataka* stories, the *Arthasastra,* and the *Visuddhimagga* of Buddhaghosa. But the *Jatakas* are written a few centuries after the period they are supposed to describe, a period not less than two thousand years after the rise of the Indus Valley civilization; the *Arthasastra,* taken as a document relating to the Maurya empire, is not less than seven hundred years older than the *Visuddhimagga.* In all these cases, a single word or phrase of doubtful import is the sole evidence, if any, for supposing that we have anything to do with punchmarked coins.

Any person who attempted to follow the varying fortunes of the Roman *solidus,* through the Italian *soldo,* down to the French *sou,* without the use of a single contemporary historical document, and indeed without any knowledge of European history, would have a task similar to that which confronts the Indian numismatist and archaeologist in general. Prinsep's assignment of phonetic values to the *Brahmi* script, surely the brightest spot in ancient Indian numismatics, was possible only because the equivalent of such evidence existed in ilie way of Greek legends on the same coins describing known rulers. Medieval European coinage as compared to that of classical antiquity will show that a cruder technique does not always indicate priority in time. The use of the Maria Theresa silver coins in Abyssinia (at least down to 1936) will illustrate the danger of connecting the history and the currency of the primitive localities without supplementary data. IATH now, scholars puzzle over the fact that the Gothic monarchy in Italy ends with asking whom documents call Totila. and who issued coins inscribed Badila; I have seen no satisfactory explanation of this, in a large mass of speculation; and the [3] unfortunate ruler's real name is still a matter of choice for the individual scholar.

Beginning with the unhappy conjectures of Cunningham. Theobald, and Spooner the systematic classification and study of the coins has proceeded, quite naturally, according to the punchmarks themselves. In this Durga Prasad, Allan, Walsh, may be said to have succeeded in pulling the subject upon a sound footing. The analysis of these marks requires long application, years of patient study, superior eyesight, and a powerful imagination.

But when it comes to the meaning of the marks, the state of affairs is far from satisfactory. The coins were generally issued with blank reverse, and such reverse marks as occur in the earlier coins are ignored altogether by Allan, and treated with scant respect by Durga Prasad. Moving a little ahead of the older opinion that even the obverse marks were 'shoff marks', i.e., testing marks put on by silver- and goldsmiths on more pieces of metal, we have dogmatic statements of opinion as to locality-marks [Walsh, 3, pp. 18-25], ancient Hindu religious symbols [Durga Prasad], designation of officials [2, p. *Ixxii]* and in general, propitiatory, votive, dedicatory, tribal, and totem marks. Now, these various interpretations need not contradict each other, but by themselves, they and the punchmarks are of as little use as the mere names of Catholic saints would be in determining a calendar and a system of dates, if nothing were known about the Christian religion or its measures of time. Durga Prasad does cite the description of the marks in certain Tantra's (i, p. 17 *et. seq.)* following Pran Nath, who first saw that tantric descriptions fitted Mohenjo-daro signs. Unfortunately the symbol can remain unchanged over three thousand years, without retaining anything like the original significance.

The persistence of symbols from Mohenjodaro on our punchmarked coins signifies less in this country than a similar survival would in any other land. We get the trisul symbol of the Indus Valley seals on wayside temples to-day, interpreted as the *Trisula* of Saiva practice; but the crescent on the reverse of punchmarked coins has now a definitely Islamic connotation which it could not possibly have possessed in those days. We see, for example, traits of Etruscan origin in the paintings of some Italian Renaissance artists, say Luca Signorelli; and some Renaissance sculptures could easily fit into Graeco-Roman classical antiquity. Only an accurate knowledge of the provenance of most pieces, without parallel in India, keeps us from confusing European work over a thousand years apart. Yet. Indologists seem to ignore the evidence of changes in other countries, and take a leap of centuries without the least hesitation. A modern Hindu like Durga Prasad should think twice before ascribing Hinduism, of any variety recognizable today, to antiquity. Between Mohenjodaro and the tantric works which he cites lie at least two great epochs of entirely different type. One of a less cultured population, probably alter the Aryan conquest, in which the deities [4] were Indra and the *vedic* gods and wealth measured in cattle. The second, of Buddhist (and Jain! influence, which wiped out the uorslup of the vrdic deities, and was superseded Hinduism that Durga Prasad would acknowledge without hesitation as the genuine article. The caste system and the development of a fundamental unity in the country belong to the first of these periods; *ahimsa* comes into Hinduism during the second which also sees the development of the ideal, 'universal monarchy'. The beginnings of tantric literature, as far as we can trace them today, are an integral part of *mahayana* Buddhism, whatever their real origin or significance. While the Jain literature has a continuous tradition antedating the Buddhist (whatever its reliability), our numismatists and students of antiquity usually ignore it. Add to this the fact that here is hardly a classical document available with a critically edited text, and certified chronology, and it will be seen that we have very little choice except pure conjecture; an admissible procedure if it is advanced with caution, and in a tentative manner The only inscription that I know of which is supposed to belong to a period between Mohenjodaro and the Mauryan age is that at Vikramkhol; and I have seen only one (unpublished) reading of it, a dubious one by Pran Nath, who ascribed it to a warrior king Kamsa or *Sasanka.*

Let us revert, then, to the punchmarked coins themselves, and regard the symbols a little more closely. The sun-symboL [interpretation disputed by Durga Prasad, I, p. 21] and some variety of the wheel, usually with six spokes, come on all the coins. There are three other marks on the obverse of varying nature, to make up a constant total of five. The interpretation, as usual, is a-matter for conjecture, the least probable being Bhandarkar's [6, p. 102], to the effect that "one set of symbols is certainly the seven *ratnas* or treasures"; these certainly did not include the sun, and in any case seven could not be expressed in five symbols. That the wheel is also found at Troy signifies little to some people, among whom I enroll myself. The 'sun' might be auspicious, a symbol of the *angiras* clan, or signify descent from the sun *suryavamsa* (as before Egyptian royal cartouches). The *sadaracakra* is, with much greater likelihood, a symbol of royalty; its various forms might denote separate rulers or dynasties. The larger Taxila hoard has 25 forms, [3, plate I; also p. 8] and taking later coins as well Durga Prasad identifies [i, p. 40] 32 different types. Even if each were part of the seal or monogram of a king, and they had ruled in succession, the older Taxila hoard would not have gone back more than three centuries, unless several kings had used the same monogram. This conjecture of mine need not be taken as disputing the putative 'great antiquity' of the coins, because, as I shall show later, the Taxila hoard implies a relatively stable type of society over a reasonably wide and prosperous area. For the rest, the conjecture that *sadaracakra* represents a king is supported by the fact that in the very few cases where it does not appear, it is with few exceptions replaced by homo-sign [[, p. 41; -2. pp. 21-24], which might represent the issuing authority as an oligarchy, or a council of some sort, perhaps for an interregnum, or regency. The mystical [5] significance of the *cakra* given by Durga Prasad in his otherwise excellent work need not be taken very seriously, as the wheel can also represent the Buddhist *dhammacakka.* I can hardly imagine it to have portrayed Buddha in a period when—as for the older Taxila hoard—Buddhism was not a universal religion, and had not the sanction of state authority. I only give the illustration to show that the wheel was capable of many and varied functions.

What the three remaining obverse marks represent is open to still more speculation, not to speak of the far more numerous varieties of reverse marks. I claim to have shown that, for the period of the earlier Taxila hoard, the reverse marks represent some sort of periodic checking [5, and here in section 4]. Both the obverse and the reverse types persist in later coins and inscriptions [8, pp. *clxxiv-clxxvii* and almost any of the plates], as is well known, but this furnishes no hint as to their meaning at any date or period. The so-called *caitya* symbol appears before the caitya could have become common or revered, and persists after the *caitya* went out of fashion.

I have nothing to say about these details, but there still remain possibilities to be explored. The suggestion has already been made that some of the symbols on the obverse could represent time marks [5], though what the actual time might be: date of issue of the coin, or the ruler's birth, or accession, would again have to be worked out. This conjecture was founded on the fact that some of the zodiacal *(rasi)* symbols are to be found among the signs, taking of course the name and not the abbreviated sign of each *rasi.* It is considered, however, that the present Indian *rasi* scheme was borrowed from external sources,, perhaps Greek; this is borne out by the fact that the names of the *rasi* list exactly correspond to the European zodiacal names, except that *makara=capricornus;* moreover, the Indian astronomers do mention their debt to *yavana* scholars, and other Greek names can be traced in our astronomical works. On the basis of these considerations it is generally believed that the ancient Indian astronomical tradition is entirely based on the *naksatra* (= asterism) system dividing the zodiac into twenty-seven instead of twelve parts. Recent discoveries, however, show that the twelvepart scheme is older than supposed.

There is still extant the Sino-Tibetan cycle of twelve years, each represented by an animal; in order, mouse, ox, tiger, hare, dragon, serpent, horse, sheep, ape, bird, dog, hog. This was known to have been borrowed from India, and the matter finally settled by Luders [10] in his characteristic masterly fashion. A Central Asiatic document discovered by Stein on the site of the ancient city of Gadoda and worded in a *samskrta* dialect which was the local medium of intercourse in the opening centuries of the Christian era, gives the animal list: rat or mouse, cow or ox, tiger, hare, serpent, reptiles (or worms = *jandunam,* apparently a nominative plural of the equivalent of *jantu),* horse, sheep or goat, cock, ape, dog, hog. The most interesting quality of these beasts is not their persistence over a large area—they can be traced with minor variations throughout eastern Asia—but that in the document under consideration, they are labelled *naksatras* and not associated [6] directly with any period of time. This makes it doubtful that the oldest *naksatra* scheme comprised twenty-seven, and a case could easily be made out, even on internal evidence deduced from their present nomenclature, that their number has been expanded at a later date.

Almost every Indian almanac *(pancanga)* contains a familiar table, the *avakahadcakra,* which gives correspondences between the *rasi* arid *naksatra* scheme. Each of the 27 *naksatras* is divided into four sections *(carana)* and each *rasi* covers nine of these, beginning with mesa= *asvini.* Far more important is the fact that for every *carana* there is a letter of the alphabet; all consonants except *sa, ba,* are represented (these can be replaced by *sa, va),* and for most of these, the five major vowels, *a, e, i,* o,B are given. There are quite regular gaps *(stambha)* where the consonants are given without the full complement of vowels; otherwise, the number 108 letters cannot be completed. The name *avakahada* is palpably the alphabetical order, beginning with *krttika,* the Vedic initial asterism; this has a foreign or antique flavour, and reminds one of Greek or KharosthI; but I have been unable to trace the scheme beyond the *Samarasara* of Ramacandra Somayaji (or Vajapeyi), an author of the I5th century as far as our tradition goes. The whole subject belongs to the 'science' of astrology *(phalajyotisa)* as distinct from the more rational astronomy (*jyotisa),* and is neglected even in this country except by quacks; hence, tracing anything becomes impossible. But it has an important aspect for our coins because in the same table as published today (though not in the manuscript of the *Samarasara)* we have an animal *(yoni,* 14 in number, probably derived from the Buddhist *nidana)* associated with each aster-ism, and also a tree of worship *(aradhyavrksa).* The origin of the latter is not to be traced from available sources. But the importance of the scheme is obvious, if tree signs and animal signs can be associated with letters of the alphabet. In orthodox Brahmin families, the initial letter of a child's name must be the *caranaksara* of the time of his birth; when some other name is given for any reason, the child gets a name with the proper initial for *sandhya.* purposes. Of course, the scheme has degenerated now, often the initial is taken as the first letter of the *naksatra* name. The symbolism would not be unambiguous, but its origin would be very interesting, whatever its application to the punchmarks. I might add that the nine rain-asterisms *(parjanyanaksatra)* have vehicles *(vahana)* which are, in rotation: horse, fox, frog, ram, peacock, mouse, buffalo, ass, elephant.

This brings us to the last of our possibilities: that many of the symbols on our punchmarked coinage, identifying the trees more closely than the present 'tree-with-railing' or 'tree-with-fruit', can represent the initials of the rulers in question. I offer this for what it may be worth, without excluding other and even simultaneous interpretations. The *Sadaracakra* and the sun-symbol being omitted, we should have to interpret three variable symbols as initials of names. Logically, these would be the name of the king issuing the coin, that of his father, and that of the founder of his line. In that case, for a reasonably prosperous and enduring dynasty, the king who rules longest [7] would have his symbol occurring oftenest: on his own coins and on those of his son or sons. As the common ancestor is fixed, we should have only one variable symbol out of the five, for a considerable group of the coins: the symbol that represents the king's father, and one or more sons who succeeded him. In some cases, this *might* account for the variable fifth symbol [3, 7]. The founder, or dynast, could get along even on four symbols. Inscriptions of contemporary Persian kings show a similar custom: *adam Darayavaus .... Vittaspahya putra, Hakhamanisiya* (on the Naks-e-Rustum inscription; at Behistun, the whole genealogy is given).

A part of this conjecture can be given a firmer basis than most other of the sort by a document that has already been used for the purpose of historical reconstruction: the Buddhist tantric work */[ryamanjuSri-mulakalpa* [28, 29, 30]. Here, many names are cited by the initial alone, such as king *Udiyi* [29, p. 324], Vidyaraja Ukarakhya [28, p. 284] and a series of monks [30]. In fact, Jayaswal was able to identify many of the known later Gupta kings by their initials [29, p. 53 *el. seq.],* and to make an ingenious guess equating Budha Gupta with a Praka-saditya known through his coinage. The connecting link was the initial *U* on the coins and a king with the initial *U* in the *Aryamah-jusrimuiakalpa* [29, pp. 38-9]. For our purpose, it is enough—in spite of the millennium separating the earlier punchmarked coins from the later Gupta issues—that the custom of placing a single initial on coins existed and is represented by more than one example [29, p. 60].

It is natural, in view of the fact that tantric documents are an untapped source, and that Buddhist tantras are the earliest known, whether or not they contain an earlier tradition, to see if the work mentioned gives other information that might be useful for the interpretation of our obscure symbolism. We see in fact, earlier in the work, a reference to a monosyllabic king or emperor: *ekaksara cakravarli* [28, p. 289], the cakra having twelve spokes, dvadasaram; Buddha is meant here, ln:t the symbolism is surely transferred from royal usage and terminology to Buddhist iconography. The *aksara,* by the way, is the famous *am,* or its equivalent, *mum* etc. [28, p. 284]. Now the word *mudra* occurs very often in the text, but usually as a posture or more particularly as a position of the hands, used in conjunction with certain *mantras* for achieving success of various kinds, and gaining control over superhuman beings. But there occurs one brief passage in which other *mudras* are mentioned, as popularly known. These are symbols of various sorts, and I quote the first three relevant *slokas* as an example [28, p. 430; also 28, p. 53, p. 91]:

द्विहस्तपादयोर्मूर्ध्ना एकहस्ताङ्गुल योजना ।
सर्वं तं मुद्रमिति प्रोक्तं आदिबुद्धैः पुरातनैः ॥
कलशं छत्रं तथा पद्मं ध्वज पताकं तथैव च ।
मत्स्य वज्र तथा शंङ्खः कुम्भश्चक्रस्तथैव च ॥
विविधा प्रहरणा लोके यावन्तस्ते परिकीर्तिताः ।
उत्पलाकारमुद्रं च सर्वे ते मुद्रानुमण्डले ॥

[8] The rest of the page goes on in the same manner, though not always in an intelligible language. The *svastika* is mentioned as an Aryan symbol :

दिव्यार्यौ च कुलौ मुख्यौ श्री वत्सस्वस्तिकौ लिखेत् ।

Clearly, these refer to accepted usages of Mahayana iconography, but the *praharana* referred to above are familiar enough to students of punchmarked coins, some being in fact components of the *sa-daracakra* itself, which has for its points 'arrows', *i.e., chatra* marks; sometimes the fish, the oval = *kumbha* or *kalasa* and others. The *damaru,* which occurs on several varieties of Taxilan *sadaracakras* is important in tantric literature of the later period, but not mentioned in our source [28] . The *vajra* I take to be the principal part of Walsh's symbol 21 [3, plate *i],* Allan's unidentified symbol [2, p. *xxxiv],* Prinsep's *jayadhvaja* [see also 2, p. 301]. The curious use of the word *praharana* in the passage quoted deserves mention. It cannot mean weapons, as would be the common meaning; I fail to find any mention of our heroes, however archaic, fighting with water-pots, fish, flowers! The inclusion of *dhvaja* and *pataka* makes it clear that here *praharana* is to be taken as insignia, just as 'coat-of-arms'. If, however, the original sense of the root, to strike, be retained, these marks would be *praharonamudra,* marks to be stamped, punchmarks. This meaning would seem too good to be true, so neatly does it fit in with our needs for the coinage problem. Yet, after these lines were written, Dr. V.V. Gokhale pointed out to me that the word itself actually occurs earlier in the work [28, p. 46]

padmam vajram parasu-khadga-trisula-gada-cakra-svastika-kalasa-minasan-khakundala-dhvaja-patakam pasa-ghantaka-dvarakadhanurnaraca-mudgara etairvividhakarapraharanamudraih. [also 28 pp. 408-9].

Of course, nothing is said in the text about stamping them on coins; but that they had mantric connotations is quite obvious. Not only do many of these occur on the coins, but they are still used in connection with the *sandhya* ritual, at least by some *Vaisnava* Brahmins. The *mudra* is made of copper, and used to imprint the mark in *gandha,* or even to brand it, on the worshipper's skin. In my own collection of such *mudras,* there is one which is also punched ori Golakhpur and Paiia coins [I, plates III, IV], as an obverse mark, besides being a reverse mark in later periods; none other than the Pythagorean hexagram formed of two equilateral triangles, with a dot or small circle at the centre.

Because I have to make use of *AMMK* later, a few words as to its reliability might not be out of place here. It carries the account to a much later period than the puranic lists which have, taking only the common part, been edited in the third century [27, p. *xiii]*. Again, the earlier puranic kings are not mentioned at all, not even Iksavaku, who is known to Buddhist pali tradition as Okkaka. The pre-Buddhist kings, i.e., before Bimbisara, are hardly considered except the Brahmadatta of the *Jatakas.* But for all later dynasties, the account would seem to be more sensible—where it is not carried away by religious prejudice—than the puranic text. Under these circumstances, it is necessary to use these royal names with extreme caution, if at all, for the purpose of identifying punchmarked coins. But in as much as there is little else available, I have been forced to make use of the puranic and the cited tantric document, in a later section. I need not point out that although the puranic chronology can be reduced to a shorter and more sensible duration of the reigns by taking certain alternative interpretations as suggested by Pargiter [27, pp. xxiii-xxv for example, *Satani trini* as 'hundred and three', not as 'three hundred'], and that Rapson found [8 pp. *xxv-xxvi]* the evidence quite useful for the history of the 'Andhra' dynasty, extraordinary mistakes can be made by relying on such evidence alone. For example the so-called Andhra kings were rulers of the Andhradesa when the *puranas* were written, but their dynastic name is Satavahana, and their origin certainly not Andhra, as was shown by Sukthankar [31], and yet, 'Andhra' kings and their progress to the west (when they actually advanced in the opposite direction) appear in histories like that of Vincent Smith. As another example, K.P. Jayaswal inserts English headings in the historical portion of the text of the *AMMK* collated with the Tibetan by the Ven. Rahula Sankrtyayana. These have to be used with caution: we find after *sloka* 320 of the text, the heading Saisunakas, whereas the word itself occurs nowhere in the source, and is undoubtedly derived from the *Puranas.* Jayaswal believes the famous minister Visnugupta Canakya to have been mentioned twice in the account [29, p. 17]. The first of these references (verse 454 ff.) is to Canakya, and the second to a harsh, irascible, unforgiving Brahmin (verse 963 *et. seq.),* no name being mentioned at all! Visnugupta does not occur here, and the puranic name is some form of Kautilya; I take it that the name Visnugupta was derived from the *Mudraraksasa* tradition, but there seems to be no excuse for actually putting it in this text as a heading.

In addition to explicit references to kings, there is little doubt that some real kings have been put in as demons by the Buddhist writer. Dr. V.V. Gokhale points out that the reference, on pp. 18 and 452 of Ganapati Sastri's text, to Naga kings is supposed to indicate kings of the serpents, and yet contains names not usually associated with serpents (nor the *Naga* tribe): Nanda, Upananda, Mahapadma, Sagara.

To revert to the punchmarks, I need not remark that some of the king names as given in these accounts lend themselves very readily' to direct representations by the symbolism of our punchmarked coins. For example *Sunga* means a fig-tree; the hare in a crescent or circular arc [33 plate II, nos. 55, 56] could certainly be read as *Sasanka.* The dog-with-puppy or hare-with-leveret mark on Walsh's group *B.*2 might symbolize *Sisupala.* Jayaswal read the name of a 'Saisunaka' emperor as Vata-Nandl [21, p. 95], identifying the statue as that of the puranic Nandi-vardhana [27, p. 22], son of the king whom Jayaswal calls Aja-Udayin. The combination Nandi + Vata does occur on two of our coins, in fact on Walsh's group *A.n,* and another mark can be associated with *Saisunaga* line. Yet this is a particularly unhappy conjecture, because it is difficult to account for there being no more than two coins of that group. In fact, if we look a little closer at Jayaswals sources, we find that Aja-Udayin is rather *a* doubtful form, there being very slender authority for the Aja [27. p. 22]. And Jayaswal fails altogether to give a satisfactory explanation for the Aja followed by Nandivardhana at the end of the preceding dynasty, the Pradyota [27, p. 19]. But as these predecessors were kings of Avanti, heir statues would not have been likely finds at Patna, and are back the beginning. Much as I admire Jayaswal's ingenuity, lament his untimely death, it must be said that the lawyer in him sometimes overcame the scholar.

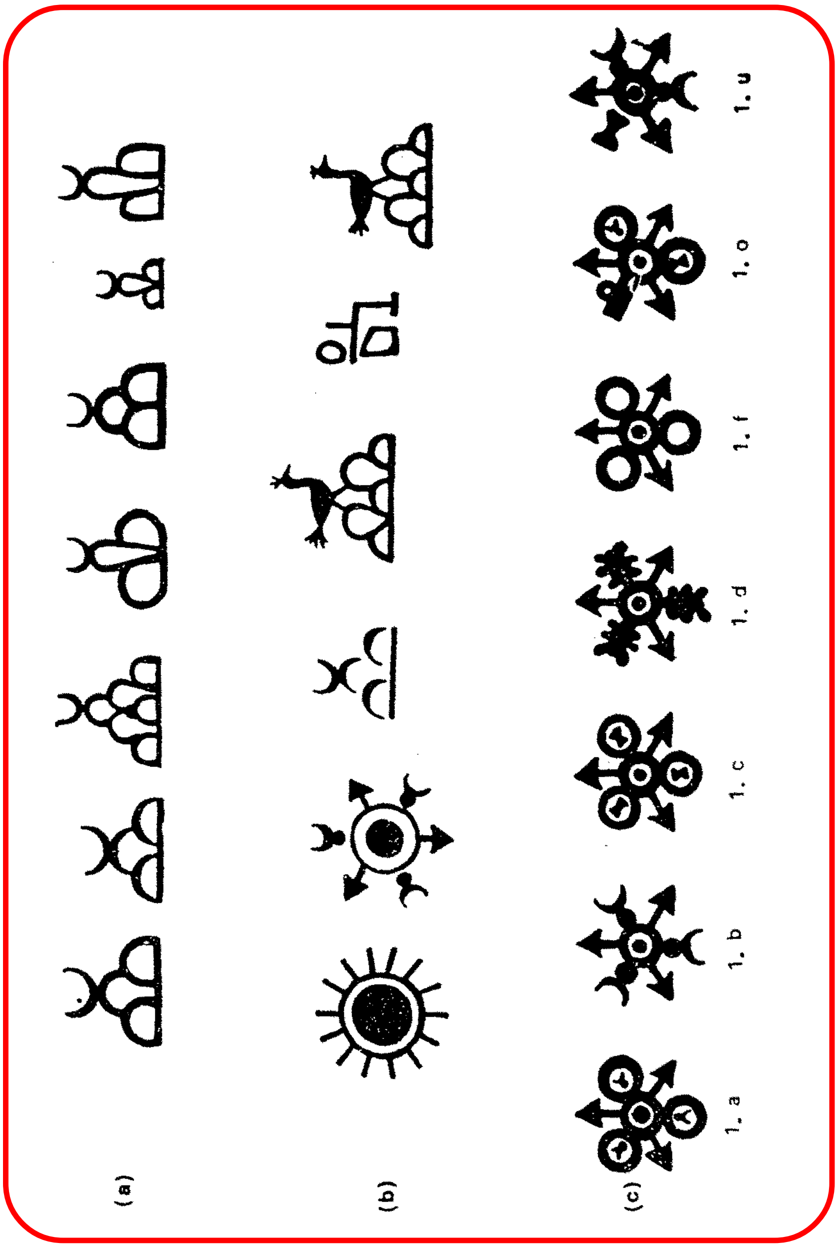


Fig. 4.1(a) Some forms of the 'creseent-on-arches' Candragupta mark.
(b) 'Peacock-on-arches', a Mauryan coinage (Candragupta?).
After Allan 2, pp. 25-26). *(c)* some forms of the *sadaracakra.*
The circular 'points' should IIP taken to be ovals

I shall make some use of one conjecture made by Jayaswal on quite admissible grounds: that the 'crescent-on-arches' mark is a sort of monogram first used by Candragupta Maurya, and then retained by his descendants on dynastic coins [i; 40; 3; 34; *JBORS,* 1934, pp. 282-288). Its varieties are given here for comparison (Fig. 4.ia)*.

Now the curious thing about this is that wherever it occurs as an *obverse* mark, it is associated with only one form of the *sadaracakra,* to wit Walsh's 1.6. This can be verified by a glance at Allan's catalogue [2, pp. 11-21, 25-32, 36, 40-41, 43-46]; Allan's index [2, p. 298] omits some of these. It follows, therefore, that the particular *sadaracakra* with three *chatras* (arrows) alternating with three *ma* (taurines) is the dynastic *cakra* of the Mauryas, the taurines not being enclosed in ovals, in contradistinction to other types of the *cakra.* We now note that some punchmarked coins with this *cakra* carry a peacock-on-arches (Fig. 4.1 b-c), it occurs on the obverse with Candragupta monogram; in other cases, it occurs also as a reverse mark. For the coins with a Mauryan *cakra,* this can only be taken to mean Maurya, the name of the dynasty; thus, the arches would signify 'descent from', at least the five arches. The peacock is the name mark of the founder of the dynasty in some remote past, or the equivalent of a *gotra* (clan, totem) mark; even further, it is likely that the monogram of Candragupta proclaims descent from the moon. A remark of Taranatha [36, p.2] can be so interpreted, and if the mark is not one of such descent, it is difficult to explain why three varieties of it also occur on coins of Nahapana and Satakarni *[JBBRAS,* XXII 1908, p. 241; also plate I, row 4; plate IV, rows i, 2 and 3]. The last variant of this mark given above, with an increased number of arches, could signify descent from a descendant of the moon, i.e., Candragupta. We have other marks of animals on arches: a hare (or dog) on Walsh's class *A. 1,* which would, if the arguments be extensible, indicate descent from *Sasa* (or if the frisking animal be taken as a young puppy, even from *Sisu),* and the bull-on-five-arches, descent from *nandi.* Walsh calls these latter marks hare-hill and bull-hill respectively, but the argumn: t that they were locality marks seems puerile to me.

The importance of the form of *sadaracakra* is emphasized here, and can be tested. The *cakra i.b* does occur in the earlier Taxila hoard, supposed to be a pre-Mauryan deposit. But it occurs only on five coins, clearly on just two [3, p. 40], and indicates that the dynasty was then not more than a petty local rule if needed it was the Mauryan dynasty. My suggestion, that the *sadaracakra* form be made the basis of a classification, has one difficulty in its way: that the actual form is not easy to identify onj ust one or two coins. Not only do wear and damage conceal the type, but the entire wheel is rarely to be seen on a single coin, and as the 'points' can vary a great deal, there is no way of determining the exact form except by reference to other symbols on the coin as compared in a group with other coins. Thus the wheel i.o given by Walsh can easily be mistaken for his *i.a* or *i.c* if only two points are decipherable. In fact, I think that this has happened in the case of Allan's class 2, Group VIII, var, *c, d,* 3 [2, pp. 52-53]. In particular, all three have distinct affinities with Walsh's class *D,* and I take them as actually belonging to that class, the *cakra* not having displayed all its points clearly.

Of course, the cakra of one dynasty may be adopted by some other, but it is unlikely unless the succession is by relationship; in case of war, you do not expect the conqueror to fly the flag of the vanquished. A king might change the form of his *sadaracakra* in the middle of his reign, but that would be unlikely unless some extensive changes took place in the nature of his rule—say great conquests or great losses. It is quite possible that the types had names. Rajuvula lables his coins *apratihatacakrasa* [2, pp. *cxiv,* 185], but as he does not stamp any form of the *cakra* itself, this leads us nowhere.

All the foregoing has been written only to point out some neglected possibilities, and also to show that as far as mere conjecture goes, a novice can compete with veterans. The problem of deciphering the symbols on these coins is at least of the order of magnitude of making sen e out of medieval European coats-of-arms in the absence of any text on heraldry, any inscription on a tomb, as a guide. But it would not be fair to let the reader wade through this lengthy discussion without some indication of my own working hypothesis as to the meaning of the symbols. This I formulated as a tentative guide, after the statistical work of the memoir was finished:

The 'sun' symbol is so universal as to be devoid of any particular significance, though its absence on coins with homo signs might indicate an association with personal sovereignty, rule by divine right. The *sadaracakra,* as has been said, is the particular mark of the dynasty. Of the three remaining marks, any that occurs on arches signifies descent, being a clan mark, or a totem symbol. There are four constant marks on most coin-groups, and the fourth I take to be the seal of the ruler under whose authority the coins were issued. The fifth 'variable' mark is probably, in spite of my previous suggestion, not that of a son but that of the issuing authority, whether a subordinate princeling, a minister, or a mint master; of course one person could hold two or more of these offices together, and even in his father's reign. But usually, the fifth mark is not repeated in the next group of coins. It is to be noted that the fifth mark is, in my opinion, the individual seal or monogram, and not the mark designating the particular office.

Occasionally, the same marks occur with two different forms of the *sadaracakra* [33, class II, group v]. It seems to me, studying the individual 12 cases, that the lesser issue was by a subordinate dynasty or ruler under the general hegemony of the greater, such as the Mauryan; for homo signs, again in conjunction with one or two of the marks on the general coinage, some form of restricted tribal autonomy would be indicated. The usual number of five marks is surely derived from mantric tradition, which always mentions the *pancamahdmudra.*

2 PRESENT STATE OF THE METROLOGY

The most important characteristics of the coins were undoubtedly the composition and the weight. Whereas the *Arthasastra* in a much-quoted passage gives the alloy of the coins: [*A.* II, 12, 30; Meyer, 9, p. 120].

लक्षणाध्यक्षः चतुर्भागताम्रं रूप्यरूपं तीक्ष्णत्रपुसीसाञ्जनानामन्यतमं माषबीजयुक्तं
कारयेत् पणमर्धपणं पादमष्टभागमिति; पादाजीवं ताम्ररूपं माषकमर्धमाषकं
काकिणीमर्धकाकिणीमिति ।

This alloy or its approximation is to be found only in later coins, such as the second Taxila hoard, which Walsh considers 'debased'. In addition, the poorer craftsmanship and increased variance of weighta of this later hoard show that the life had gone out of the puuchmarked system of coinage; in fact, the ability to alloy on such a scale without loss would also imply the ability to alloy the coins, and contemporary Greek influence, if any would provide additional impetus in the same direction. If, however, use is made of the constitution of the coin itself, it will have to be based on any assay of many samples of *every* group, preferably an assay of every known coin, and not a single representative. This means damaging the coin in some way, though a boring edgewise into the coin might do the trick with minimum harm. The assay of a single coin will tell very little, as also the rougher analysis giving 'traces of impurities' such as lead, gold, etc. It is precisely these small impurities that accurately characterize the source of the metal, and if they were determined properly, one could indicate the locality from which the metal was imported, w·thout relying upon the *Arthasastra* alone (II, 13, 31. Meyer p. 123; *Assam, the Tuttha mountain,* etc.), or a doubtful reference in the Bible (Jer. x, *v,* 9) which might have absolutely nothing to do with India. The assay and some test-drilling can decide with accurate density measurements whether the obscure reference above to *masadbija* implies an alloy, as Meyer seems to think, or a core, as would seem likely from the mention of iron in the list of metals to be used.

This leaves us, then, with the most obvious quality of the coinage, the weight. The usual study has been based on two assumptions of unequal value: first, that the system of weights proceeds by the binary (dual) or quadragesimal scale; and secondly, that the basis of the system was the *ratti-raktika-krisriala,-gunja,* the seed of the *abrus precatorius.* The first of these is very likely indeed, as Indo-Aryan Linguistic survivals of the dual system rise to 8 units and we find it in use for all periods, from the Mohenjodaro finds (7, Chapter XXIX] to the present day, when accounts are still recorded in the Indian market place by a quadragesimal notation, employing alternately horizontal and vertical strokes in place of numerals. Of course, the decimal system is also used conjointly, and the combination might be said to characterize the Indie civilisation, just as the use of the sexagesimal and the decimal system characterizes early Mesopotamia [Childe, 11, 112] in the fourth millennium B.C. It is the second assumption that leads to trouble.

All known ancient standards of currency and commercial weights can be assumed to have been based upon cereal grains or seeds [Ridge-way, 20], as is shown by philological survivals such as carat, grain, etc. for modern weights. In particular, the *ratti* is still used by our goldsmiths and jewellers. But it is quite ridiculous to work back from the average of these to ancient times, and to except our coins to tally. Yet, Gunningham's average of 1.86 grains is cited by our numismatists, who puzzle over the fact that even unworn punchmarked coins are several grains underweight. The obvious explanation, that the *ratti* seeds vary enormously, and that the ancient had not the respect for Cunningham shown nowadays, seems not to have struck our experts. Durga Prasad even makes the astounding statement [i, p. 13]"... the coins are *Ardha Karshapanas* weighing on an average 14 rattis of 25.2 grains, having lost 3.4 grains by wear and tear". The coins might be *half-karsapanas* but there is no evidence whatsoever that they ever weighed 16 of Durga Prasad's variable *ratis,* and to say as he does in the same passage that a single coin has lost so much weight by usage from what it ought to have been had it been a *dvi-pana* of 32 doubtful *ratlis* is a sad commentary on the procedure of at least one authority on punchmarked coins. In a single page of Durga Prasad, the *ratti* used .works out at 1.43, 1.80, 1.85, 1.89 grains. Walsh [3, pp. 15-17] is equally helpless in the matter. Weighing modern goldsmith's *rattis,* he concludes, "It is therefore clear that at the present time only the largest seeds are used as weights, and Cunningham's *full* weight is correct, and, on present practice, the theoretical and actual weight of these' coins cannot be reconciled". Yet, four lines above, he says about his own experiments "400 average seeds weighed 673 grains, giving an average weight of 1.68 grains". Leaving aside the remarkable procedure of obtaining the average weight of the seed by selecting the average seeds first and then weighing them, it would have been found that 32 of Walsh's own *rattis* would have come to within a grain of the average coin in his own tables. His touching faith in Cunnin-gham is unfortunately of very little use; 'present practice' is based on the fact that an honest goldsmith or jeweller will choose his seeds to conform to the measure of 96 per *tola* (180 or 183.75 gr.)- I submit the opinion that the *ratti* was not used, even in ancient times, to weigh the coins, but rather the coins determined the choice of the seed, exactly as at present.

My opinion is based on the fact that, even now, the goldsmith uses only one or two seeds, in either pan of the balance to make up the weight. This, one feels would have been the practice in all but the most primitive times, and the people who punched these coins were not primitive in that sense of the word. At Mohenjodaro, weights have been found which I analyse later on in this work, and the average of class D given by Hemmy [7, p. 590, table I] agree to within a fraction of a grain with the coin weight average that I have found from Appendices XI, XII of Memoir 59.

Our numismatists could have saved themselves a lot of trouble by referring to Sanskrit dictionaries *sub* the words *gunja, raktika, masaka.* The identification with *Abrus precatorius* is uniform, but little else. V.S. Apte gives the average weight as 15/61 gr., without reference; this is likely to be a misprint in copying from Monier Williams, who gives *(gunja)* the weight as 15/16 gr., Bohtlingk and Roth give the badly needed reference (under *raktika)* to the *Journal of the Royal Asiatic Society* of London, New Series, Vol. II, 1866. This issue contains (pp. 145-224) an article by Edward Thomas on the initial coinage of Bengal. In the note on pp. 151-3, Thomas quotes a letter of N. S. Maskelyne, then of the British Museum, in detail; I recommend its perusal for those who write on the subject of punchmarked coins. We read as Mas-kelyne's opinion (p. 152), "Nor can you get any result from weighing carob beans to determine the carat, or Abrus seeds to determine the *ratti"*. Maskelyne proceeds to give all averages known to him. from the 1.318 of Sir William Jones (probably the source of Monier William's standard) to a theoretical 2.483 from Mohammedan coinage, supported by documentary evidence. Maskelyne obtained, by his own experiments, an average of i .694 gr. fort he *ratti* which is close to that of Walsh. In conclusion, let me state that any of these averages is well within the range of probability, as even small samples of the *ratti* show enormous variation in weight. My experiments on this point are not yet complete, as I have weighed only seven hundred of these *one by one;* but I can assure the reader that even from a single vine one can select seeds that agree with any of these averages. The seeds that are the largest in appearance are by no means the heaviest, and it is impossible to grade their weights by eye or by hand; a single local sample of 117 showed all weights from 0.07 to 0.16 gm., and this spread is characteristic of such samples from any part of the country, unless they have been specially chosen. The average, in these cases, is immaterial, and can say nothing about the weight of the coins under discussion. I found the average to be 1.864 grains, which does not indicate that Gunhin-gham's is the 'true' value, but that his samples were, like mine, fresh, mostly from the season's crop. The standard deviation is 0.2754, which means that if we take a *raktika* at random, we are about as likely as not to get one of weight differing from the mean value by approximately a fifth of a grain. Also, it means that the coins could never have been weighed against 32 *raktika* taken at random, because the variances would then have been ten times the maximum now observed in any reasonably large sample.

For the rest, documentary sources regarding ancient standards of weight do not agree, as was shown by Golebrooke in his *Essays.* It was known to the meticulous Rapson, even if sometimes forgotten by later scholars, that the *masaka* varied for copper, silver, gold; and often, with the period and locality [8, pp. *clxxvii-dxxxii).* Some help might have been obtained in these matters from Government reports, but the one I have been able to consult is [12] a most disappointing document, containing only the usual display of bureaucratic incompetence, mixed with great contempt for native usage. The committee making this report was more concerned with the status of the witnesses than with the actual weight observed and its variations.

Mr. A.S. Hemmy, former Principal of the Government College, Lahore, has devoted several papers to the study of weights and currency standards [7, 24, 25] of ancient India. Properly done, this work would have saved me a great deal of trouble, and enabled my principal conclusions to be set forth without this mass of extraneous criticism. Unfortunately, Hemmy's idea of statistics seems to belong to a school of his own, and his conclusions, when hot absurd, can be obtained by mere inspection of the tabulated data, without any attempt at pseudo-statistical analysis. He starts with the 'Law of Error', giving a distribution of measurements about a central or'true' value $y = k\exp(h^2x^2)$. This is rather an antiquated way of putting what is now called a normal distribution, and would not be wrong if only Hemmy showed some consciousness of the fact that for unit area, k=h/v/ii. BuUHemmy estimates his constants in a strange fashion. First, he groups his data for coins by the rather large steps of one grain [24, pp. 10, 25, 672]. This is permissible and even sensible, but the next step is neither: he obtains a curve by *smoothing* the points out [24, p. 10]. He then fits a probability curve, sometimes with a further imaginary allowance for loss of weight, etc. but taking the *h* and *k* to suit himself, without being troubled by such a thing as goodness of fit. He is, finally, quite pleased that there is close agreement between theory and practice! As a matter of fact, his grouping gives a histogram, and the rounding off is better applicable to a frequency polygon. Again, there are excellent methods for the estimation of statistical parameters [13, 14, and 15, p. 186 ff.]; and in any case, the central value for normal distribution is better estimated by the observed average than by the maximum of a smoothed curve that Hemmy uses in all his work. In the work on weights of the Indus [7, p. 25] system, he uses the mean deviation, apparently the mean absolute deviation, with the average vaiue; but the median should be used if mean absolute deviations are to be taken [14, p. 32], or the average given with the standard deviation (or variance). If imposing technical terms are to be used to impress archaeologists and orientalists, at least the most useful ones should be taken, and an attempt made to use them properly. And there are, even for curve fitting, far better methods available than just smoothing coarsely grouped data. Hemmy's procedure is on the same level as 'smoothing' the newspaper caricature of a celebrity and then expecting to get a photographic resemblance to the original. A strongly marked feature would survive, but the rest are more likely to be obliterated.

The consequences of this procedure are quite clear when one regards Hemmy's conclusions. When the sample is small, he is quite helpless, though undaunted; this is seen by his approach to the tribal and city coins [24, pp. 16-24]; and to the 'aberrant' Mohenjodaro weights [7, p. 591] for which he discovered a system, about the existence of which he began to have doubts with more data [25 p. 604]. His analysis of the evidence for change (with difference of level) of system of weights at Mohenjodaro proceeds by comparing mean deviations and averages, and ignores the existence of the *t* and *z* tests [25, pp. 605-6). He finds little in common with Mohenjodaro and contemporary Egyptian and Babylonian weights, but does not hesitate to state that some of the silver punchmarked coins have an affinity with ihe Daric standard [24, pp. 25-26]. Yet he found that most of the silver punchmarked coins are weighed according to the Mohenjodaro system, having a theoretical average weight close to 1/4 of the theoretical Mohenjodaro principal unit, [24, pp. 10-12]. This close correspondence between two fictitious quantities seems quite rational and conclusive to him. Not the most ridiculous of his conclusion is "The uniformity of distribution of weight in punchmarked coins, both silver and, copper, shows that those conforming to the Indus standard must have come from a single mint. Their widespread provenance indicates the Mauryan Empire, and the uniformity of weight indicates strict and capable administration. This points to Asoka". Walsh noted the futility of this notion [24, pp. 293-304]. As a matter of fact, the uniformity of weight is due more to Hemmy than to Asoka; the British Museum coins, which he lumps together in one lot, came from widely scattered regions; a unified provenance, such as we have for our Taxila hoards, would have given him the conclusion, had he known of recent developments in statistics due to his own countrymen, that the Mauryan empire was less efficient than its immediate predecessor at least for the Taxila region. A method exists for analysing such data [19], but the weights speak very clearly for themselves, and I can draw slighty different conclusions from Hemmy's. In the first place, the actually observed weights of his class *D,* even for the earlier Indus excavations, bracket the observed weights of most of the Taxila coins. The variances of the weights given in Hemmy's first report [7, p. *xxix]* were compatible by the *z* test with the first of the hoards, not the later one. In as much as the Indus system contains both .decimal and binary multiples, it would be worth while to look for such fractions of the lowest weights found. In fact, a plausible conclusion is that the *raktika* is the basis of the system. From the weighted averages of all groups given by Hemmy, except *C,* [25, p. 602], this is estimated to be about 0.106 grams, which is about 1.636 grains, 'and thus close to the experimental averages obtained by Walsh and Maskelyne. If, now, decimal and binary multiples and fractions are allowed we find almost all the aberrant weights that nonpluss Hemmy fall into place. In addition, his class *C,* (which he puts at the awkward fraction 8/3 of unit *A)* is 20 *raktika* in weight, approximately. The lowest weight found, 0.55 gm. is, in my opinion 5 *raktika,* admissible in as much as it is a half of ten. Hemmy came very near to this conclusion [7, p. 596] when he divided the Group E weights by 60 and found: "the coincidence between the ratti and the dividend by 60 is tempting, but as there is no evidence in favour of a sexagesimal system, I am more inclined to prefer the relation between the rice grain and the dividend by 200". While admiring his manly resistance of temptation, one is inclined to wonder why he assigned the weight 8/3 to class *C* (there are at least two clear misprints in his Table I, 7, p. 590, for that class), and why he did not divide by 64. I might add that rice had not then (and perhaps has not yet) been found at Mohenjodaro, though both wheat and barley occur [7, p. 586]. For that matter, no *ratis* have been found either, but these last are subject to borers and decay very rapidly, the oldest sample. I have been able to obtain being not more than 50 years old, and mostly wormeaten. Hemmy's class A seems to me to be a *masaka* of 8 *raktika* weight, and the later *masaka* description of 5, 6, 7, 7.5 *raktika* standard would probably indicate local usages, and show not so much that the *masaka* varied as that at first people chose the *raktika* to suit it, the *masaka* b;ing fixed. The various standards for gold, silver, etc. could have developed this way. The *masa* bean of Sanskrit tradition is the *Phaseolus radiatus,* far too light for even the 5 raktika *masaka.*

Before coming to my own work on coin statistics, let me add that the 'best estimate' of the *raktikā* on the basis of the χ^2 test would be somewhat higher. If u be the unit, a_{ij} the ith weight of the jth group, n_j the number in the jth group, r_j the expected multiple of the group, in terms of u, $a_j = \frac{1}{n_j} \sum_i a_i$, its average or mean value, and $x_{ij} - a_j$. the residual, we can formulate χ^2 as

$$\chi^2 = \sum_j \sum_i \frac{(a_{ij} - ur_j)^2}{ur_j}$$

This estimate of u that makes this a minimum is immediately obtained by a simple application of the differential calculus as

$$u^2 \sum_j n_j r_j = \sum_j \left(\frac{n^2{}_j a^2{}_j}{r_j} - \frac{1}{r_j} \sum_i \sum_{\substack{k \\ i \neq k}} a_{ij} a_{kj} \right)$$

This *u* can be calculated even for a single group, and used in place of the average or median, though the weighed mean is theoretically better. For Susan weights, which "form a very complex series running almost continuously from 0.95 gm. to nearly 90 gm. before the first real break" [25, p. 675], the method used by Cramer [19] in determining a Mayan unit of linear measure would be applicable, with the caution that linear dimensions are not so likely to depreciate as weights. I hope that an analysis of the Mesopotamian weights will be made by this last scheme, as also one of the units of measurement at Mohenjo-daro.

D.R. Bhandarkar [6, p. 12], finds that Spooner's Peshwar coins tell a strange tale: "they reveal a gradation of weights, each gradation marked by 1.83 grains, i.e., exactly by half Masha". This is suspicious enough, and suspicion becomes deeper when we consult his chart of the hoard [6, p. 123]. There, for example, we find no less than twenty-one coins weighing exactly 45.75 grains. Even to have a single coin honestly weighed twenty-one different times on the usual laboratory scales and to have weight come out the same each time to a hundredth of a grain would be a nice piece of work. To find in an ancient hoard of coins originally 'badly corroded' [32, p. 150], 21 coins weighing the same to a hundredth of a grain would be a superior miracle. Here, Bhandarkar is not to blame for the data, only for his gullibility in accepting it. Turning to Spooner's report, we find an imposing table [23, p. 159] of conversion from *ratis* and *masas* (= 8 *rati)* into grains, at 1.83 grains per *rati.* Nothing whatsoever is said as to how the coins were actually weighed, but the weights are given by masa + *rati* and also to the hundredth of a grain, on pp. 160-164. If Spooner had them weighed by the *masa-rati* scale, he should at least have said that he had checked them to see that the weight was accurately transferable to grains. If he weighed the coins to a hundredth of a grain, he surely rounded off the weights to fit the *masa* scale.

I wrote to the Director General of Archaeology in India, at his own request, asking for accurate information about discordant Mohenjo-daro weights, and the actual weights of suspect coins. The reply, dated November 18 (to my letter of July 12, 1940) contained a painstaking report on 4 by the Curator of the Taxila Museum giving, among other matters, the corrections to 19 misprints in the published weights of Walsh's Appendices XI-XII. None of the coins having been actually reweighed, and the information I needed not being available, it was not possible to make full use of the revised data, particularly in view of the fact that this paper had already gone to press. But all figures given in Table I and those in Table III for group *D.2* and *B* (e) 2 were recalculated in haste. Thanks are due to Rao Bahadur K.N. Dikshit and to his Curator at Taxila for the trouble they have taken.

3 MAJOR GROUPS: THE KARSAPANA

We come now to the coins themselves. By methods explained later (or well known to those who can read *13, 14, 15, 16, 17)* I calculate the necessary statistics, taking as basis the weights entered in 3, App. XI, App. XII. However, the hundredths of a grain weight has been ignored except in the case of the minute coins. The notation is, n = number of coins in the group, m = mean or average value of the weight in grains, s^2 the variance. The general unreliability of Walsh's work [4] is not likely to make any serious difference in the weights, as the weighing was, apparently, not done by him [3, p. *i:* "the examination was made from the photographs of the coins"].

The long-bar coins have to be compared to Persian sigloi, and will involve a study of Persian hoards. Bhandarkar's identification of this type with the *Satamana* [3, 3: 6, pp. 56-58] seems doubtful to me because the measure *(mana)* is not specified ('see Bohtlingk & Roth s.v.

TABLE 4.1: GENERAL CHARACTERISTICS OF THE COINS

group	n (number)	m (mean)	σ^2 (variance)	median
Long Bar Coins	33	175.6	4.5657	175.9
Minute ,,	79	2.639	.037389	2.68
Double Obverse	64	49.47	1.5903	49.6
Eearlier *Kārṣāpaṇa*	995	52.63	1.6354	52.9
Both above classes	1059	52.44	2.1983	52.8
Later Coins (3, App. XII)	162	52.71	5.6752	52.4
(Separate coinage)	5	49.56	4.393	50.0

dharana); if the *rati* was meant, it should have an average weight of 1.57 gr., which is more than 1/32 *karsapana.* There is no weight of this *Satamana* standard in the Indus finds.

The minute coins, the small change of the day, are taken by Walsh [3, P- 3] to be the pana of two rati weight. But he quotes the *Artha-saslra* to give a *dharana* of 16 *masa,* which would not be heavy enough on the average given in the Table 4.1. It is to be noted that Walsh says, on the same page, "their weights vary from 2.3 to 2.86 grains", which is definitely wrong; App. XI gives coins 40, 48, 49 as not less than 3 grains. If these coins represented two *raktika,* the average *raktika* would be slightly lower than the Indus standard, though by no means impossible, being close to Sir William Jones's average. But the main coinage would represent a multiple of twenty small ones. For this multiple, there is also documentary evidence, as we have the *Narada Smrti*—whatever its date-saying:

Masho Vishtayo Bhagha Panasya Parikeertitaha

The variances are such that twenty ol" these coins would not, unless carefully selected, give accurate approximation to the main coin, to within the ancient limits of observation. But it must have been relatively much more difficult to mint the smaller coins weight, and they might have been more worn, having more circulation than the larger ones; both factors would increase the variance. I call them 1/20 *karsapana* and let it go at that.

The main body of the coins of both hoards have been called *karsapana* in my table, though their actual nomenclature is doubtful. The *Arthasastra* uses *pana* for (silver) coinage in general, and by the time of our present recension of the *Manusmrti,* the coins had become archaic: *purana.* The term continued as literary usage, and we get the traditional description:

Karsharpanastu Vighneya Tamrigha Karshigha Panha

This can be translated as *Karsapana = copper* coin one *karsa* in weight', and our lexicographers (see in particular the *Amarakosa* with Mahesvarabhatta's comment) give *karsa=16 masaka,* which removes the question to its final stage of insoluble doubt: what was the weight of their *masaka?* The other interpretation of the above passage (I exclude the more fantastic ones) would be *karsapana=* the farmer's copper coin. There is a bit more to be said for this interpretation than would appear. We never find copper *karsapana's* in the oldest hoards — whether because there weren't any, as I incline to think, or because they were not considered worth hoarding then, or because our archaeology is still in its initial stages, the fact remains that the earliest hoards are of silver coins.

But we begin, later on, to get debased and plated coins with much the same sort of marks. Finally, we have the billon coins replaced by copper, perhaps the 'peasant's copper coin'. We also hear that the coin was practically worthless :

Vachaspatyam Karshapaniyaka Mulya Hi Daridranam Prakrititaha

I and this is supported by our *pali* tradition. The one explanation that fits in all this is that our coin repeated the history of the Roman solidus and denarius (C. Oman, 22, pp. 37-60). To trace it through undated and uncritical literary sources would be difficult, as can be seen from the example of what happens to a known coin, the *dinara,* which is considered to be a Kusana adaptation of the contemporary Roman denarius. The *Siddhanta Kaumudi* gives:

Didnurah Dinarah Suvarnabharnam

a commentator gives the etymology dinara, and the *Vacaspatya : di arak nut.* A glance at the dictionaries shows that the coin is of varying weight and import, being equivalent, among other things, to a *niska;* and is often taken as an ornament, not a coin. Because of this, we should not be surprised at anything said of the *karsapana.* From the commentary of Maheshvarabhatta to the *Amarakosa,* we can work out the equation *karsapana*= 16 *pana*= 64 *kakini* and a *kakini* is not only the *gunja* but also the cowrie shell. That is, the lexicographers do not always give a weight equivalent, but slip off into values in terms of small change. Mahesvara-bhatta's comment of *paisa iti khyataya* for the copper and *rupaya iti khyaatsya* for the silver *karsapana* shows that he carries out the general tradition of assimilating an ancient name to coins in contemporary circulation. The Pali tradition is summed up in a letter from my father, Prof. Dharmananda Kosambl: "...The description of a kahapana in the *Vinaya Pitaka* (cf. Vin. *Hi,* p. 294) is older than that in the *Atthasylini.* There *rajata* is given to mean *kahapano, lohamasako darumasako, jatumdsako.* On this, the *Samantapasadika* comments: *kahapano ti sovannamayo va rupiyamayo va pdkatiko va, Lohamasako ti tamba-lohddihi kalamasako; darumasakao ti saradarund va velupisikayi va aniamasai talapannenapi rupam chinditva katamasako:' jatumdsako ti lakhayi va niyydsena va rupam samutthdpetva katamasako . . . antamaso atthimayo pi, cammamayj pi rukkhaphalabijamayi pi samutthanita rapo pi asamutthapita "Rup pi.* This shows that *kahapina* means coin in general. Nevertheless, the term was particularly used for gold and silver coins. *Masaka* means a small coin *Lohamasaka* means coins of copper or other base metal. The wooden *masaka.* was carved on pieces of sara wood, bamboo, or palm-lead. Bones, leather, seeds were also used. Cowries are not included in the list. That is, their value was even lower jhan one *masaka,* and they were not counted as coins.... The *Jataka's* have this *gatha:*

saddahassi sigdlassa surapitassa brahmana
sippikdnam satam natthi kuto kamsasata duve

(*Jat.* i, 426)

“He hasn’t a hundred cowries, how could he have two hundred bronze coins?

“Those who examined coins were called *herannika* (Sanskrit *haira-nyika) Heranilika’s* are described in the *Visuddhimagga,* 14, 4....”

The comment on the *gaika* quoted above: *kamso iti kahapano* shows that the *karsapana* had been debased by that time.

After going lo press, I obtained access to magnificent paper in the finest tradition of German sonolarship, by H. Luders: *Die Sakischen Mura* [Sitz. Preuss. Akad. Wiss., Phil-Hist. Klasse, 1918-19, pp. 734-766]. It would have saved days of work for me had it come to hand earlier. The reader cannot do better than to consult it himself, as it is surely the profoundest discussion to date of all aspects except the mantric one of the word *mudra.*

To revert to Table 4.1, it is clear that the double-obverse coins which Walsh describes as greatly worn, are much lighter than the others. Assuming the *t* test [13, p. 177; 14, pp. 131-5] applicable, we have here for the 64 double obverse as against 995 *karsapana* 1=19.17. This indicates a negligible probability that the double obverse coins were meant to represent the same weight as the *mainkarSaparias* of the earlier Taxila hoard. The double obverse lot can therefore be taken as much older, and is significantly below the standard weight. The ancients must have recognized this difference, as it is (dealing with mean values only), of the order of two *raktikas.* Perhaps, this much was allowed for in the current market value of the coins, and the second obverse is a “mark of this. Against this last conjecture is the fact that the heaviest of these, No. 838, weights 52.1 grains, which overlaps the lower range in standard *karsapana* weight. I shall also suggest, later on, a political reason for the second obverse. The variances of double obverse and *karsapana* coins are not significantly different according to the *z* test. That is, the general method of manufacture and allowances must have been about the same for both.

The difference between the *t* test and the *z* test becomes clear when we consider the case of the latter coins as compared to the *karsapanas.* The difference of means is 4/07/5 gr., which signifies absolutely nothing; both coinages could represent the same unit of weight. But the variances are significant by the *z* test, and the chances are much less than one in a thousand that the two lots were weighed by the same system. The later people were very careless, or had a rougher set of scales or allowed more legal variation; with the debased alloy and rougher punches, the evidence towards a cruder technique seems to accumulate. Now if we take the five coins of a separate coinage in the later hoard, the difference of means seems to be large, when compared to the 162 just preceding. As a matter of fact, by the *t* test, we find a value of $t=2.917$ approximately, which again carries us to a level of significance that gives less than one chance in a hundred of the two means belonging to coins chosen at random from the same general lot; but by the *z* test, the difference is not significant at all, *z* for these two groups being 0.12805, which gives better than one chance in five that the two are weighed according to the same system. That is, the five coins of a separate coinage belong to the later period, but are of earlier manufacture, and have been longer in circulation.

To extend this a little further, consider Allan's British Museum list [2]. On pp. 11-15, he gives details of a set of coins that he indicates by Class I, Group I, variety *a.* My calculations for this lot give the statistics: n=58, m=53-34 and t^2= 7.9476. By mean values alone, if we ignored the punchmarks themselves, this might belong to the period of either of the two Taxila hoards. But the difference of variances shows clearly that if it belongs to cither, it must belong to the second group. Applying the *z* test to the British Museum sample and the later Taxila hoard, we see $z = 0.$ 16837, approx. and that the value is just not significant at the 5 per cent level. Had coins 52 and 19 not been present—they are 44.3 and 46 gr. respectively—the difference in averages and variances would have been negligible. It is plausible then, that the coins belong to a period and manufacture comparable to that of the later Taxila hoard. This is not quite the same conclusion as that which could be reached by study of the marks alone, as a glance at the plates given by Walsh [3, plates XLII-XLVIII] and Allan [2, plate II] will show that the British Museum coins show better workmanship. They should have shown less variance also, but for the fact that the later Taxila hoard consisted more or less of newly minted coins of weighing, as well as by the archaeological evidence and the marks given by Walsh and Allan that both belong to the Mauryan period.

If, however, we try to extend this to the older Taxila hoard, the method fails altogether, and shows the limitations of statistics. The largest number in common with the British Museum is their class 6, Group III, Var. *c.* which Walsh [3, p. 28] puts in his own class *A.i.* These are the most numerous class of coins of the older hoard, and in fact characterize the older punchmarked class by their substantial proportion in finds throughout India. But our statistical analysis will show a significant difference in weights and variances. The reason for this isnot that the British Museum coins were originally different, but more probably that they had a decidedly different history. The Taxila hoard was underground for over two thousand years, without use or wear. The British Museum coins of the same style [2, pp. 66-69] were gathered by various people in widely scattered localities, and have not a common provenance. The minimum weight is 30 gr. (no. 16, 2, 67) and the maximum 52.8 (nos. 22 and 27), which shows that the coins were much more worn on the whole. The British Museum Class i, Group I, variety *a* could be dealt with only because almost all the coins come from the Swiney collection, and have presumably just the unitary provenance needed.

I trust that this shows the usefulness of studying *groups* (not individual specimens) of ancient coins by weight. The primary condition is that their history should be as nearly the same as possible. For this purpose, hoards closed at an early date are the best, and it is for this reason that I have chosen Walsh's memoir, in spite of its defects [4]. If there are small errors in weighing they will disappear in the group statistics; for a large number of coins, even a comparatively serious error, or an occasional coin having an aberrant weight will make no significant difference. It is the small sample that needs much more careful handling, as I shall show in the next section. If my study of the coin weights is valid, it follows that the people of Mohenjodaro and the older Taxila hoard had weights and balances comparable in quality, and that they were pretty good; better, at any rate, than those of the Mauryan period. What the reason is I do not know, bui the chances are that the wider extent of the Mauryan empire allowed a greater latitude in weighing, and perhaps that the older coins themselves began to be taken as standard weights, instead of the neat stone weights of the Indus valley. The various standards of the *raktika* and the *masa,* which had probably been localized before, must also have tended to cause a greater variation. The point cannot be discussed properly without analysing many more hoards. For all that, the weights even in the later period were fairly good. D.R. Bhandarkar attempted to explain the greater variation as a deliberate deceit practised upon the people of the empire [6, p. 116]. His method was to blindfold people and ask them to estimate weights, by which procedure he arrived at the conclusion that "the ordinary human hand .... cannot unaided detect a difference of even 15 garins". This would do nicely as a parlour game, but is of doubtful value in assessing the currency standard of a bygone age. The difference between the lightest and heaviest coins of the. older Taxila hoard is something like twelve grains, and the light coins are suspect for rcasorn that will appear later.

The weight of the coins, before Mauryan ?} debasement of the alloy set in (for the sake of saving wear on the coins, or to relieve the shortage of currency in a country that had to import its silver, or on the Athenian (Solonian) model to relieve the debtors—the weight!""! say, must have been the important characteristic. There can hardly have been any such thing as legal tender, except that the coin represented a certain value of metal. As silver was then relatively much rarer than now [Meyer 9, p. 319, line 26], people would have been more likely to weigh their coins than in a later age; and we have seen that for the best part of three millennia B.C. they had rather accurate sets of weights. Even as late as a hundred years ago, I feel convinced that an Indian goldsmith or moneylender (the professions were not seldom combined) would have, when a customer presented a coin-of the older Taxila hoard, valued it by taking a streak of colour on his touchstone and weighing the coin; and accepted it for payment accordingly. The marks would have signified nothing. Even today, British Indian coins are current in state like that of Hyderabad, which has a coinage of its own that is not accepted in British India. Still better, British rupees are legal tender, or at least current in the marketplaces, throughout Portuguese Indian territory. I remember seeing in the summer of 1916 or 1917, in the till of a single village shop in Goa, Portuguese and British Indian coins, Australian half-crowns, English shillings, American cents, and in a word the small change of almost all the world. Both shopkeeper and customer accepted the coins as equivalent to the nearest Indian coin in appearance and weight, and this helped to relieve the currency shortage caused by the war of 1914-18. The variety of coins was unusual for India, but to be explained by the fact that a large number of Goanese found employment on ships that sail to all corners of the world, and brought back the local currency with them. Incidentally, banknotes were accepted only if British or Portuguese—*Indian* issue, and the notes of lower denominations issued for Goa as small change were not willingly accepted at all. This procedure I take to be typically Indian, and the reader can draw his own conclusion as to the ancient period. At the shop mentioned, I used to find an occasional copper coin of low denomination, of the rough hand cast type, but they all belonged to the Portuguese period, were not very old, were comparatively rare; no such silver coins turned up.

I am told on quite reliable authority.that even in so important a centre as Poona, cast silver coins of the Peshwa period were accepted in the marketplace at an exchange rate of their own, down to the eighteen nineties. I myself remember the cowrie shell in use as small change in Poona during the opening years of the first world war. In fact it was the pressure and the industrialisation of that war which ushered in a modern attitude towards currency, at least in the larger Indian cities.

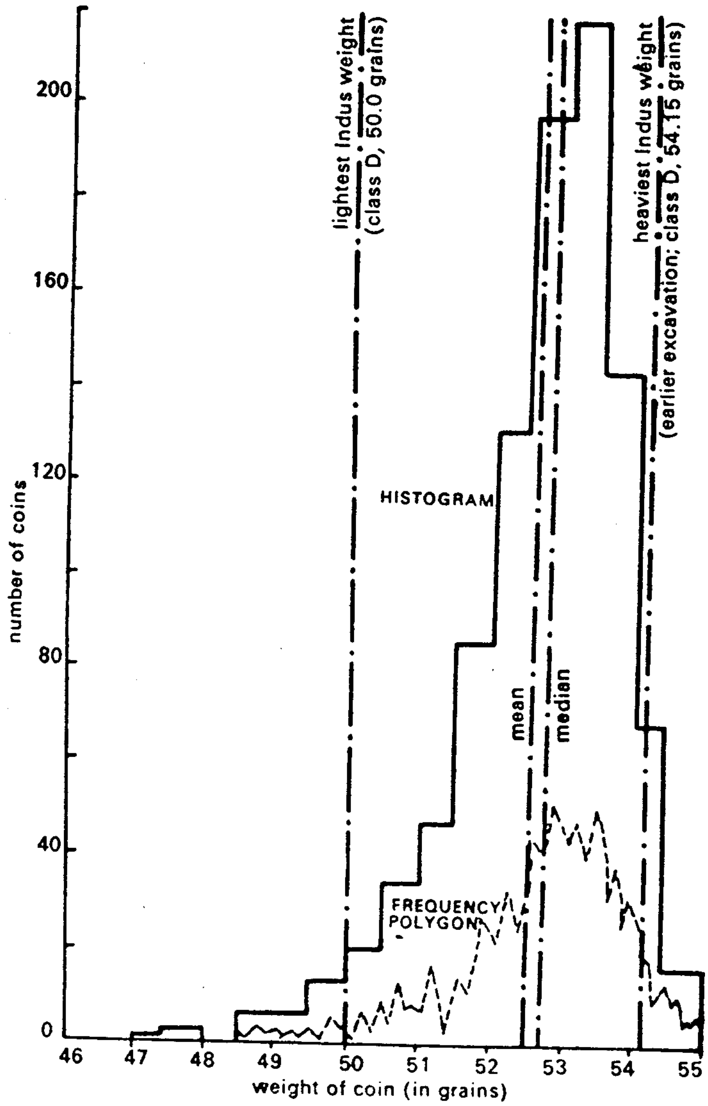


Fig. 4.2 Weight distribution of coins in the earlier Taxila hoard.

4 LESSER GROUPS : THE REVERSE MARKS

The distribution shown in Fig. 4.2 raises our first serious difficulty, that the coins are not normally distributed as regards weight. But the and the *z* tests apply only to normal distributions [but see 26 for the contrary], and a purist would at once raise a theoretical objection as to our conclusions. One way of settling this would be to work out theory of such tests for anormal distributions and then to show that in the present case (the distribution being platykurtic and skew-negative) no substantial difference will be made. But if this be possible at all, the gain in the way of new results is not likely to be commensurate with the labour involved. A simpler method would be to chop off the long tail of the histogram and frequency polygon in Fig. 4.2, as it is this that causes all the trouble here. This procedure is statistically unjustified, particularly as we do expect more in the range of overweight coins.

The third way out of the difficulty, whether it succeeds or not, is more reasonable and attractive: to analyse the structure of the group a little closer. We have put all sorts of coins together, without regarding the evidence of the classification by marks, and might have lumped together too many coins with a decidedly aberrant history. The numismatists' analyses of hoards I have seen are perfunctory, and lead to rather strange conclusions. Not the strangest is Allan's [2, p. *lvi]* that the similarity in the structure of the hoards '’suggests the period of the Maurya empire—which ruled all the regions mentioned and suddenly collapsed everywhere at the beginning of the second century B.C.—for the issue of these coins". This is definitely ruled out by the fact that our older hoard must have been closed at about the time Candragupta Maurya's accession; and I am inclined to take references in the *pali* canonical literature (not including the *Jataka* legends) as authentic mention of a system of coinage contemporary with or preceding the Buddha, say at least the sixth century B.C. Allan [2, p. *lvi]* thinks it "very possible that the idea of a coinage came to India in the late fifth or the early fourth century B.C. from Achaemenid territory, being suggested by the sigloi, although its character is entirely Indian". This smacks of prejudice, being just one step removed from the mind that sees everything of any value in India as having been introduced by the Greek conquests. As a matter of fact, Darius I ruled, in 522 B.C., a territory extending some inside the Indo-Afghan frontier *(kambujiya, gandara, Hindus* in the Naks-e-Rustum inscription), but that would not'account for coins at the time of the Buddha in U. P. and Bihar, unless the idea caught on with alarming rapidity. As a matter of fact, coinage appears in India and China at about the eighth century B.C., and allowing for the influence of trade, it is not clear why it should not be put at that date in India, for the country was certainly not isolated in those day .

Something could be done with a chart of find spots, but not in the accepted dilettanish manner. If the find spots are accurately marked with groups, and the *numbers* counted instead of just the occurrence of *a* single coin of the type, we could make better conjectures. Age and distance might be shown by loss *of average* weight, and the numbers or at least proportion would increase as one approached the locality of issue. For this, however, will be needed not only better grouping of information but also far more information from new excavations and more thorough-going surface collections Cunningham's genial and well-meant but very destructive methods are to be deplored in this connection. In any case, for the hoards under consideration, we can hardly use any such method, though it would have been of value to know the stratification of the coins in at least the older Taxila hoard. Therefore, there is nothing left but to classify by the marks on the coins themselves, a procedure that would have been followed without the sligh est hesitation, as the most natural, had there been some clear knowledge as to the meaning of the marks.

The reverse marks are far more in number, and lighter in stamp, as well as of smaller size. Occasionally, a reverse mark appears on the obverse, but this is rare enough to be written off as an accident. Walsh concludes, [3, p. 25-7] following the practice of 'Native States' until modern times, that these might be the marks of money changers or marks put on by the state itself after testing. It is (roughly) obvious that the number of marks increases with age, and the weights decrease correspondingly, as 3, Appendix XI shows, the coins there being arranged and numbered approximately in the order of increasing number of reverse marks. The hypothesis is then worth testing that there is some relation between the number of the reverse marks and the drop in weight, i.e., increasing wear. For this purpose, I retabulated the coins by number of reverse marks alone, neglecting the difference in the marks and in the obverse marks as well.

This gave the usual trouble to be found in trying to get information from Walsh's work [4]. Taking App. XI to be the source of information, we occasionally find some coins mislabelled, even without the possibility of reverse marks on the obverse: no. 320 is given simultaneously as blank, with one indistinct mark; no. 474 has 2 against it in the column headed number of reverse marks, but only one mark, no. 111, is given in the adjoining column; similar contradictions arise with coins 526, 599, 661, 749, 865 (a double obverse) 1115, 1124, i iso, 1149, 1150, and a few others. I have tried to settle the discrepancy in each case by reference to the plates, and have taken the rest of Walsh'i statements as authoritative; but his work oug^t to b: thoroughly recast by some competent numismatist.

On my tabulation, then, I find the following results, keeping the square and the round coins separate:

The accompanying figures, Figs. 4.3 and 4.4, make the structure of the hoard, analysed by the number of reverse marks, clear. The suspect coins are those that differ considerably from the rest of the group.

TABLE 4.2 CLASSIFICATION BY NUMBER OF REVERSE MARKS

No. rev. marks	*square coins*				*round coins*			
	n	*m*	σ^2	*no. of suspect coins*	*n*	*m*	σ^2	*no. of suspect coins*
1	2	3	4	5	6	7	8	9
0	224	53.26	1.1043	...	58	53.35	.6618	
1	128	52.93	0.5953	...	34	52.84	.7515	
2	131	52.78	0.7395	558	29	52.75	.5926	
	132	52.74	0.9370					
3	85	52.47	0.6673	...	27	52.21	1.3623	1075
					28	51.90	3.8967	
4	64	52.53	0.7092	...	25	52.29	1.6399	
5	46	52.17	1.575	...	10	51.67	1.2534	
6	21	52.03	1.1973	...	13	51.82	0.5947	
7	25	51.67	1.8364	...	8	52.23	1.6536	
8	10	51.4	1.6556	...	9	51.23	1.0550	
9	9	51.47	1.0725	...	3	50.1	0.7200	
10	8	51.01	0.5670	890	3	51.20	0.21	
	9	50.17	6.1644					
11	5	51.16	0.188	...	1	49.9	..	
12	4	51.25	1.2633	...	3	50.37	1.4034	
13	2	50.9	0.18	...	1	54.4	...	
14	1	50.2	...	...	1	49.5	...	
15	0	...	...	...	1	49.0	...	
16	3	48.93	10:6	...	0	...	...	

But they make very little difference in the means. In any case, the data is hardly worth using after 10 reverse marks, simply because the number of coins in each class thereafter is too small to give reliable averages and variances. So, the regressions calculated in Figs. 4.3 and 4.4 to fit the data observed were computed from the first 11 rows, only.

The first point that strikes us [5] and is quite clearly illustrated by Fig. 4.4 is the remarkably steady drop in average weight particularly for the square coins. The differences of means in the classes are hardly significant for any neighbouring pair, but what is significant is the relative steadiness of the drop. The curve that follows this best— technically the line of regression—can be fitted accurately enough by eye, and the equation is given by 7-53.22-0.212 *x*. where *y* is the average weight in grains and -v the number of reverse marks. The best explanation of this phenomenon would be that the marks were not put on haphazard, but at regular time intervals. It is known, in fitting such lines of regression, that the possible errors in *y* would not make much difference, if only they balanced out on the whole, as is to be ecpected. But any error or omission in *x* (the number of reverse marks) or in the regularity with which the reverse marks were put on, would be serious, and would affect the line of regression much more, even to the extent of destroying its linearity altogether [13, p. 135]. This inclines me to the opinion that the reverse marks were periodic, and regularly placed in time. The departure from the straight line, in the range x = 0 to 10, is not serious [13, pp. 261-263] as compared to the sampling errors. The only awkwardness is in the fact that the variances themselves do not increase steadily with *x,* but this is to be explained by sampling errors, the presence of suspect coins, and for the blank reverse class by the fact that the (almost unused) class contains many distinct issues, which we shall look into later, that had not had time to get worn down to a common level by circulation. From the fact that an occasional coin with blank reverse occurs in the oldest groups, it is clear that the system of reverse marks applied only to coins in active circulation, and perhaps in a limited region.

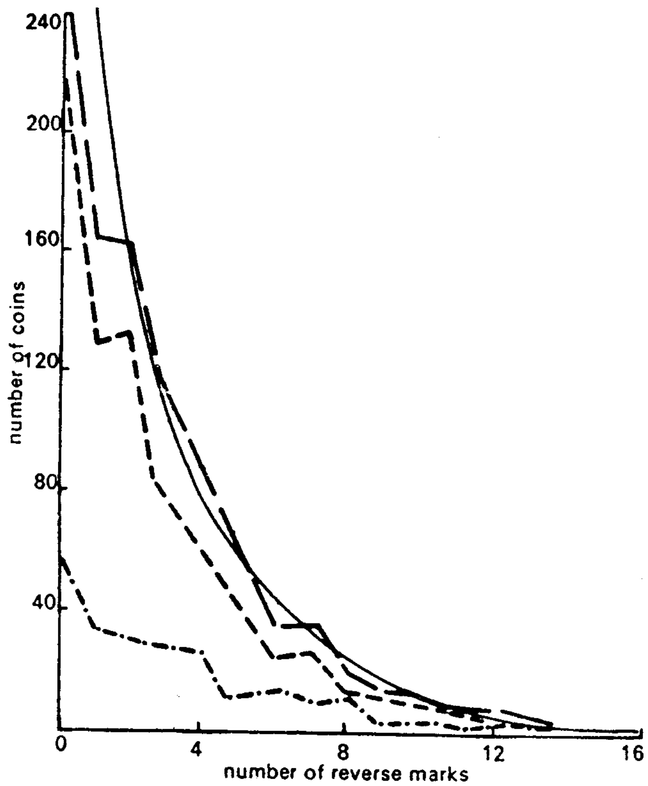


Fig. 4.3 Number of coins in groups by reverse marks, showing absorption of coins. The solid curve is theoretical; the broken curve overlapping with it represents all coins. The other two curves indicate square and round coins, respectively.

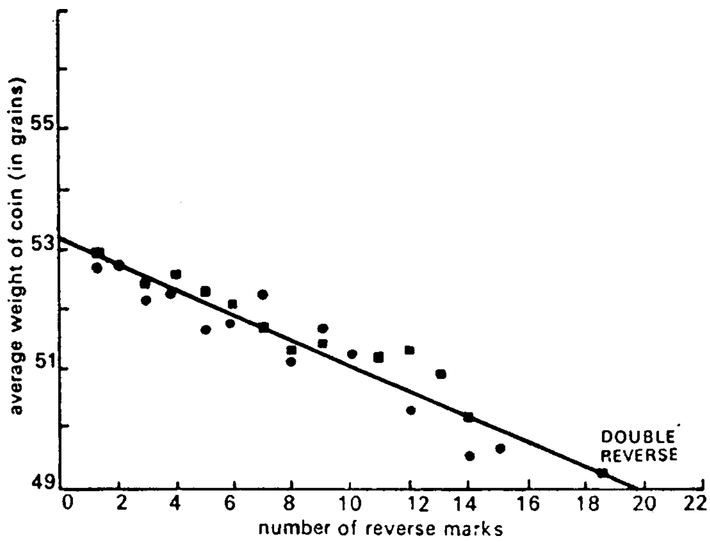


Fig. 4.4
Mean weight of coin groups by reverse marks.

A further proof, in so far as statistics can furnish one, is to be had by considering the numbers uf the coins in each group. These decrease,as is seen in Fig. 4.3, in a fairly steady manner, taking the natural logarithms of the numbers of all the coins (it is clear that on the whole, there is no essential difference between round and square, for any *x,* as regards mean weight) in each class, we can obtain the formula for the number y as $y = 283.86\, e^{x13}$. This is a just tolerable fit, and indicates or. :h..t the whole, a constant proportion of the coinage was absorbed during every interval between reverse m-irks; a proportion between 7/10 and 3/4 of all the coins surviving at the time of the next check by marks. Had the marks been put on by money-changers whenever a strange coin appeared and pass through their hand (made an unlikely hypothesis *a priori* by the fact that the same mark can appear twice) we should have had a random distribution of the marksj and expected a Poisson distribution [13, p. 56 *et. seq.]* to fit. But this is not at all even a possible fit, and the conclusion I have*given above is still further strengthened: the reverse marks indicate a system of regular checks on the coinage. The disappearance of the coins would be due to the fact that the coins might be used as a source of metal by the general population; to hoarding, loss, damage; also to the export of currency. Lastly, the proportion of round and square coins in any one category is about the same, which might indicate that the round coins were made by gathering the scraps left after the square coins had been cut out of a plate, and melting them down into pellets (or a cylinder). There is every danger here of guessing too much, but it is usually accepted that the square and the round coins were respectively cut out of plates and punched on a ready-cast piece of metal, the latter showing no signs of trimming as do the former. As a result, the line of regression fits over square coins much better than the round. For the rest, at a first glance, it is clear that the square coins are not square, and the round coins not round!

It is to be noted that these remarks and statistical findings apply only to the earlier Taxila hoard. For the later hoard, and the coins I ascribe for the greater part to the Mauryan age, the method cannot be used. The reason is that we do not get so many reverse marks in the later period. They had become an extension of the obverse, a sort of head-and-tails affair; their regularity and comparative lack of variety attest this. They might have beer, the marks of local satraps, or other issuing authority. That is, the bare difference of 70 years [between Philip Arrhidaios and Diodots: cf. 3, p. 1] marks an enormous change in the fundamentals of the coinage system, keeping in mind the cruder technique and the greater weight variance. With the greater stability to be expected from a universal monarchy, we have a decaying system of striking the coins; perhaps, because the stage was set for casting coins, though this would seem a lame explanation.

The question now arises, who did the checking, and far more important, at what intervals of time ? Where was it done ? Taking into account the coins with double obverse, we can say that the hoard contains coins of approximately 19 or 20 intervals earlier. There is no way of determining the rate of wear. The coins would have been of varying alloys (even from the natural incidence of other metals in native silver; whence my contention that every coin should be analysed), though reasonably uniform in the earlier period. The circulation would be very much less than now, but if the touchstone were used *[A,* II, 13, 31; 9, p. 124] the coin might suffer more. For modern Indian currency, i.e., the British rupee, the rate of loss is not more than one grain per sixteen years. The surest method would be to analyse weights of similar coins found in some other hoard of different but known date, and compare the losses in weight. The trouble here is that dated hoards cannot be had to order. I only point out that inthe *Arthasastra,* there appears to be mentioned an official whose business it is to check the currency. What happened in the older, more accurate, and relatively stable period, can only be a matter for conjecture. That the period was — whatever its duration — relatively stable can be seen from the fact that the currency was being obtained and lost by Taxilans at a more or less constant rate, as is shown by Fig. 4.3. In the time of the Buddha, according to sources like the *Anguttaranikaya,* we can see a lot of petty warring kingdoms eternally quarrelling with each other, and a movement towards the formation of larger states, say of the later 'universal monarchy', first realized for the eastern end of the Gangatic plain by *Ajatasatru.* Even in warring states, a comparative stability can be built up, if according to the immemorial Indian custom, the general population were quite indifferent to the strife of small princely armies, the trade of weapons being the monopoly of the *ksatriya* caste.

To revert to the *Arthasastra,* we find an official mentioned in several places, who might have done the checking (in spite of the lack of reverse marks of the older type on later coins) : the *rupadarsaka.* The most relevant passage runs as follows [A. u, 12, 30; 9, p. 120]:

रूपदर्शकः पणयात्रां व्यावहारिकीं कोशप्रवेश्यां च स्थापयेत्—रूपिकमष्टकं शतं; पञ्चकं शतं व्याजीं; पारीक्षिकमष्टभागिकं शतं; पञ्चविंशतिपणमत्ययं चान्यत्र कर्तृक्रेतृविक्रेतृपरीक्षितृभ्यः

As Meyer reads and Shama Sastri punctuates it, the taxes are clear enough: the 8 per cent *rupika,* special or individual tax (I should have translated it currency tax, but Meyer shows that *rupika* is also applied to a salt tax, and it is not likely that salt was a form of currency) ; five per cent unfair profits tax *(vyaji),* testing fee of 1/8 per cent and a net penalty of 25 *pana.* This last is to be remitted for those who made, bought, sold, or examined coins; I take it that the last class, the coin dealers, were not government officials, but a class sanctioned by the state with some sort of inspection to see that they kept a legal currency in circulation; their presence might account for the pejoration of coinage in the Maurya period. The *rupadarsaka* is to establish or adjust the *panayatra,* or circulation of currency [Meyer: *Geldkurs],* and his speculations are carefully regulated in a later chapter [Meyer, 9, p. 319]. The whole question so far as we are concerned is: does the expression *panayatram vyavaharikimn kosapravesyam ca sthapayet* indicate that he was to stamp any reverse marks on the coin, in token of having assessed the taxes, or checked the coinage for fair weight? There seems to be nothing to indicate this, although the officials of the book have to pay, in general, special attention to the seal for octroi or customs *[A.* n, 21, 39] pass for breaking the curfew order [A n, 36) 56], and permits of all sorts. The old system of many small reverse marks vanishes for the Mauryan period. One would expect that the *rupadarsaka* would have some method of showing whether a coin had been examined or not by him. Beyond this I cannot go here, though it is conceivable that the functions of a *rupadarsaka* as distinct from the unofficial examiner of coinage *(pariksit)* might be traditional, and affect the period of the older Taxila hoard. We note in passing that Bhandarkar, interprets [6, pp. 157-158] the three taxes as levied on the four classes of dealers in coins, a rather fanciful interpretation of a passage that is not intrinsically very clear.

The tax that does not explain itself is the vyaji, which is defined elsewhere as the royal levy upon the profits made by the trader by unfair means: short weights and measures, price-fixing and raising, etc. How this could be made out of currency is not at all clear, unless coin-clipping is meant; and as this was forbidden by law, and inhibited by the *rupadarsaka,* the one chance of increasing a hoard of cash would be by charging interest. Here I am slipping further into the realm of pure conjecture, but unless interest be regarded as one of the unfair practices, it is difficult to assign an etymology to the modern vernacular term for interest: *vyaja,* which is the *samskrta* for cheating.

'Interest' in the classical language is *vrddhi = growth,* from which the modern word cannot be derived; in fact, the Hindi dictionary prepared under the auspices of the Nagari Pracarini Sabha derives it from vyaja. It is unfortunate that the *Arthasastra is* not critically dated, and that we have no proper manual of an older age. But the merchant *(vaisya)* comes only above the *sudra* in the caste system, and if the taxes are an indication, he had not a very happy time of it under the *Arthasastra* code, whatever might have been the value of an empire in maintaining law and order. His status in the times of Buddha seems to have been somewhat higher, and if this speculation has any real basis, it might also help account for the crudities of the Mauryan period, as compared with the earlier Taxila hoard epoch, which has, at least in its weight system, clear survivals from an ancient and predominantly trading age; an age when the *ksatriya,* backed by the theoretical support of the *brahmana* and his monopoly of the art of war, had not as yet imposed himself upon the means of production of the country; at least, not to the extent of regulating the currency.

What was the period of the assessment? It could hardly have been one year, unless there was a veritable horde of tax-gatherers in those slow-moving days. The longest unit of time mentioned in the *Arthasastra* is the *yuga,* the lustrum of five years [9, pp. 165, 168]. Even this seems rather short for the examination and taxation. I should have thought that the Roman indication of fifteen years would have been a fairer period, whatever the Maurya empire and the Taxilans actually practised. Perhaps, the twelve year cycle was used. With the smaller period, our coin-checking system would go back not more than a hundred years, say to 417 B.C. For the twelve year cycle, we should get something like 500 B.C. for the beginning of the systematic checking of coins. I am urtable to account for the tremendous number (nearly 400) of the older reverse marks, and the precise nature, purpose, and operation of the system is still a puzzle which we cannot discuss here. It seems to me less likely that all coins were checked every so many years than that a coin checked once was again checked after the lapse of the set period. Milne [22] thinks all the reverse marks on the Persian sigloi, (although many of them occur in the Taxilan reverse marks) due to Levantine traders.

It should once again be made clear that the drop of 0.2 grains per indiction (I use the term, without specifying its measure, for the period of checking or stamping on the reverse marks) would be too small to be detected in the old days. There is considerable overlapping in the distributions of weights. But there is every likelihood of the worst coins having been withdrawn at the time of the indiction.

Mr. T. Streenivas has given the description of silver punchmarked coins found in the Karimnagar district of Hyderabad state [23]. It might have been possible to determine the average loss of weight from this data, had the grouping of the coins been in conformity with that of Allan or Walsh, and had some effort been made to date the hoard. The weights given [23, pp. 43-66) are rounded off to the nearest grain, which would not make it impossible to calculate fairly reliable statistics, but some of the coins are described as ‘encrusted’, and there is no analysis of the provenance; the description of the marks is perfunctory. Mr. Streenivas uses Cunningham’s nonexistent average of 58.56 grains, and g.ves an undocumented and unproved estimate of the loss of I i gr. per century. I am unable to see how he terminates the period of circulation of the coins at “about 150 A.C.” [23, p. 43]. But his estimate of the loss, if it applied to our earlier Taxila hoard, would give the indiction as between 12 and 15 years. Without any evidence, I must confess to a predilection for the 12 year indiction.

5
classification by obverse marks: indus weights

There still remains the classification by obverse marks, and the hoard can be made to give a little more information this way. I accept Walsh's classification of the earlier hoard, but the list given in Table D [3, pp. 50-71] is worthless except as a rough guide to Appendix XI. I have had to take Appendix XI again as the final authority, and retabulate the omissions and misclassifications of the lists copied first from Table D. From the classification of the last section, it is possible to conjecture too much: that if the abnormalities in the number of coins as plotted in Fig. 4.3 are significant, then an unusually large number of coins reached Taxila at periods of 2-3 and 7-8 indictions before the hoard.

Walsh's classes *A.1, C.1, D.2, D.3,* are prominent in the table, the rest being represented by comparatively few specimens. Amalgamating the data for round and square coins, *A.* i is found to contain 207 coins, the distributions being (by number of reverse marks, starting with blank coins), 29, 45, 39, 33, 23, 16, 7, 9, 2, 2, 2. That is, these had been checked at Taxila over a long period, and were the commonest currency of the region. Now *C.1* has, according to Walsh's only 70 coins, the distribution by reverse marks being 14, ii,. 16, 5, 12, 5, 3, 2, 2. Class *D.2* has 88 coins, distributed as 63, 16, 9. These are from Walsh's Table D, uncorrected. Keeping in mind the fact that coins not issued by the rulers of the territory would be quite legal, it would seemt hat the *A.1* currency was in general use, but that its day had already begun to pass; that *C.1* was also currency of trade but less common. Both of these were more distant in time—and therefore, possibly in space—than *D.2 (D.3* has much the same characteristics), which seems to be fresh and perhaps a local issue. I should like to go deeper into this", but not on data as printed in Walsh's memoir. I should have taken the *Sadaracakra* as the first criterion, whereas his classes *A.1-A.* 34 contain several forms of this, *C.1* has the same *cakra* but the other marks differ. Finally, *D.I,* 2, 3, 4, are given with a different royal symbol, this makes me so bold as to conjecture that the difference in structure of *A. 1* and C. 1 is less significant than with D.2, which is really a different issue, indicating, perhaps, a new dynasty, or a change of government.

The complete analysis of a single type of currency found in a hoard like the older Taxila hoard would be of the utmost interest. But for the present, I shall have to abandon it, because it would mean a careful reclassification checking of data, particularly reweighing the coins and also alternative regrouping and recalculation, just to see which of several hypotheses fits best. All of these are beyond the scope of the present memoir, and as matters stand just now, beyond the means of the present writer. It would be, however, worthwhile to look closer at the coins that have, as far as possible, the same history. For this purpose, I select the following classes with blank reverse: *A.1, A.19,B.(e)2, C.1, D.2, D.*3; and several *A.1* with various reverse marks. In this I have had to examine Walsh's.table D more closely, and assign several coins to different groups, on the basis of the plates and his own classification as in Appendix XI, in particular, Nos. 237, 146, 247, 212, 648, 355. 370, 526, 607, 624, 636, 770, and a few others. It would have been more convenient to pool round and square coins, but as they have 'different histories' at least at the time of manufacture, they are kept separate in spite of the resulting smaller numbers and less conclusive statistics.

The complete table for class *A. 1* would be useless because the numbers are far too few, and the suspect coins therefore become of great importance. I have already given a few for the larger numbers in Table 4.2, and briefly explain the 'method' here, leaving the technical terms to be explained later. We calculate the mean *m* and the variance rho^2 for any sample of the coins, taking them as given in the data. Then we make the—*unproved*—assumption that the distribution should be normal, and that the variance estimated for the sample is close enough to the actual value for our purpose. Now it is known that standard deviation (square root of the variance) being J about 1/22 of the total number in a normally distributed lot should differ from the average by aor or more; about 1/370 exceed 3 rho, and 1/17000, 4 rho. If more than the proper number fall outside the ranges, particularly the 30 and 40 range, there is good ground for suspicion. We can then reject the suspected coin or coins, and recalculate the statistics. The mean will rarely differ by much, but the variance will usually be reduced in a marked fashion. The greater this reduction, the better the ground for rejection. On this basis, coins 1075 and 890 should certainly be rejected in Table 4.2 as the recalculated variance would make the adjusted group incompatible even with entirely independent groups having the given numbers and variances; the same can be said of coin 958 in Table 4.3, at a lower—5%—level (in all cases testing compatibility by the *z* test as for independent groups). That is, these coins have been treated in an entirely different manner from the rest of their group, and have a distinct individual history. I might add that the only way of testing a single coin for loss of weight can be by reference to its group. In particular, No. 890, weight 43-46 grains, has been reweighed (with four other suspects) for Dr. V.S. Sukthankar by the Curator of the Taxila Museum, and found to be correctly entered. It is the lightest coin of the hoard, and shows no sign of having been damaged in any way, hence its loss of weight must have occurred in antiquity. On the other hand, coin 212 of the blank *D.* 3 group, weighs 54.1 instead of 51.1 as entered in Walsh's tables, and though it was not a bad suspect, the mistake was discovered by the method outlined at the beginning of this paragraph; so, I recommend it to the attention of numismatists—in spite of the fact that it involves some circular logic in reducing a non-normal class to normality by brute force, and that it can easily be overdone. I have had to use it without reserve in one case, the analysis of Mohenjodaro weights.

TABLE 4.3 SELECTED GROUPS

	square				*round*			
group	*n*	*m*	σ^2	*numbers suspect*	*n*	*m*	σ^2	*numbers suspect*
B (*b*) 1.*D*.o.	13	49.0	2.6	...	9	49.6	0.785	
Un. 3 (blank)	5	53.2	0.615	...	...	...	...	
D.2 (blank)	51	53.83	0.1407	corrected	14	53.9	0.1754	
D.3 (blank)	21	53.7	0.1695	...	5	53.34	2.348	
					4	54.0	0.2267	958
C.1 (blank)	7	53.46	0.5229	...	6	52.93	0.5387	
B(*e*) 2 (blank)	13	54.03	0.1123	corrected	5	53.88	0.362	
					4	54.13	0.0825	933
A.19 (blank)	10	52.32	0.7907	...	6	52.78	0.2857	
A.1 (blank)	21	52.43	3.7986	114,269				
	19	52.95	0.5161	291,300	8	53.09	0.3384	
	17	53.18	0.2441	291,300				
A.1 (one rev. mark)	35	52.77	0.4552	381,435	10	52.9	0.14	
	33	52.87	0.3098					
A.1 (2 marks)	30	52.75	0.3957	...	9	52.58	0.8344	
					8	52.83	0.325	1045
A.1 (3 marks)	20	52.67	0.4845	...	13	52.06	0.7492	
					12	52.22	0.4761	1040
A.1 (4 marks)	17	52.53	0.75					
	16	52.75	0.392	636	6	52.67	0.5707	
A.1 (5 marks)	12	52.79	0.4917					
	11	52.96	0.1505	777	4	52.35	0.2567	

Ignoring Hemmy's 'theoretical' conclusions, and taking only the weights as actually found at Mohenjodaro and Harrappa that come near the *karsapana* weights I construct the following.

TABLE 4.4 CLASS *D* INDUS WEIGHTS (IN GRAMS)

3.03	3.313	3.38	3.405	3.43	3.48	3.554	3.780
3.12	3.329	3.381	3.414	3.43	3.484	3.556	3.90
3.24	3.343	3.39	3.418	3.44	3.49	3.604	3.93
3.24	3.362	3.39	3.422	3.44	3.51		3.96
3.30	3.367	3.394	3.424	3.465	3.520		

These weights were obtained from Hemmy's tables [7, pp591, 596-8, 25, pp. 602, 607, 677-8], but not without trouble. The figures to two places of decimals are from the earlier report [7], and the rest from the later one [25], which should have simplified checking, being given that the final table [25, pp. 676-8) is supposed to give value of all Indus weights found. Unfortunately, only two weights of 3.44 grams can be traced, namely DK 1428 and HR 2191 [7, p. 597] although three of these are given in succession in 25, p. 678. To make up for this, weight 3.367, numbered DK 4973 in 25, p. 607 is left out altogether in the final table of 25, p. 678. There are two weights of 3.24 gm. in both the earlier report and the final table, but one of them has clearly been counted as 2.24 in Table III of the first report [7, p. 591], to give a fictitious class *C,* which also appears in the later reports, always with the extraordinary table of 8/3 times the class *A* weight; in addition, class *C* is given in the same tables as with 2 weights, while in table 1 [7, p. 590], it is given as with 9 weights! I have accepted both the 3.24 gm. weights. Hemmy ultimately breaks off his class *D* at 3.24-3.780 gm. but I have had to take all weights in the 3 gm. class, and as there is a large gap above and below those I have chosen, these would be sufficient for the purpos: of analysis. It would have been helpful to know that processes had been used to clean the weights, (if they needed cleaning) and whether they are likely to have gained or lost by the long burial which has impregnated so many of the other finds at Mohenjodaro with salt and made them subjects to decay upon excavation.

In the reconstructed table of weights approximating to those of our coins, we notice some gaps; between 3.12 and 3.24, and after 3.604. The first two weights, and all the weights of the last column, are therefore suspect *a priori.* For ease of calculation, we round off the last place of decimals. Calculating the mean and variance, we find: m=3.45, rho'=.03728718 approximately, which gives rho =.1931 gm, and gives four weights, when we should not get more than two, in the class differing by more than 20 from the mean value. We can repeat the process, discarding the two worst, i.e., 3.93 and 3.96 or even the latter, and repeat the process. This procedure finally leads us to discard the first two and the last four weights, although Hemmy retains 3.780. The final result is n =31, m=3.417, rho^2 = 0.007353, rho = .08566 in gram units. As I have said before, one of the 3.24 weights is confused, and might be mis-entered; I am inclined to suspect 3.604 also, becauseit isgiven[25, p. 607; DK 7161] asbeingmade of paste, which would not seem so likely to remain unchanged as chert or other stone. But I must let that pass too.

In grain units, this is m = 52.73, rho^2=1.7511, approximately. We compare this with the data of Table 4.1 comparing with the 995 *karsapana,* we find t=.398, P>0.6, with the 162 later coins, *P>.9.* In neither case is the difference at all significant. That is, so far as the mean values go, both sets of coins could have been meant to be the same as the Indus class *D* weight. But *z.* test tells a different story, and we find the corresponding values of *z* as .03417 and .58795. The first is not significant, even on the 20 per cent level, i.e., there is more than one chance in 5 that the Mohenjodaro system persisted till the time of the first hoard; the second is significant even on the 0.1 per cent level. This means that there is every likelihood of the earlier Taxila hoard being weighed on much the same kind of balances and by much the same sort of weights, as at Mohenjodaro some two thousand and more years earlier; but there is about one chance in a thousand that the Mauryan hoard was so weighed, though its average weight is actually closer to my Indus average than for the earlier hoard. Whether due to the fact that we have a hoard of very poor workmanship, or more probably (recalling the Swincy collection analysed in section 3) because the Mauryan period developed rougher standards of accuracy, can be decided only after comparing the data for several other hoards. This information, obtained after comparing weights actually found in the Indus excavations with the Taxila find of coins seems to me more conclusive and useful (in spits of the curious story it tells of Mauryan crudeness) than Hemmy's results, that the theoretical weight of the *karsapana* of whatever period and locality, was about a fourth of another theoretical weight approximately four times as much, and that all the coins came from Asoka's mint!

Table 4.3 gives us little new information on averages, as the significantly low group is *B(b).* i which is a double obverse group, and expected to be well below the standard weight. If we retain No. 370, and test 13 coins of *B(e)z* against the ten blank coins of *A.19* we find t=6.4, which is significant. That is, the two sets did belong to different times, or systems of weighing, in all probability. We have tested the extremes, however, of the square blank coins of Table 4.3 and the explanation would be quite simple: the new coins would, being all manufactured at the same time and the entire sample weighed against the same weight give smaller variances—due to the errors of weighing alone and not to the fact that different weights were used for different coins. These variances are very small, and in fact not compatible with the variance of the entire 995 *karsapana,* for which many distinct varieties have been pooled. The *z* test alone applied to the two variances, *B(e)z* against *A.* 19, would show a significant difference at the one per cent level and almost at the 0.1 per cent, that is, there is less than one chance in a hundred and just about one in a thousand that the two lots were weighed according to the same scheme. It is to be noticed that the variances for any one group with blank reverses are remarkably small. A modern sample of 208 freshly minted rupees was tested at the Bombay Mint, and I find the variance to be about 0.163, the sample being significantly skew negative, though the kurtosis is trifling. Of course, the rupee weighs 180 grains as against the 52-54 grain weights of the *karsapana*; but it is clear nevertheless that the ancients did a pretty good job of their coinage, at least for the earlier Taxila hoard. Walsh [3, p. 32] takes the later coins as all new at the time of deposit in their hoard, which would show an astounding carelessness on the part of the Mauryan coiners or regulators of currency.

To verify the 54 theoretical conclusions by experiment, I weighed each specimen of a sample of 3000 current rupee coins taken out of circulation at random. The average weight was found to decrease with increasing length of circulation, with about the same regularity as found in our *square* coins. The average annual loss of weight is, from this relatively small sample, 0.06258 gr., which means, roughly, a grain in 16 years. The variance go up with age, but the samples of each issue are too small to make the estimate of any value. As for the rate of absorption, it could not be determined either by direct count of my individual issues, nor by the ampler figures of the Mints' special remittances. One reason is that the number of coins struck and put into circulation is not the same for.each issue. When the number in the sample was divided by the number in the issue, it became clear that the ratio was approximately constant for all issues since 1903 (Edward VII, George V). But for the earlier coins, (Empress Victoria) the exponential rate of decline was clearly visible. This means that the rupee was not taken by the public as a token coin in the earlier period, but used as a source of metal. For the earlier Taxila hoard, the conclusions are that the Taxilans received their coins at a remarkably steady rate, and that they were absorbed with great regularity. The balance of trade must have been in favour of Taxila, and the form of society comparatively stable over the best part of two centuries.

Just as a matter of curiosity, the rather arbitrary process of discarding coins which differ by too much from the rest of the group on the basis of the variance of the group itself, can be applied to the classes as given in table II. The process is not unambiguous, but a justification of sorts can be found in that the weight even for a single coin would tend to vary according to the normal distribution, if many distinct observations were made [15,174 *et* al.]; again, all the coins discarded are invariably underweight and many of them decidedly underweight; certainly, the ancients would have been able to say that each of the coins I discard varied from the rest in its group, though they would have been likely to discard a few more, which I retain on the grounds that I should apply only my own *rho*— criterion for rejection. The 'improved' table 4.2. now reads:

TABLE 4.5 ADJUSTED GROUPS BY REVERSE MARKS

x	0	1	2	3	4	5	6	7
n	275	161	159	111	88	53	34	31
m	53.39	52.93	52.79	52.44	52.51	52.23	51.95	52.02
σ^2	0.5497	0.5798	0.6677	0.7252	0.7952	0.8704	0.9533	1.10

The round and square coins have all been put together, and groups with eight or more reverse marks ignored only because the numbers are then too small. It will be seen that only eighteen out of 930 coins have been discarded, some of which have most probably been clipped in the good old days; and a couple might have been mis-weighed or entered with a misprint in Walsh's memoir. Yet with this trifling adjustment, we have the means generally going down; the variances now go up steadily, and even quite regularly, as expected. It is the occasional badly underweight coin that conceals the character of a group. In case the reader wishes to know of somewhat more impressive and decidedly more complicated methods of selection, he will find them in text books [[7, pp. 125-129], or *Biometrika* XXVIII, 1936, 308-320.

The real objection to discarding coins, or to any form of non-random selection—as for the Patraha hoard [33, pp. i, ii]—is that our tests are likely to be invalidated at the very outset. Statistics takes its data and hypotheses in the bulk. We test, by compatibility or otherwise, at any level of significance, the chance that two lots of coins should have been selected at random from a general 'population' of coins-whose weights were distributed according to the normal law. Insignificant difference or ratio means that all this is likely to be true; by a significant deviation, we mean that this is not likely to be true, to within the probability imposed, but in the latter case, we do not know what portion of the hypothesis is contradicted. For selected hoards, it is clear from the very outset that randomness has gone by the hoard. Nevertheless, it must be pointed out that out of the 3000 rupee coins I weighed, just ten were suspect by the method given, of these, eight were counterfeit, and two mint-defectives.

6 COMPARISON WITH OTHER FINDS

Before any general remarks can be made, it is necessary to see what other hoards can tell us. The information available can be put in another table:

TABLE 4.6 OTHER HOARDS

reference	*description*	*n* (*number*)	*m* (*average wt. in grains*)	σ^2 (*variance*)
22, 2–4	Milne's Sigloi	52	84.95	1.5504
22, 2–4	Milne's Group A	14	85.12	0.2843
22, 2–4	Milne's Group B	38	84.88	2.0211
21, 16–17	Golakhpur Find	10238*	51.54	3.7063
21, 471–482	Gorho Ghat Find	58	48.72	3.7316
		57	48.86	2.5995
23, 43–66	Hyderabad Museum	412*	46.21	6.4745
24, 301	Paila Find	436	40.86	1.9701
32, 159–164	Peshawar Find	61	47.4	7.89

Spooner's weights for the Peshawar find are very doubtful. Weights are given by Streenivas for the Hyderabad Museum coins, and Walsh for 436 of the 1014 [originally 1245, of 3, p. 7] Paila coins as rounded off to the nearest grain, without specification of the lowest weight distinguished. I have taken the weight given as the central weight for each class, and applied no correction of any sort.

The sigloi are most interesting for the problem of the long-bar coins of Table 4.1. Clearly, Milne's class A have a homogeneity absent from the rest, and the comparatively low variance shows that they are struck very accurately, and have all been used in the same way, i.e., are in all probability unused or less used than the coin's of *B*. Coins 21-32 of group *B* [22, p. 3] do form *a.* subgroup by themselves, but the variance is still large. A particularly interesting feature of these sigloi is the presence of small punchmarks [22, p. 5], which are reminiscent of the earlier Taxila reverse-marks, and make it likely that the coins, even if hoarded in India, had circulated in a portion of the Persian empire not far from Taxila. For all this however, the double-sigloi would differ significantly from our long-bar coins. If both sets of coins were minted by weighing against fixed weights, the variances would be about the same, and the Taxila long-bar coins incompatible by the z test with either of Milne's groups. If we make the unlikely hypothesis that the long-bar coins were weighed against any two sigloi chosen at random, the variances for long-bar coins should come out to be four times those given for the siglor, but in either case, the difference of means would be significant. That is, the long-bar coins are too heavy for the double-sigloi standard. Whether they are just a Souvenir of Persian influence or represent the *satamana* standard I cannot say: probably the former. The Paila coins form a distinct system by themselves, even in the way of fabric and punchmarks (the 4-spoke wheel and 4 marks, in place of the 5-spoke and 5 marks). The weights are certainly not of the *karsapana* standard. Walsh thought that his data "shows an actual standard of 42 grains" [24, p. 301], but as usual, it does nothing of the kind. If the find is to be taken as homogeneous, and Walsh's weights represent a fair sample, then the variances are larger than at Taxila, and the coins must have been somewhat more crudely trimmed—though far more accurately than in the Mauryan period—or have been used considerably. On the strength of the averages, the coins are a little heavier than 3/4 of the Taxila coins. They could, however, represent 24 to 30 *rattika* in weight, or any other nearby standard, if the *raktikas* were selected accordingly. There seems to be no approximating weight among those hitherto found at Mohenjodaro. The grouping unit of one grain is much too coarse for these coins [13, pp. 53, 79].

Of the remaining three finds, that of the Hyderabad museum contains, as nearly as can be ascertained from the meagre descriptions and unsatisfactory plates, coins of Walsh's group *A,* group *D,* and abo of the later period, characterized by the 'Taxila mark'. The variance is of the later period; but the mean is far too low for either issue of punchmarked coins. The conclusion is that the Hyderabad coins, found in Karimnagar district, circulated for a long time after punch-marked coins ceased to be issued and that the earlier coins did not all disappear during or even after the Mauryan period, but extended their domain of circulation quite independently of an extension of sovereignty. If the rate of loss of weight is to be taken as comparable with that at Taxila for the earlier hoard, the circulation continued for not less than 33 indictions after Mauryan coins with the Taxila mark began to be issued. There is no evidence whatsoever that they circulated from 650 B.C. as Strecnivas would have it.

The Golakhpur (Patna City) hoard is quite unsatisfactory. Walsh believes that it shows definite evidence of the weights of the coins having been brought up by pouring molten copper (or perhaps dipping them in it) over them: the baser metal has covered the punchmarks [21, p. 17]. If this be so, then the attempt was extraordinarily successful, because the average has come up very well, and allowing for the loss of weight by corrosion and subsequent cleaning by archaeologists one would be inclined to think that the make-weight system had been miraculously good. Even now, if we omit eight of the worst coins (in addition to those described as broken, with missing pieces), the variances come up to the earlier Taxila standard. But this sort of argument is spurious, because we know that in this case the loss of weight by cleaning off the verdigris amounted to something like 12 per cent, most of which might represent the metal, not dirt. The 'added' copper, however, must be due to decuperification, that is to the actual travel of the cupric portion of the original alloy to the surface of the coin, by electrochemical action of the surrounding medium. I am obliged for this information to Dr. S. Paramasivan, of the Government Museum, Madras, who supplies the reference to Fink and Eldridge, "The Restoration of Ancient Bronzes and other Alloys", First Report, 1925; the Metropolitan Museum of Art of New York. Dr. Paramasivan has found many such examples of decuperification in coins which he has examined himself. The coinage with this *sadaracakra* occurs in other hoards as well [33, class m], and my interpretation is that it became a subordinate dynasty during the earlier period. The coinage might be that of Anga.

The Gorho Ghat hoard has a higher mean than that of the Peshawar coins, and a lesser variance. Nevertheless, the variance is too great for the earlier and too little for the later Taxila hoard. As I interpret this, the Gorho Ghat coins are a worn mixture of older and Mauryan coins. Walsh's description of the hoard is not accurate enough but he gives six different forms of the *sadaracakra* in his plate of marks, and only one of these is the characteristic Mauryan form, i.b of the Taxila hoards. These coins appear to have circulated about 20 indictions.

The weights for the Peshawar coins, as given by Spooner, are most unsatisfactory. But taking each g_{ro}up as having been 'forced' or rounded off to the nearest *raktika* (=1.83 gr.) and taking the weight given as central, I get the mean and variance given. The variance is clearly incompatible with any but the later Taxila hoard. If the coins are a mixture, they must be predominantly of Mauryan fabric. This is substantiated by the form of the *sadaracakra,* as seen on the plates. A few of the coins (in particular plate *B.* No. 1 of 32) might belong to or have affinities with the class D of our earlier Taxila coins. The means show that the circulation lasted about 25 indictions after issue, so that the Gorho Ghat hoard must have been buried the earlier of the two.

The coinage *B(e}2* might seem to be the freshest number of the earlier Taxila hoard—all coins but one blank—and the question will then arise as to why this is not the immediate predecessor of the Mauryan empire. The numbers are too small for this coinage to have been general. I take Walsh's *sadaracakra i.u* to be the same as *Bhattacharjyas 2.0* of the Purnea hoard. In that case, it is clear that the dynasty (or king) survived in Mauryan times. It was prosperous enough to issue quite a few coins [33, pp. 55-63, class II, group XI, var. *b-h,* coins 1073-1252]. The coinage *B(e)2* of the first Taxila hoard is surely cl. II, gr. XI, Var. *F* of the Patraha find. The latter find can be arranged in a tentative order of date as varieties: *f,e,d,c,g,b,* and *h.* But the characteristic marks of the last two varieties are duplicated under the Mauryan *sadaracakra* and the crescent-on-arches mark, in varieties *a* and *i* of the group. On my present hypotheses, this signifies that the dynasty or the king arose and enjoyed independence just after the death of Mahapadma Nanda, and afterwards as acknowledged the suzerainty of some Mauryan emperor. The *sadaracakra* is almost the same as the Mauryan, with a *damaru* replacing one of the taurines. I do not know whether this indicates any close relationship between the two dynasties, either as to geographical, or tribal origin. One other bit of information we get is that the heaviest standard of weight for the earlier Taxila period could not have gone much over 54 grains, as *B(e}2* would have suffered the least by circulation. Finally, in the Purnea coins, we note that with the change from varieties *b* to *a* and *h* to *i,* i.e., with transfer to Mauryan hegemony, the variances jump up suddenly: from 2.9654 to 5.1612, and from 1.6485 to 3.9922, respectively.

Let us sum up the addition to our knowledge—or at least to our conjectural store—that can be made by statistics. I take it that the absence of the 'Candragupta' mark and the fresh coin of Arrhidaios found in the older hoard [3, p. i] date it approximately to a period just about the beginning of the Mauryan empire. The reign of Alexander's feeble-minded half brother was brief, nominal, and turbulent; for a coin of his to have reached Taxila in excellent condition under these circumstances implies rapid travel, Walsh's assignment of the approximate date 317 B.C. seems to me quite reasonable for the earlier hoard. The second hoard is similarly placed by a coin of Diodotos, at, say, 248 B.C. Both of these, therefore, provide very important starting points for a discussion such as the present. When other dated hoards are found, we shall be able to round out the conclusions.

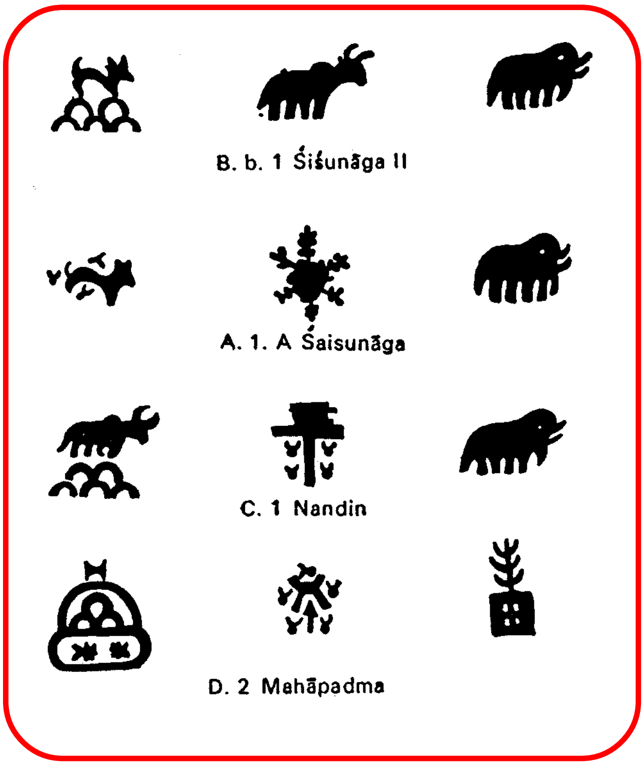
We see, first, that the system of Mohenjodaro weights was applied for the earlier hoard, but that in the Mauryan period, although the average remained the same, the variance increased enormously, showing a far cruder system. As I have said before, I am inclined to ascribe this to the rise of a new system of government, spread over large areas, and with a new type of ministry that handled the actual rule and took over many of the functions that must have been settled by common agreement by the traders of the older period. The system of reverse marks implies some sort of checking. Inasmuch as badly underweight coins occur in each group, this checking need not have been primarily for the purpose of weeding out light coins; more probably, the marks are a token of assessment. However, it is just possible that the occasional light coin lost its weight between checks. If the system were official, and fully developed at Pataliputra, it is difficult to explain why it disappears with the Mauryan period. But if the system were local to the Taxila region or restricted to an unofficial practice, this disappearance is natural, inasmuch as Mauryan conquest ruined Taxila as a great city. The presence of many reverse marks of the earlier Taxila hoard on Persian coins [22, p. 5] is natural if the marks were peculiar to a region or community which had trade in common with both Persia and India proper.

We know that for the earlier period, Taxila was either part of a Persian Satrapy, or in the Persian sphere or influence. Any coins issued there are likely—as in the case of the long-bar coins—to imitate a Persian standard or fabric. But the earlier hoard is in the main characteristically Indian, and identical coins are found as far to the east as Bihar, and to the south as Hyderabad. Therefore, it is likely that such coins were not manufactured at Taxila, but imported from the east, because of a favourable balance of trade. Silver in India, so far as we know, was imported from the *eastern* frontier, though only a thorough going analysis of all coins for minor impurities such as lead could settle this point, when coupled with a statistical analysis of the assay variations. All known records show that there existed a powerful succession of monarchies, a fully developed civilization, in the Gangetic basin; it extended over a territory from Pataliputra to Kausambi, and at least from the time of Buddha onwards. A logical inference would be that these monarchies would strike silver coins that would gravitate towards Taxila, and that the silver that reach" ' Taxila was much more likely than not to be in a minted form. Tluu, the obverse marks would *be* eastern in origin, while the reverse marks would presumably come into operation after the coins reached the Taxila region or the hands of trade-guilds, and remain in effect only so long[1]- as they circulated there. I am inclined to believe, without being able to give direct proof, that this regulation system was broken up with the formation of the Mauryan 'Universal Monarchy', and the Brahmanical regulations of the *Arthasastra.*

7 DYNASTIES AND KINGS

As I see it, the prime basis of the classification should be the individual form of the six-pointed symbol, the *gadaracakra.* Inasmuch as this has not been made the principal characteristic of the classes given in the sources I use, the discussion by classes must always occasion difficulties. However, Walsh's *A.i, C.i* and *D.z* are the most prominent classes in the earlier Taxila hoard, and analysis by reverse marks alone shows that *D.2* is the latest, *A.i, C.i* being older. This is, significantly, also the order of increasing average weight and decreasing variance for the *blank* coins of the three classes, as in Table 4.3. It is significant that whereas the earlier class has quite simple marks, i.e., sun *gadaracakra,* humped bull, elephant, hare (or puppy or some such animal) on five arches, the class *D* has marks of far greater complexity: a more complicated *sadaracakra,* a tree with railing, and two symbols which arc hard to describe (Walsh's 5 and 9 *c];* one of them contains a string of taurines *(ma* in Brahmi) which would give it a tantric or mantric character. Dating these classes by means of the reverse marks alone is impossible, and even their duration cannot be ascertained. For example, *A.1* has coins with as many as 10 reverse marks, and at first sight it might appear that it was issued over 10 indictions, this would mean an indiction of not much more than five years if a single king issued the coins, or that a dynasty issued coins with the same five marks over at least 10 longer indictions. But if the coins were issued in the east and reached Taxila only in the course of trade, as I believe to have been the case, then a single king may have issued them, even at one time, and they could have reached Taxila separately over a long period, a period that would not coincide with that of the king's reign or the issue of the coinage. But in any case, it would seem reasonably clear that *D.2* was newer than *A.1* and C.1.

If the puranic lists are comprehensive and cover the Taxila region as well (which is doubtful as they have primarily to do with the Gangetic plain), and in any case if the coins were issued in the east (which seems more reasonable because of their wide spread to the east and to the south), then an attempt to collate the puranic evidence



4.6 Tentative identifications of marks. The 'sun' symbol and the *sadaracakra* have been omitted. The latter is 1.0 for Mahapadma and 1.a for the other three.

as well as that of *Aryamanjusrimnlakalpa* with that of our coins would not be out of place. This is conjecture, not statistics, but after all a working hypothesis can always be produced, to be modified by newer evidence. It seems to me, then, that *A.1* is a coinage associated with some of the *Sisunaga* kings, and that *D.2* belongs to the coinage of Mahapadma Nanda, or some of the Nava Nandas. This can be argued out in some detail. Certainly, *D.2* cannot be associated with any *known* king except (Mahapadma) Nanda because it is immediately succeeded by and intermingled with Mauryan coins; this much cannot be contested, as far as I can see, on the evidence that we possess. If there were a larger and powerful empire between *D.2* and *A.1* and C.1, it would have interposed a group of coins of its own, of equal prominence. But there is no such group available, and it would thus seem highly probable that *A.1* belongs to the empire preceding that of the Nandas, i.e., to the Sisunaga coins. This last point I mean to argue a little more closely on the strength of the coins themselves.

It has been shown before that *A. 1* was a very large and general issue, but that its day was passing at the time of the earlier Taxila hoard (taking the hoard as fairly representative of the currency in circulation at that time, a plausible contention, as we have the minute coins and the long-bar coins included as well). The symbols are easy to interpret, except that of 'hare-hill' mark, or 'puppy-on-five-arches'. If we associate the coins with not too distant predecessors of (Mahapadma) Nanda, the mark, which occurs according to Walsh's count on no less than 485 coins of the hoard, (while the particular form of the *gadara-cakra* comes on 642 coins) must signify something important. But according to my interpretation of the three symbols (excluding the *Sadaracakra* and the 'sun'), this would be the mark of the founder of the dynasty in question. The *AMMK* says nothing on this point, but the puranas give the founder's name as Sisunaga. *Sisu* means child in Sanskrit, which is not admissible here, even if the elephant be taken as the equivalent of *naga*. But if *Sisu* is taken with one of its subsidiary meanings, i.e., the young of any animal, and the animal in question as a puppy or leveret, then the mark can represent the king Sisunaga. Alternatively, his name might have been falsely restored from the *prakrta* which was the original source of our puranic lists [27, pp. *x,xi,* 77-83], and could be read as *Sasa-naga* (a variant that never occurs in the purapic or any other list I have seen), which would give the animal as a hare. If it is necessary to take the arches as a component of the name, we shall have to take them as the equivalent of naka=heaven, or the firmament. This does not conflict with my conjecture that makes the arches a symbol of 'descent from' the totem symbolized on top. For the rest, while we are exploring possibilities for the dynast's name *sasanaka* and even *Sasanka* cannot be excluded. We have a foreign king of Egypt represented by the consonants, *s-s-n-k* whose Greek equivalent Eeowyx's would make his actual name something like sasanka this is not to be taken to mean that sisunaga or whatever his name was ruled Egypt as well, but that the name occurs among Aryan chiefs over a wide interval of time and space. I take, provisionally, my own reading of the 'hare-hill' mark as 'descendant of sisu'.

The humped bull is surely to be taken as *nandi,* and we find two puranic kings with this as a portion of their names: Mahanandl and Nandivardhana [27, p. 22], at the end of the dynasty in question. But as I take *C.i* as a latter successor to *A.I,* and *C.i* has the same Sadaracakra, with a *nandi* on five arches, it would seem likely that the kings at the end of the Sisunaga line claimed descent from a Nandi or Nanda, and not from the remote ancestor. This would imply a break in the direct line, but not a conquest nor an entirely distinct family. Moreover, the Buddhist tantradoes not give the same list as that of the puranas, which give the succession as Darsaka, Udayi, Nandivardhanan, Mahanandi, with reigns of 25, 33, 40, 43 years: rather too long, I think, to be probable. The *Aryamanjusrimnlakalpa* seems to make Udayi the successor of Ajatasatru, and then takes up other tales, to come back to the time and territory under consideration with entirely different king-names from those given in the *puranas:* Asokamukhya, Visoka, Surasena. I do not see how Jayaswal's identifications are to be justified, and I also think that there is at least the implication of an unfilled gap after Udayi. That is, the tail end of the Sisunaga line could very well contain more than two kings, and kings who would claim descent from Nandi, whether due to a break in the direct line after Udayi, or for some other reason. This must not be argued too closely.

I state again as a working hypothesis, the following: *A.1* is a saisu-naga coinage. But *C.1* and similar classes are of a latter king of related line who chose to claim descent from Nanda or Nandi. Finally D.2 is an entirely distinct reign, comparable in power with its two predecessors, without any other powerful intermediary. This, therefore, with other members of Class *D,* must be Mahapadma Nanda's coinage. The class *B, b.1* is older than any of these, having as many as 15 reverse marks, and occurring also on 22 double obverse coins.

The coinage *D.2* is obviously of a different character from its predecessors, in the complexity and mysterious nature of its symbols. In particular, the symbol 5 with its chain of five taurines and two extra marks seems mantric or tan trie in character; we actually know of a similar mantra: [28] Om , M, M, M, M, Hum, Nih King Mahapadma Nanda was, in fact, a different sort of king from his predecessors. He had been a wealthy minister who took over the throne (Tadvanam Prapya Mantri Sau Loke Parthivta Gatha 29, 434) He was low-born *nicamukhya* [28], and in fact supposed to have been a bastard of the last Nandi King [27, p. 25] according to the puranas which describe him as *Maha-nandisuta,* with the addition *sudrayamKalikamsajah* or *sadragarbhodbhava.* Like Vidudabha at the time of the Buddha, he wipes out the *ksatriya* members of the old dynasty, and apparently goes further in cleaning out all the *ksatriyas* of his time. Whether this was a form of revenge or for safety in the way of preventing a rival to his usurpation is not known, but he is definitely a land-mark in what survives of our historical tradition: the *puranas* state explicitly that the age of darkness (Kaliyuga) came to flower with him [27, pp. 74-5]. It is, therefore, to be expected that his coinage would be distinctive in appearance. Finally, he has a mantric connection (however late the tradition): he possesses a *mantra* of his own [29, p. 423]

तेनापि साधितो मन्त्र पिशाचो पीलुनामतः ।
तस्य मन्त्र प्रभावं तु महाभोगो भविष्यति ॥

which brought him his success. In addition, he has two Brahmin friends similarly equipped with *mantras:* his minister Vararuci [29, pp. 430, 433] and the great founder of samskrta grammar, Panini [29, pp. 437-438]. The absence of ancestors to display, and the connection with *mantra* as a means to success seem to be reflected in the coinage *D.* 2. The chronological evidence is not bad: the *Aryamanjusri-mulakalpa* makes Nanda (Mahapadma Nanda) the immediate predecessor of Candragupta Maurya, which agrees with coins bearing *D* marks on the obverse and Gandragupta's monogram as an issuing mark on the reverse. The puranic account (27, 25-6) gives him the improbable reign of 88 years, with 12 years for his eight sons, of whom only one is given by name. This seems very unlikely, and I explain it as a confusion *of nava* (Nanda) =new (Nanda) with *nava* (Nanda) = nine (Nandas). The Nanda or Nandi dynasty would be the *C.* i dynasty, (or king) and *nava* Nanda would be the *usurper* Nanda, our Mahapadma Nanda. The identification is generally confirmed by Plutarch's *Alexander,* which mentions a king who was 'hated and despised for the viciousness of his life and the meanness of his extraction'. But to trust Plutarch in detail would be folly; it seems quite unlikely that the boy Candragupta (Sandracottos or Andracottos) could have actually seen Alexander.

The Puranic chronology is confused enough to be typically Indian, but there is a significant mention of astronomical details at the end of the account [27, pp. 74-5] which should not be glossed over. From Parlksit to Mahapadma is given as 1050 or 1015 years, and from Mahapadma to the Satavahana king Pulomavi as 836 years. This is not borne out by the location in the 2700 year precessional cycle, which gives the pointers of the Great Bear as passing through the naksatra magha(No. 10) at the time of Parlksit, *purvasadha* (No. 20), at the time of Mahapadma, and in the 24th constellation at the time (termination *?)* of the 'Andhras'. As the time per *naksatra* is a hundred years in this cycle, we have about 1000 years from Parlksit to Mahapadma Nanda, and 400 from his day to that of the Andhras, which checks very well indeed with accepted historical data, with an allowance for the fact that the scheme of dating does not allow smaller units than a century. It is a remarkable conclusion that the data of Parlksit, and of the Mahabharata war would not go more than about a 1000 years before Mahapadma, say 1400 B.C., and that would be the beginning of the Kali yuga, not to be reconciled with the usual beginning at 3101 B.C., a time coinciding with the Mohenjodaro period. What confusion lies here is beyond the scope of the present memoir to discuss, but one is reminded of the Mesopotamia!! 'long-chronology' coexistent with a more plausible 'short chronology'. The '836 years' between Maha-padma and Pulomavl are probably to be explained by the supposition that the puranic redactor counted his *naksatras* from kritika and not from *asvinl,* and possibly, even with the extra naksatra, *abhijit,* thrown in for good measure. There are not wanting those patriotic critics [G. Bose on Andhra Chronology, *JRASB V]* who maintain that the figure of 836 years from a doubtful Nanda to an unidentifiable Pulomavi is exact and shows the general credibility of the *Puranas.* Whatever the value of this chronological particular, I do not find it possible to attach any great worth to an account which gives so little reliable information about the two most important dynasties of the

‘836’ years, being unable even to report the Mauryan names properly, and mentioning the Guptas as a local dynasty that rules along the Ganges, Saketa, Prayaga, Magadha [27, pp. 53, 73].

All the foregoing structure has been erected solely on the comparison of the purana tradition as collated by Pargiter, and the mutilated text of the *AMMK,* which existed during the 10th century A.D., but is surely late enough. The Pali tradition claims to be older, and is certainly a good deal simpler, without the mystical accretions and prophetic style. The period of our punchmarked coins, i.e., from the death of Buddha to the rise of Asoka Maurya, was of extreme importance for Buddhism, and it is not a matter for wonder that the Buddhist tradition should be somewhat more reliable. We find even an occasional pali line in the *Puranas* [27, p. 78] and the inclusion of Suddhodana, Siddhartha, Rahula in the Aiksvaku king list [27, p. n] is undoubtedly copied from a genealogical source used by pali texts [35, 2 v. 20.24]. Now the available pali texts give the following list, with reigns; Ajatasatru (24 after Buddha’s death); Udayibhaddaka 16, Anuruddha and the whole race is charged with parricide *(pitughata-kavamsoyam),* and an official *(amacca)* Susunaga is put in his place, who rules 18 years. Then we have his son Kalasoka 28, his sons, ten brothers *(dasabhatukukaraja.no)* 22, and then nine Nandas, supposedly brothers too, for 22; the last of these, Dhanananda, being followed by Gandragupta Maurya. The minister Canakya is also mentioned [34. 35l.

This helps a good deal. The direct line of Ajatasatru came to an end with Nagadassaka, the third after Udayi. One Susunaga was then put on the throne. This probably indicates a member of another branch of the royal family, possibly a descendant of Sisunaga I; at any rate one who could have the hare-mark for his coinage. The time of Kalasoka is given by the tradition that the tenth year of his reign was the hundredth after the death of Buddha. The large number of variable ‘fifth marks’ on coinage of class *A* is perhaps to be explained by the ‘ten brothers’ who ruled after Kalasoka. Now the chronology, though reasonable for the length of each reign, is twenty-five to a hundred years too short, if the ten brothers are at once succeeded by nine Nandas who rule 22 years to be followed immediately by Candra-gupta Maurya. The time of Kalasoka we may take as reasonably accurate, as the second Buddhist Council took place then, and formed a great event in the history of the religion; also, the lengths of the reigns from Ajatasatru add up well, though this may be just a matter of design. Hence, the gap comes at about the time of the’Nine Nandas’. My explanation of this, based on our coins, is that when the direct line of Sisunaga II came to an end, there was a peaceful succession of some other member of the same or related family. He continued the same *sadaracakra,* but claimed descent from a Nanda or Nandi and struck the coinage *C* (though Walsh’s *G.* 5, 2 coins, is excluded, as having a different *cakra),* while the preceding kings had the coinage *A* (excluding A.12, 4 coins: No. 205 of *A*.21; *A.21, A.25,* 24 coins). On my own hypothesis as to the meaning of the five obverse marks, there is no evidence for a Nanda dynasty, the coinage C with the bull-on-arches mark being then associated with just one king. The coins of Sisunaga II himself might be the class *B(b) i,* which is mostly restruck on older coins, and contains the hare, but *not on arches.* Coins *B(b)* i are at least 15 indictions old, as some of them contain that many reverse marks; *A* at least 10 indictions, and indicative of the rule of at least five monarchs. C is roughly 8 indictions in age. Allowing for a 12-15 Year indiction, and the time taken to reach Taxila, this checks approximately with our revised king lists. The term Nava Nanda is to be taken as *new* Nanda, and refers to the coinage D.i-D.4, which is so fresh as to require very few reverse marks, as we have here only one coin each with 3 and with 4 marks. This is the coinage just before Candragupta.

The king-names I give are conjectural, because literary evidence is unsatisfactory, and shows at any rate that each king had more than one name. For example, the Chinese translation of the *Samantapasa-dika* dated A.D. 488, has the reading Hsiu-hsiu-fo-na-ko, with a variant Hsiu-fo-na-ko, which would be equivalent to Susubhanaga or Subha-naga. The puranic variants are numerous enough, as can be seen from Pargiter's text. But I feel that on the strength of the archaeological evidence that we possess we can say—by an application of quite elementary statistics—that the principal coinages were, in chronological order: *B(b}i, A,C,D,* Mauryas. *B(b)i* not only precedes *A* but is re-struck on coins of an older issue. But these double obverse coins are as a group not less than 18-20 indictions old, say the time of Ajatasatru and his descendants. And to my mind, the second obverse does not indicate that the coins had to be re-struck because of wear but that a dynasty was superseded, and coins in the treasury re-issued. A parallel would be coins of class *D.2* with Mauryan reverse jaarks or coins of Nahapana counter-struck by Satakarni.

I have made little use of the Jain material, which is, however, accessible in a rather uncritical encyclopaedia, the *Abhidhanarajendra...* There, Bimbisara is called Seniya; Ajatasatru, Kuniya, Kunika, Konika. The nine Nandas are mentioned as beginning with the successor to Udayi, about 50-60 years after Mahavjra and displaced by Ganakya-Candragupta. This is not very helpful, and Jayaswal's reading Ajatasatru and Kunika *Sevasinaga* on a Mathura statue [21, pp. 550-551] makes matters decidedly worse. Yet it is not impossible to get some palatable conclusions by a careful and reasoned collation of extant records. For example, the puranic list of Sisunaga kings ends:

Ityete bhavitaro vai saisunaga nrpa dasa satani trini varsani
sasti varsadhikani tu Sisunaga bhavisyanti rajanah ksatra-bandhavah
[27, p. 22]

Pargiter translates [27, p. 69] *ksatra-bandhavah* as 'with ksatriya kinsfolk'. But surely, there is a chance here of confusion with the 'ten brothers' who end the direct line of the king I call Sisunaga II; also, his displacing a predecessor (Nagadassaka) might explain why the last king of the Pradyota line, displaced (according to the puranas) by Sisunaga I, is called Nandivardhana, a name that occurs again in the Sisunaga list and perhaps corresponds to the king displaced by Sisu-naga II. For the rest, there is no evidence that Bimbisara and his line were ever called Saisunaga, except of course the puranic list—which might have been miscopied as explained. So, 'Sisunaga I' might never have existed; I have let him stay, solely on the evidence of the puranic list, as the original founder of the line of Ajatasatru.

The later Buddhist records separate Nanda from his son and *successor* Mahapadma [36, p. 55]. Bu-ston's king Nandin [30, p. 106] who comes 108 years before Candragupta could hardly be Mahapadma. And there is no Mahayana tradition of nine Nandas. But Taranatha puts Candragupta just after Mahapadma [36, p. 58], and this must be the Nanda of the *AMMK;* a secondary confirmation is that the *AMMK* gives a description of the social evils of its Nanda's reign and this coincides very well with the puranic tradition that the Kaliyuga came to flower with Mahapadma. We can proceed in this vein forever. The Dhanananda whom Canakya polishes off as the last of the Nava Nandas is capable of explanation; a wealthy Nanda. In fact the *AMMK* gives such reference [29, p. 424 as preceding; pp. 426-427]; tesam dasyati toddhanam, etc. It is unfortunate that the Chinese translation of the *AMMK* which dates from the tenth century, with two chapters from the eighth, should not contain the historical (prophetic) portion at all. Taranatha mentions Nanda as the possessor of the

Pisaca-Pilu mantra [36, p. 53], and the friend of Paninl. But Vararuci is the minister of Mahapadma [36, p. 55]. With such sources it is impossible to identify the great king (or kings) who struck *A.I.* with its fifteen distinct issues and vast number of specimens.

For the Mauryan period, arrangement and identification are much less easy, although the literary evidence is far more satisfactory. The second Taxila hoard is worthless because the marks are almost unidentifiable. For the Purnea coins [33] the surrounding medium has caused a lot of damage, and there is no information available as to the extent of the decuperification. Moreover, the hoard is mixed, coins of the earlier period also being present (A1=Class III, gr. Ill, var. *b; C.1* =cl. III, gr. II, var. *b;* D=cl. II, gr. XII, var. *a,b,c;* B.b.1=cl. III, gr. VII, var. *a.,* etc.). Now the increase of variance between two comparable groups might be due to greater age, or the very reverse, to the sudden change from the older accuracy to Mauryan crudeness. The effect of age in depressing the average weight might also be reversed if Bhattacharyya has removed more copper from the later than from the earlier coins. However, on the dangerous assumption that there would have been no substantial change of proportion among the coins found even if the entire hoard had been published, one can guess something from the numbers of the coins alone. That is, the longest and most prosperous reign should have the greatest number of coins, and also the greatest number of varieties of the fifth mark. This description fits the coins of the Purnea coins Class II, gr. Ill, var. *c,* gr. IV var. *a-k.* Therefore, these coins must be the coins of Asoka, and the 'caduceus' [33, plate III, mark 86, perhaps 87 also] must be his personal mark. With similar arguments, and a little support from the means and variances, I conclude that Bhattacharyya's class II, gr.I, var. *a,b,* are coins of Bindusara, and gr. II, var. *a,b,* the coins of Candra-gupta himself. The last contains the 'peacock-on-arches' mark, as well as the crescent-on-arches whereas the rest only contain the crescent-on-arches. Moreover, the fifth mark on Candragupta's coins is [33, plates 3, 104, 105] the one obverse mark that stands out among all the others as having the appearance of being composed of letters of the alphabet. The *mudra* is more likely to be alphabetic monogram of a minister like Kautilya than to be 'steelyard' [i, p. 52] even if something similar, the *bismar,* is to be seen in Egypt. I am unable toassign the rest of the coins to Mauryan rulers: but there are at least three more of them associated with the marks numbered. In Bhattacharyya's scheme, 102, 27, 124.

Only one prominent group, *M.i.e.,* that with the rhinoceros mark, seems at first to contradict the findings of this memoir. As Walsh gives the sadaracakra in the form *1a,* as *M.1* appears on the double obverse coins, has other members with as many as 14 reverse marks, and contains no less than 38 coins [3, p. 67], we should have a group comparable in age and importance with *B.b.1,* apparently belonging to the same dynasty, but with entirely different marks, and without a successor. The explanation seems to be that at least in this case, Walsh's identification of the sadaracakra is wrong; in fact, both Durga Prasad [i, plate 10] and Bhattacharyya [33, pp. 69, 70] give a different form for the *cakra,* one with dots or taurines enclosed in trefoils, not in ovals; the divergence is unmistakable. One possibility would be that the coinage is to be associated with the final survivor of the older line the last descendant of Ajatasatru; or, it might represent some independent ruler who reigned at about the same time as Susunaga of the Pali records and coinage *B.b.* i and whose prosperous but evanescent kingdom was later absorbed in the general Magadhan empire.

Statistics will give a respectable footing for conjecture. Surely, if mathematical analysis tells some watcher of the skies where to point his telescope that a new planet might swim into his ken, it is capable of rescuing a dynasty or two from oblivion. But to expect it to reveal the name of either planet or king is a bit too much. Of course, the names are not so difficult a matter of conjecture as what song the Sirens sang or what name Achilles bore among the maidens; but with our monstrous number of conflicting variants, even the Valentinian law of citations is useless. Only Bimbisara, Ajatasatru, Udayi, occur in all sources with the exception, again, of Taranatha.

We have come far enough from statistics, but one question must be raised nevertheless. What was the epoch and the effect of the rise of this new form of Government, associated with a *mantri* (as distinct from the official lieutenant, amatya) ? What is the etymology of the word *mantri?* Does it not originally signify the possessor of a mysterious ritualistic formula for success? Allan notes that large clay seals of the type of Yaudheya coins occur [2, p. *clii]* with the legend "Yodheyanam jayamantradharanam";* he (or Hoernle) translates *Jayamantradhara* as "councillor of victory", whereas it should be, with a greater probability, 'possessor of the formula *(mantra)* for victory'. Contemporary Pali records show comparatively small kingdoms directly administered by the Ksatriya's. But susunaga is an *amacca;* and Jain records are more interested in the mantri's of the 'nine Nandas' than in the rulers themselves. Nanda (Mahapadma) is a *mantri* himself who becomes king and has trouble with his own ministers; [29, pp. 434) 435] (Viragyamas Mantrina and Virakta Mantrivargastu). So far, we have not a Brahmin of prominence, though mantric knowledge must have been the virtual monopoly of Brahmins, the witch-doctors or medicine-men of a previous age. But with Canakya, we have the minister towering (at least in theory and tradition) far above the occupant of the throne. The process culminates logically a couple of dynasties later in a neat parallel to the Peshwa usurpation: a dynasty of Brahmin *kings,* the Kanvayanas [Sungabhrtyas, 27, pp. 33, 35]- The reference to a mantri Kanika in the Mahabharata is spurious, and purged from Sukthankar's critical edition of the *Adiparva.* It is also significant that the detailed, even unpractical, regulations and penal theory of taxation of the *Arthasastra* are associated with Canakya (Kautilya). Does this not mean a change of quality with a change of quantity: the spiritual and religious minister to a petty ruler transformed into a political minister when the kingdom becomes too large and prosperous for direct personal administration ?

In the *AMMK, mantri* means usually the possessor of a formula: *evam mantri sada gramam pravised bhiksanujivinah* [28, p. 99; also, p. 89]. The work of Taranatha, late and unreliable though it might be, contains an unexpected confirmation. We find that Asoka (whether the Mauryan or the Saisunaga Kajasoka is immaterial) was under the influence of Brahmins of the Bhrgu clan, and it is now known that precisely the *Bhdrgava's* were responsible for the rewriting of our most important ancient works, particularly the Mahabharata [Sukthankar, Epic Studies VI, *Annals of the BORI,* 1936]. Such things have happened in other countries. From Geoffrey de Beaulieu, father confessor to Louis IX to his 'Grey Eminence' Cardinal Richelieu overshadowing the throne of Louis XIII is surely a natural progression.

No exploration is done without a great deal of preliminary spade work, usually by others who take no direct part in the expedition. This is no exception. I have to thank my colleagues, officials of the Bombay University Library, the staff" of the Bhandarkar O. P. Institute, and senior and junior officials of the Indian Meteorological Observatory for the use of their library and calculating machines. But this work owes most to the help and criticism of three friends. Dr. V. V. Gokhale of the Fergusson College helped me with his knowledge of Maha-yana Buddhism, and reinforced my hardly rudimentary knowledge of Sanskrit; he also read through the whole typescript in all of its several stages of growth. All Chinese and Tibetan references arc due to him. Dr. V.S. Sukthankar of the Bhandarkar Institute helped by means of an extensive correspondence on my behalf, without which I should not have been able to obtain many of the books necessary, nor a good deal of the data given by various officials. Prof. Jon Maclcan of the Wilion College, Bombay, also helped in the inspection of my bibliographical

material; in particular, the reference to Eden and Yates came from him; he was also instrumental in obtaining data from the Bombay Mint. The reader can blame Prof. Maclean for being the person who is responsible for my taking a holiday from tensor analysis to dabble in the intricacies of statistics; but otherwise, no blame attaches to any of these three for whatever I may have done or failed to do in this paper. My faults are my own, and should not detract from their reputation; but surely, if this paper represents any solid achievement, a good deal of the credit must go to them, and to my father, Prof. Dharmananda Kosambi, who first gave me an interest in our classical antiquity.

The work of Bhattacharyya [33] came to hand too late for the fullest use to be made of it. It is, however, a fairly competent piece of work, publishing the find of the Patraha hoard on the model of Allan's British Museum Catalogue. One fault has been copied from the model: labelling the numerous small punchmarks of the earlier period as 'various', instead of counting them directly. Though the author corrects Allan's readings wherever necessary, the memoir is by no means irreproachable. On page *iv,* we find all the metrological fables repeated trustingly: Cunningham's standard *raktika,* the Bhandarkar-Spooner gradations "by the successive and regular rise of a 1/2 *masha",* and Walsh's molten copper poured over the coins, to make up for the weight. The 'new' forms of the sadarcakra described on p. 5 have something in common with Walsh's: *2g=1 d, 20= in* (probably), 2 5=10, and perhaps *2p = 1h* or *1hh.* The statement at the bottom of p. *v* "up to this time no animal turned to left has been found on punchmarked coins" is definitely not true, as we find such animals in Durga Prasad's comprehensive work [i, plate 3, 10; plate 4, 84; plate 5, 98-101; plate 6, 4-5; plate 8, 2; plate 12, 39-40, etc.]. From my point of view, one of the most serious faults is that a selection of the coins has been made, so that statistical analysis becomes very difficult; in any case, the material would have been refractory, and as the author does not specify the amount of copper removed from some of the coins, the metrological value of the publication is low. In the preface by K. N. Dikshit, and again on the opening page of the author's introduction, we find that out of 2873 coins, 1703 pieces were selected. This statement might be true, but only in a very peculiar way. In fact, pp. 93-97 give an appendix which lists the coins by serial number and their classes. In this, coins numbered 589-602; 774-6, 815-^6, are omitted. But they occur in the text; the coins being numbered 1-1703, with an extra coin 8143, and with the single specimen of Class 1 not numbered at all, one expects the total to be 1705. But on closer investigation, it will be found that the text omits, though the appendix does not, coins 109 and 369, without any explanation.

It seems to me highly objectionable that two such technical works as those of Walsh and Bhattacharyya, appearing in the same press and under the same authority within a few months of each other, and containing so much that corresponds and must be compared, should contain different systems of arrangement, and two entirely different notations. This is all the more surprising in view of the fact that our Archaeological Survey can never be accused of rushing into print. The Taxila hoards were found in 1912 and 1924; the Patraha hoard in 1913.

I could have gone a good deal further but for the unsatisfactory condition of the date. As dated hoards are rare enough, and yet provide the only method of studying our punchmarked coinage, at least in the absence of literary evidence, I suggest that our numismatists and treasure trove officers pay more attention to numbers and weights, before and after cleaning. This does not mean that hereafter an archaeologist must also know statistics; an acquaintance with the elements of arithmetic and of proof-reading would do.

8 THEORETICAL CONSIDERATIONS

It was my original intention to add a final section explaining the techniques of statistical analysis. But the memoir has grown far beyond its initial conception; and any numismatist who is capable of understanding such an explanation would do better to consult the works cited, particularly 13-18 of the bibliography. A weak point of this paper is the omission of tabulated data for the coins in a form which would make it possible for any statistician to check my results. These tables have to be omitted not only to save space, but also because my sources are open to suspicion, and should be revised from new observations. I feel confident, however, that whatever the errors of calculations and even of observations recorded in my sources no important conclusion presented in the foregoing as definite will be upset by fresh study of the available material.

There remains, however, one point of some theoretical interest which I discuss here without preliminary explanation: Is the *z* test applicable to skew distributions? The question seems to be still open [26], and a theoretical discussion would not be superfluous. For any particular and specified distribution, the problem can be formulated — usually in a stupendously clumsy manner — as an exercise in the integral calculus.

Let *p (x)* be a frequency function, i.e., have the properties:

$$p(x) = 0,$$

$$\int_{-\infty}^{\infty} p\,dx = 1.$$

The distribution function of the corresponding variate would be

$$F(t) = \int_{-\infty}^{t} p\,dx$$

and the characteristic function can be calculated as usual:

$$f(x) = \int_{-\infty}^{\infty} e^{itx}\,dF(x),$$

the integral being valid, if it exists in the Lebesgue-Stieltjes sense, even when $p(x)$ is not continuous. Let us further assume that the mean value of the population is zero, i.e.,

$$\int_{-\infty}^{\infty} x\,dF = \int_{-\infty}^{\infty} xp\,dx = 0,$$

which involves no loss of generality.

The distribution function of the square of the variate is easily worked out from the above. The probability is that $0 < x^2 < t$ is clearly the same as for

$$-\sqrt{t} < \sqrt{t}\;,\ \text{i. e.,}\ \int_{-\sqrt{t}}^{+\sqrt{t}} p\,dx.$$

But this integral is seen at once to be the same as

$$\int_{0}^{\sqrt{t}} \left\{ p(x) + p(-x) \right\} dx.$$

It follows, therefore, that the distribution of the square of the variate measured *from the mean of the population* is the same as for a normally distributed population provided

$$p(x) + p(-x) = \frac{2\exp(-x^2/2\sigma^2)}{\sigma\sqrt{2n}}$$

The condition is both necessary and sufficient, as is obvious. It follows that mere skewness of the distribution does not affect the distribution of the sumsquare, because the sum of n squares has a characteristic function which is the nth power of the characteristic function of the distribution of a single square. If the function *p (x)* can be written as

$$\exp(-x^2/\text{rho}^2)\, h(x)$$

and the power series expansion of *h (x)* has no even power of x except the constant term, we see that the distribution of the square and of the sum of n squares will be the same as for the normal distribution. Alternatively, we can state the result in the form that the expansion of *h (x)* in Hermitian polynomials should contain, except for the constant term, only polymials of odd degree. Except formally, the two statements are not the same, as the types of convergence are in general entirely distinct for the two expansions. In any case, all moments of even order must be the same as for the normal distribution.

The real difficulty of the problem lies in the fact that the variance calculated is never from the usually unknown mean of the population, but from the actual mean of the sample. For a normal distribution, this means only the loss of one degree of freedom, the resulting distribution function being the same as before with n replaced by $(n - 1)$ [J. V. Uspensky, *Introduction to Mathematical Probability,* 1937, pp. 331-336 contains the best derivation of this result]. In our case, this cannot be true; for the skew distribution, the distribution of the mean,

$$m = 1/n\,(x_1 + x_2 + \ldots\ldots + x_n)$$

is not the same as for the original population; and $S\,x^2 = S(x_i - m)^2 + nm^2$. So, there remain two procedures. In the rare event of the 'true' or population mean being known, calculate the variances of the samples from this (dividing by n instead of (n — i) as usual) and entering Fisher's tables of z with the degree of freedom as the actual numbers in the two samples (instead of one less than the number, as usual). In the general case, however, when the population mean is not known, it is clear that the usual distribution of a will be closely approximated by all but the smallest samples. It might ,however, be better—when the means of the two samples show only a trifling difference or when several samples have to be compared to each other as for analysis of variance—to calculate a general mean from all the samples present, which can then be taken as a reasonable approximation to the population mean.

For kurtosis, the general situation is decidedly not the same. The distribution of the square of a variate with the frequency function

$$cx^{2k} e\, \exp(-x^2/2\text{rho}^2)$$

is again of the incomplete gamma function type. Now, because the characteristic function for the sum of n variates is the nth power of the characteristic function for a single variate, it follows that the *"cupid's bow" distribution has the same behaviour with regard to the z test from the population mean on a normal distribution provided the number of degrees of freedom is multiplied by (2k* + i). Let us, as an illustration of the procedure, consider further a leptokurtic distribution with frequency function given by

Of course, no generality is lost from the fact that the origin has been chosen as the mean, and the variance specialized. The question of skewness has already been settled. Applying the methods cited, it will now be seen that the net effect of using Fisher's tables of *z* for two observed samples drawn from such a distribution will be to *underestimate* the significance of the result. That is, a ratio of variances that is just on the border line could be regarded as significant. And in fact, if we take the extreme case a=0, it will only be necessary to enter the tables with three times the usual number of degrees of freedom. Now, inasmuch as the task of fitting such distributions has to be fulfilled from the samples themselves, the 'true' or population distribution being unknown, it is not worth while here to go deeper into this matter, particularly as the methods of Cramer reduce the entire problem to an exercise in integration. For platy-kurtosis, the opposite effect, i.e., over-estimation of the significance is to be expected.

Let the weight of a population of coins as it leaves the mint be normally distributed with mean *mu* and variance *rho*^{2} *1* Let the loss of weight per unit of time be also normally distributed, with mean mu1 and variance *rho*^{2} *2.* It then follows [18, p. 50] that the population *t* units after the time of issue has normally distributed weight with mean mu = mu1 — tmu2 and variance rho^2 = rho^{2}1 + t.rho^{2}2. This must hold at least to first approximation as the usual law for coin-weights. However, inasmuch as the only possible gain of weight for a coin is by encrustation or the accumulation of dirt, both of which it is the practice to remove before weighing one would expect worn hoards to show more and more strongly marked skew- negative weight distributions.

In general, greater variance would be as much a characteristic of age as lower average weight. But in case the minting process changes suddenly, the problem becomes complicated, because the greater variance may be associated with the new process and hence indicate the later coinage. There is a curious parallel to this in modern times. Like the Mauryan Karsapana, the new rupee coin, about to be issued for general circulation, contains much more copper than its immediate predecessor, is minted by an 'improved' process, and is expected to show a greater variance, perhaps to the extent of making it necessary to abandon the present legal remedy.

The theoretical coinage absorption curve (Fig. 4.3 ; p. 46) was obtained for simplicity by fitting a linear regression to the logarithms of the observed numbers of the coins. But there are better ways of estimating the rate of absorption. Let *a* exp(-r) be the number expected, and y, the number observed at the rth indiction, the observations extending from zero to *n* indictions. Then we must have

$$\sum_{0}^{n} a e^{-r\theta} = \sum_{0}^{n} y_r = N, \text{ or } a = N(1 - e^{-\theta}) / (1 - e^{-\theta\overline{n+1}})$$

The likelihood [13, p. 312] is given by

$$L = \sum y_r (\log a - r\theta) = N \log N + N \log (1 - e^{-\theta}) - N \log (1 - e^{-\theta\overline{n+1}}) - \theta \sum r y_r .$$

For the maximum likelihood, we set the derivative equal to zero, and obtain as the 'best' estimate, the sole positive real root of

$$(n-s)x^n+(n-s-1)x^{n-1}+\ldots+(1-s)x-s=0$$

$$\text{where } x=e^{-\theta},\ s=\sum ry_r/N.$$

Using the notation of p. 34, the maximum likelihood estimate of the fundamental weight unit would be given by

$$u=\frac{\sum \frac{n_j r_j a_j}{j\sigma_j^2}}{\sum \frac{n_j r_j^2}{j\sigma_j^2}},\quad (\sigma^2{}_j \text{ are the population variances}).$$

The best estimates of the variances would be sampling second moments calculated from the theoretical means arj The complication due to the reappearance of *u* may be avoided by using the ordinary sampling variance rho.j^2 which will make very little difference in practice.
t

In closing, it should be noted that there are other modern theories of statistical estimation than that of Fisher [13, IX and bibliography]. The most prominent of these is that of J. Neyman and his collaborators [Phil. Trans. Royal Soc. London. Ser. A. Vol. 231, pp. 289-337; vol. 237 PP- 333-380].

The punchmarked coins have led us around the full circle from pure conjecture to pure mathematics. But I think the effort justified, however unreadable my paper might be. A Central Asiatic document [Liiders: Die Siakischen Mura, 736-7] reports the Tathagata as saying: "With these, once upon a time, have various beings performed significant actions; for this single coin have men once destroyed one another. Numerous are beings who have once falsified just one coin and even now find themselves in the state of painful transmigrations, experience diverse sorrows. Some gained merit for themselves towards the Buddha the Order, or a preacher of the law, and even now sit among the gods." The attitude towards currency has changed. Yet, these crude-looking bits of metal are the remaining drops of a stream that flowed, then as now, for the benefit of the few, and was kept moving by the bitter exertions, abject poverty, hunger, misery, toil, and bloodshed of the many. These pitiful remnants of a remote and powerful but obsolete civilization should not be without interest when our own is moving so rapidly towards obsolescence.

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5
On the Origin and Development of Silver Coinage in India

ON JULY 22, 1941, I received from the Director General of Archaeology in India the eleven (actually twelve) silver pieces illustrated and described here. The question that I was expected to answer was whether these were the predecessors of the later punchmarked silver coins. One glance at the weight showed that, taken as a group, they could not possibly be such predecessors, except in so far as any historically earlier bit of precious metal precedes any later one. But I was particularly intrigued by the cuneiform mark on No. 9, and asked Rao Bahadur K. N. Dikshit whether he or any of his experts had noticed anything special about that piece. In answer, he sent me a copy of his report made on the very date of his discover)', whereof the relevant portion is appended here.

1st January, 1926. The most important discovery during this year's excavations was made on the morning of the New Year's Day when underneath a wall running east and west in the trench between sites *B* and *C* was discovered a silver vase (No. DK. 1341) complete with lid containing jewellery, square and circular silver pieces. One of these is inscribed in cuneiform characters, thus connecting once for all, the period of the last city on this site roughly with the cuneiform world. As it is well known, the Babylonians had no regular coins but used lumps of silver and gold of definite standards known as Mana or Shekele. In the 8th Mandala of *Rg Veda,* Indra is asked to bring *Manas* of gold *(Hiranyaya Mana)* which conclusively proves the use of these forms of weight in India at the time when the Aryans came. The find of these rectangular and round silver pieces (the precursors of punchmarked coins of later times) with cuneiform signs is, therefore, of the highest importance for setting the chronology of Indian history.

My own comment on these views will appear later on. But I cannot refrain from expressing surprise and regret that this report was not made public by the authorities to whom it was submitted. The Annual Report for 1925-26, rewritten by Mr. Ernest Mackay states: "The find is so important that it deserves to be described in detail",[1] but omits all mention of the cuneiform punches on No. 9: the vase itself is shown on Plate XLIIIC, and the contents on Plate XLII, with the piece in question shown at the bottom, though the cuneiform marks cannot be discerned clearly (due presumably to the angle of incidence of the light). No mention whatsoever has been made of the piece or of the marks on it in the imposing tomes of Marshall[2] and Mackay[3] on the subject, which is curious in view of the fact that Mackay was specially brought in as "an archaeologist from outside with a firsthand knowledge of pre-historic excavations in Sumer and Western Asia" [Marshall, p. 13].

The pieces sent to me (see Fig. 5. i at the end of this book) fall into no less than four classes, best described separately. With the exception of the cuneiform, on No. 9, the rest only bear 'incisions', which are merely chisel marks. Such of the pieces as have been cut off from bits are undoubtedly cut by the process of hammering on a cold chisel and then breaking off at the mark by force; the resulting fracture shows an edge that is partly smooth and partly rough. Thus the 'incisions' are trial marks, perhaps marks of the end of the chisel when making other cuts.

Class I, No. 9 (23. 4010 gm): The piece has been cut off at both ends by chiselling and breaking off from a larger cast silver ingot. The process of cutting described above characterizes currency in the earliest times, and still survives in some parts of the world. In parts of Burma, at least till 1897, it was the custom to take along a metal ingot when out shopping, and cut off suitable pieces to approximate weight as small change.[14] The pieces then continued to have their own independent existence.

Assyrian inscriptions mention ‘sealed’ *minas* and *shekels* from the time of Sennacherib onwards, and these are taken by archaeologists to be cast roundels, which might have resembled our Class II here. Our piece seems to be too light to be a *mina,* and is too heavy for any *shekel* within my knowledge. It is to be noted that the standards of weight varied; an accurate study, particularly of hoard material, might enable us to date the piece according to the weight standard. The Assyrian *Zizu* continued to be known in early Christian times as a round coin of very small value, having followed the course of all coinage in debasement and lightening. The *shekel* of Josephus is about 210 grains, which is almost exactly the Mohenjo-daro weight that amounts to four times the *Karsapana* weight of our punchmarked coins. But this is probably insignificant, because the earlier *shekel* (= 1/3000 talent), whether Attic or Hebrew, was heavier.

The inscription on the reverse is most probably to be read as *gam* or *gur* (No. 206;[4] No. 318;’ No. 344°) taken horizontally, with less probability, it would be sign No. 2, *hal.* The meaning is not clear when the sign stands by itself, but here it might indicate ‘to pour forth’, perhaps the casting of the original ingot. The larger ideogram on the obverse would certainly have been taken as a mark of denomination or a numeral sign, but for the fact that three of the wedges are long. Even now it is difficult to see what else it could indicate in view of the fact that all the wedges point in the same direction without a single cross or *u* wedge. The nearest signs to it are the *in* of Elamite inscriptions at Behistun, and *dugud (Barton*[5]*,* p. 401); it is certainly neither of these. I hope expert Assyriologists will forgive my amateur efforts, as also the fact that I am unable to see anything special in the signs that might permit us to date the find. The ‘Cuneiform World’ endured from at least 2500 B.C. to the Persian Empire, and we know that Alexander’s conquest and the supersession of the empire by the Seleucids did not end the use of cuneiform, inasmuch as an inscription of Antiochus Soter (280 B.C.) has been found in quite good Assyrian. Not only that, the ‘letters of the Ammunneer’ of PhiloByblius probably refer to the Ras Shamra alphabet and would indicate that there existed people who could read cuneiform writing in very much later times, though the full bloom of the Assyrian language begins about 1400 B.C.

The primary importance of the piece, then, derives from the obvious conclusion that it was imported from the West, presumably Mesopotamia, in the way of trade. Silver deposits are not known in India within reach of the Indus Valley; it would seem likely in view of the Indus seals found in Mesopotamia that all the Indus silver was imported thence in payment for other commodities. The piece under discussion and other pieces of the find show us that we are, before the last city on the Mohenjo-daro site, already at the beginning of a rough coinage system. A late Sanskrit word for such a cut and broken piece of silver or gold might be *kanakabhangah,* which is found in our lexica. But, along with the silver, the coinage system is also imported so far because the pieces, except, Class IV, do not conform to the general standard of weights found at Mohenjo-daro and Harappa.

In some respects it might be possible to go further than this. There are many who assert that an intimate connection between Sanskrit and the cuneiform script must have existed, because Sanskrit is yet a ‘syllabic’ language, our alphabets still hearing the consequent marks of intricacy; as with the cuneiform ideograms, a single Sanskrit word can mean a large number of ideologically unconnected things. Some have attempted to trace Assyrian roots and names in the Vedas, attempts originating in as well as hampered by the autgor’s lack of mastery over one or (as in my case) of both the languages concerned, and their disregard for the fact that we are at the dawn of modern language structure, in an age when language itself was one of the mankind’s rare instruments. It would, however, be possible to admit that in the very first line of the *Rg Veda* the root *il* occurs which can be connected with the Assyrian *ilu* =god. And the cuneiform determinative *ilu* can also be read as *an,* which is the name of one of our ancient deities. But all this need not be relevant here because the root can also be taken as Dravidian, and even today the Brahui language is a Dravi-dian survival far to the north, surrounded entirely by Aryan languages. The Aryans who succeeded, perhaps ruined, the Mohenjo-daro culture could have had their contact directly with the Assyrians in Asia Minor or Mesopotamia, as witness the Mitanni inscriptions, and the 'Asuras' mentioned so often in the Vedas. By this, the 'Asura Vipracitti' would be a Hittite, as *citti(m) =hittim* (Hebrew) seems to be a permissible equation. The Assyrian word for silver *ka-as-pu* might have left the Sanskrit root *has* or *has* (to shine).

As I have said, there is no evidence that these speculations are to the point when dealing with Mohenjo-daro. The war has made it impossible to communicate with the leading Assyriologists. It would have been most helpful, for example, to be able to consult Hrozny's reported decipherment of proto-Hittite inscriptions on Indus seals. The question might be raised, however, whether the cuneiform marks could not have been made in India. Certainly, there has been found one seal, at Ur itself, which is definitely of the Indus type but bears cuneiform marks in place of the usual linear Indus script (Barton[2], pp. 406, 413). The evidence before us at most allows us to expect that there were some people in the Indus basin, whether indigenous or immigrants, who might know how to use cuneiform, but it could never have been a common script in India. The Sanskrit for a cuneiform seal, punch, or ideogram would be *Kilamudra,* which is not to be found in the dictionaries, though it might conceivably occur in some obscure tantric work. Liiders, however, has pointed out that the Prakrit equivalent does occur in the Niya Kharosthi tablets, where he takes it to indicate the sealed wedge-marked documents themselves. Two Assyrian clay tablets of about the sixth century B.C. relating to the sale of two women were found in a Bombay store room; the provenance being unknown, these are probably to be taken as modern imports from some tourist's acquisition at a Mesopotamian site. Pran Nath reads a wedge as *ni* on a punchmarked coin in the Thorburn collection, but this too seems doubtful to me. The one find comparable to that described here is the pot-inscription published by C. L. Fabri[10] as a Sumero-Babylonian precuneiform label. But even here, the reading was contested by Heras who preferred to read the complementary area on the pot itself as being typical Indus writing. So, we have before us the first, and at the present the only, known cuneiform and definitely Mesopotamian writing in ancient India.

Class II: This consists of three round pieces which have really nothing in common but their shape; they form no system of weight, and as each is manufactured by a different process, it is doubtful whether they represent coinage. There is just a chance that they were meant to find their way into a jewellery pattern, which need not, however, conflict with their interim use as coins. No. 3 (2. 2177 gm.) has been flattened out from a cold silver pellet or other smaller piece by means of hammer blows of considerable force, as is seen from the cracks that have developed at the edges, and the appearance of the surface. No. 11 (2.9353 gm.) is of very bright silver, but has a patch of brown lacquer-like coating that prevents a thorough examination. From its shape and general appearance, it must have been cast to size and then lightly worked over. No 2 (4. 3108 gm.) has been trimmed from a larger silver plate, the corners being neatly rounded off. The only mark it bears is the common chisel mark or incision.

Class III: This can be taken, roughly, to form a fystem, though the system cannot be associated clearly with any known Indus weights. The basis might, at best, be connected with the Paila coins, about which I have not at present sufficient information for a definite statement ; if the coins now in the Lucknow Museum become available for study at some later date, the point could be settled.

The nearest pieces in this group are too heavy for the Taxilan 'long bar' coins. It must be kept in mind that the standard is only roughly followed; but it would seem to be a foreign standard so far as can be judged from the evidence. No. 6 (2.8867 gm) is cut and broken off from a thin plate, rather like the later punchinarked coins in appearance, but too light for the *Karsapana.* The sole mark is a chisel mark on the face. No. 8 (5.8353 gm.) is similarly manufactured, but with only one rough edge, one chisel mark on face. No. 10 actually happens to be two pieces made by cutting a round, fairly neat, well-filled piece almost exactly in two with a blunt cold chisel. It is described as 'broken', on the DGA's containing envelope. This might denote completion of the fracture after excavation, but the original intention of cutting the piece in two is in any case obvious. It is also clear that whoever did the cutting had plenty of practice, inasmuch as the two pieces weigh 5.9039, 6.0720 gm., an excellent dichotomy, considering the bluntness of the tool. Finally, No. 1 (19.4787 gm.) is apparently half of a still bigger piece, the cut edge having been made smooth. The system of weight is apparently on the scale of 1, 2, 2 + 2, the last being close to seven units, which does not coincide with the dual Indus system of increasing weights. Perhaps, the last piece should not be included here at all.

Class IV: These three pieces are weighed on the Indus system, and if there be any 'precursors' of the punchmarked coins in the pieces sent to me, they can only be these.' These are all from one find (DK. 1341), seem unfitted for use as jewellery without further shaping, and the weights belong approximately to the Indus Class *D,* being: No. 4=3-3576 gm., No. 7 = 3.7025 gm., No. 5 = 3.9282 gm. The first is a sector from a round piece, the second from a plate cut off after several trial attempts; the third also trimmed from a plate, but with one edge probably circular in the source. The fact that claims our attention here is that these are significantly cruder (by the *Z* test), than the Mohenjo-daro Class *D* weights or earlier Taxilan *Karsapanas;* and also significantly heavier than both by the *t* test. There are two interpretations possible: that the pieces represent purchases of silver to an approximate Class *D* weight; or that they were tc be smoothed down at a later date to the precise weight, having had a little margin left and in fact as little margin as possible with fairly clumsy cutting tools. If the latter explanation is accepted, the conclusion must also be taken that we are already beginning to see bits of a precious metal trimmed to *a* standard weight, hence the beginning of a coinage system. In any case, the coinage that came later must have originated in some such way, if this be not its immediate origin.

The later developments are quite clear. Even after the destruction of Mohenjodaro, which was entirely a trade city as shown by its fine weights and poor weapons, the traders persisted, and continued to use the very accurate weights of that period. The first marks were traders' marks, such as are seen on Persian sigloi, and the reverse of the punch-marked coins of the pre-Mauryan age. This is shown clearly by one coin published by the late Babu Durga Prasad.[12] This coin is blank on one side like our Mohenjodaro pieces, but the other contains no less than thirteen small mark"!, similar in type to those known as the later 'reverse' marks. For the earlier Taxila hoard I have established these marks[13] as having been regularly placed in time, with a loss of about 0.2 grain weight per mark. Moreover, the newest coinage of the earlier Taxila hoard, *B.E.* 2, shows that if a single standard prevailed for those coin[1], it must have been almost exactly 54 grains at the time of issue. So, Durga Prasad's coin, weighing (according to him: "I have not been able to check the weight") 105.75 grains would have been worn down from the 108 grains *double-Karsapana* particularly as the central one of the 13 marks seems to me to be an issue mark.

My contention is that the manufacture of coins continued to be the trader's function for a long time after the Indus period; that the small marks were put on according to a system generally understood at the time by those who handled the coins most frequently. It follows from my previous work that the traders (or the Vaisya caste) were very accurate in their workmanship, and gave good value.

At a *later* period but not later than the sixth century B.C., the Ksatriya steps in as the king who claims the royal prerogative of stamping his own marks on the coins. The punchmarked coins then begin to have larger obverse marks, usually five in number (four for the Paila Coins), and are issued with a blank reverse. The Mohenjodaro accuracy still persists, the trader still continues to stamp on his own small reverse marks as per his own checking system, till the Mauryan period. This ushers in coins characterized by the crescent-on-arches mark on the obverse, and the system of traders' reverse marks disappearing very soon, being replaced by a single large reverse mark, such as the 'Taxila Mark', or some other characteristically Mauryan Stamp. The superb accuracy of the weighing is also lost, and the coins have much more copper than before. Some of my critics wonder at this cruder technique, which seems unlikely to them in view of the *Arthasastra* and the fine sculpture, architecture, epigraphy of the Mauryan era. I prefer to form my judgement from the coins themselves. As a matter of fact, the present year in India is certainly not inferior in productive technique to any of its predecessors; but, due to pressure of increased trade and a corresponding increase of the need for coinage, along with a certain amount of hoarding caused by the war, the new rupees will be found inferior in minting to the older ones. At least, they contain more copper (an increase from 1/12 to i), and the variance at the time of minting[15] is, to the best of my knowledge, much greater for the George VI than for Victoria, Edward VII, or George V rupees. The parallel explanation is undoubtedly that the Mauryan conquests opened up entirely new regions; the old, limited, slow, cumbrous trading system between India and Mesopotamia must have vanished against die pressure of a rapidly increasing volume of trade in the new areas opened up in the south. At any rate the primitive tumuli, the pandukutis of the southern part of the peninsula survived so late as to contain coins of Augustus, not to speak of our punchmarked coins. Not only that, in such southern hoards as I have been able to study, the proportion of Mauryan coins is very large[16] some hoards of over a thousand coins apparently consisting entirely of punchmarked coins of the later Mauryan period or their imitations. This can mean only one thing: that coinage as such was virtually unknown in the south of India before the Mauryans. In the north, we rarely get a Mauryan hoard of any considerable size unmixed with pre-Mauryan coins. The later Taxila hoard of 168 coins is purely Mauryan (excepting one coin of Diodotos), though so crude in fabric as to b'; suspected as a forgery; and all but five coins are in mint condition, which indicates some unusual circumstance attending the deposit.

Thereafter, we come to the period of cast coins, which nevertheless retain some of the earlier marks. Local and transient weight systems also develop, and the unifying influence of the trader is entirely lost, probably because of the development of large kingdoms at war with each other, each with its own provincial culture and language. Indian numismatics thereafter becomes a branch of epigraphy.

Nevertheless, in closing this note, I wish to point out the necessity of studying *hoards* of coinage as a whole and for every period if we are to reconstruct the lost economic and political history of our country from our unusually meagre and conflicting records. For example, from a study of the earlier Taxila hoard, I have been able to show that the Taxi-lans enjoyed comparative economic stability for at least sixteen and probably twenty inductions, say two centuries or more. But a great deal more can be said from the mere structure of the hoard. Of its 1175 coins, 1059 were exactly of the type found further east with maximum density at or near the ancient Magadhan Kingdom; 79 were minute coins, the small change of the day, and might have been local; as also tht 33 'long-bar[1] coins, not found in Magadha, which are close to being double-sigloi. Just four more coins were found in the hoard: two of Alexander, one of Philip Arrhidaios, and an unidentifiable Daric (siglois). This shows quite clearly that Taxila belonged to the Indian, Magadhan, economic sphere at a time when it is supposed to have been a part of the Persian Empire, or at least in the Persian political sphere, since the conquests made by Darius I. The balance of trade, moreover, was in favour of Taxila, the coefficient of survival of currency being 0.71 for the currency so regularly imported from the east. Therefore, after Alexander's invasion had swept away the strongest tribes of the Punjab that acted as buffer states, a Magadhan conquest of Taxila was inevitable. Therewith must have followed the doubtful staius of a frontier dependency to replace what had essentially been a centre of exchange between two vast trade regions, and the Taxilan economic advantage must have been lost. This would explain the revolts that are referred to as having occurred at Taxila, one of which Asoka[17], apparently, had to quell as Viceroy; and the steady ruin of Taxila following the Maur-yan conquest. But without the hoard material, we must always remain in doubt as to the true significance of our literary sources. Just as a race has to be studied by taking a fairly large sample of its representatives, so also the coins left by a vanished age must be studied by looking at their weight and chemical composition in a group. A single coin is just about as representative of the culture as a single individual of the race.

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9 Pran Nath, *Indian Historical Quarterly,* 1931, Vol. VII, Supplement., 14. 10 C. L. Fabri, *Indian Culture,* 1936-37, 3, pp. 663, 673, plate, n H. Heras, *Indian Historical Quarterly,* 1937, 13, 697-703.

12 Durga Prasad, *Journal of the Asiatic Society of Bengal,* 1935, Numismatic Supplement No. 45, 13, plate 7.

13 D. D. Kosambi, *New Indian Antiquary,* 4, 1941.

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15 My own observations show that the variance of the George VI (1940) rupees is not less than that of George V rupees twenty-three years in circulation, i.e. of the order of three to four times the former minting variance. The legal remedy seems to have been related to more than twice its former value of i /aoo.

16 From a letter of Dr. K. N. Puri, this seems also to hold for his Rairh finds'in Jaipur State.

17 *Divyavadana,* (ed. Cowell and Neil), p. 371 *et al. seq.*

6
The Effect of Circulation upon the Weight of Metal Currency

IN CONTRAST to the physical sciences, the social sciences allow, even now, the detection of quite important effects with the aid of comparatively simple apparatus and a certain amount of knowledge of modern statistical technique. The historical evidence of the demand of currency shown by the loss of weight of coins still in active circulation comes under this head. The same methods may be applied to hoards deposited in ancient times and recovered intact, thus giving the foundations of numismatics as a science.

The normal law of weight distribution may be assumed to hold for a lot of coins honestly minted to a fixed legal standard in large numbers. The population mean may be taken as the supposed legal weight, the variance could be estimated by taking the number of rejections at the mint beyond the fixed 'legal remedy' by which the coin is allowed to differ from legal weight. Supposing the minted weight distribution to be represented by I in Fig. 6. 1 (and ignoring the absorption of the coinage), the effect of circulation will be to lower the mean and to increase the variance as in II. Further circulation changes die curve to III, where only the heavier half has been drawn. Deviations from normality will become more strongly marked and the currency will tend to disappear from circulation. While the general case can be brought under the "homogeneous random process"[1] which is *to* universal in application as to qualify for a law of nature, it suffices for comparatively short periods of time to take the average weight as a linear function of the date.

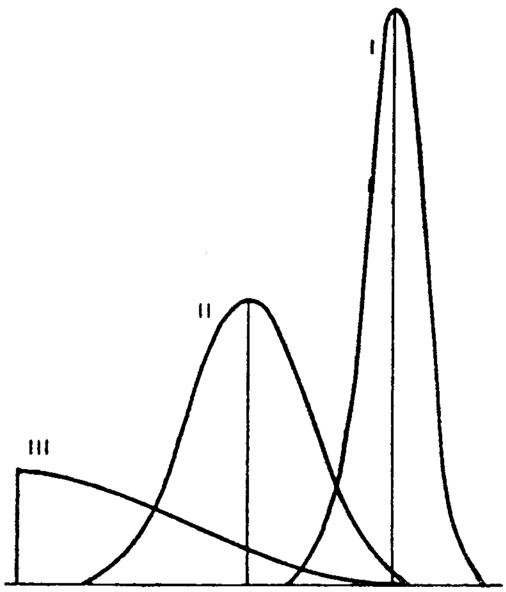

Fig. 6.1: Effect of circulation on weight. Curve I represents minted weight distribution; curve II represents the lowered mean weight and increased variance after circulation; curve III represents the heavier half of the weight distribution after further circulation.

This theory was applied to a statistical analysis[2] of the earlier Taxila hoard (deposited *circa* 317 B.C.), but work on other ancient hoards of interest was prohibited by lack of access to the material and by the honoured custom of scattering most such material *unweighed* after a perfunctory study. So, the validity of the theory is here proved on modern coins from active circulation,[3] as a control measure. During March and April, 1942, I gathered from some stores in Poona, from the great market place *(mandai),* and when not otherwise available, from the day's take over the counter of a local bank as many specimens as my finances permitted and my energy sufficed to weigh. These were stripped of the pieces whose date was illegible, or which were severely damaged by accident, or which did not ring true for the higher denominations. Experience shows that, as regards weight, coins of the latter two classes invariably differ in a marked fashion from the rest of their annual group; for the first, there was no choice. The effect of the two latter discards is to decrease the variance within a year, so that the goodness of fit is actually reduced by this process and the theory stands confirmed even under the most unfavourable circumstances. The date on worn specimens could probably be restored by means of an examination of the crystal structure formed at the time of stamping, but I was unable to devise any method with the apparatus at hand. The pice were taken as they stood; for the other currency, modern specimens, minted in 1936 and after 1939, were in overwhelmingly large proportion and subsam-ples had to be taken to reduce the numbers. The final selections were classified according to the date of the issue and each coin weighed to a tenth of a milligram. The time of the weighing was reduced by using a chainomatic analytical balance of Indian manufacture; the error of the (new) instrument was rather high—0.5mgm.— but decreased with use. Proper checks were taken regularly, and the fourth place of decimals ignored in the statistical work: all means would have to be increased by half a milligram and Sheppard's corrections necessary for the variances of the data were to be used for purposes of estimation. The final stage was the statistical analysis of the weights by the methods of R. A. Fisher.[4]

With larger samples the estimates of composition and even of the actual weight and its variance would be more accurate; reliable information could be gained as to the proportion of counterfeits, mint-defective, dumb, and accidentally damaged coins in circulation. The variation between localities and local needs can also be estimated by the allocation of properly randomised samples to various regions. Finally, the residuals after fif ting the regressions would be of great use in correlating the wear of various denominations to show the extent to which one type was supplementing another and enable a scientific distribution of currency to be made. Any method of currency control based on science, not on the fiat of authority, would have to consider these matters seriously. As for the weights of a large sample, the analytical balances will no longer be necessary; a histogram can be run off directly by setting the mint's automatic weighing machines in series and counting the number of coins not rejected at each step.

A look at the tables of analysis of variance shows at once that the results of my observations are highly favourable to the theory.

Where deviations from the linear regression become significant, they are immediately explicable. The pies being not current in Poona bazaars had to be imported from Benares where they are gathered from the shops before Hindu holidays by the frugal pious, distributed to beggars, and revert to the shops immediately after. This can hardly be called active circulation; as an aside, be it noted that in places like Benares simple bits of copper can be and are still used to substitute for the lower currency: for Benares, the Butwal 'pice'; almost any ancient coin in most of the purely agrarian districts of India.

TABLE 6.1: ANALYSIS OF VARIANCE

(Regression given only where significant)

Unit: one milligram; y—weight in milligrams, x—date in years

Source	d. f.	Sum-square	Mean sq.	F
AE Pice (Benares) 1912-1939; y—1599.55=1.955 (x=929.12)				
regression	1	43015	43015	36.66***
deviations	23	61528	2675.13	2.28**
within a year	198	232300	1173.23	r=0.357
Total	222	336843	1517.31	1.29*
AE Pice (superseded) 1835-1906				
regression	1	35969	35969	(5.95)-1
deviations	27	7133371	264198.92	1.234
within a year	99	21195723	214098.21	r=0.0356
Total	127	28365063	223346.95	1.0432
AE Pice 1907-1941; y—4728.86=9.903 (x=1928.87)				
regression	1	8574800	8574800	1663.96***
deviations	26	201108	7734.94	1.50
within a year	639	3292918	5153.24	r=.843
Total	666	12068826	18121.36	3.516***
N Annas 1908-1941; y—3803.20=6.545 (x=1927.70)				
regression	1	3250147	3250147	1903.31***
deviations	26	132110	5081.15	2.975***
within a year	698	1191923	1707.63	r= 0.843
Total	725	4574180	6309.21	3.695***
N 2-Annas 1918-1941; y—5759.2=8.516 (x=1931.99)				
regression	1	1890586	1890586	695.86***
deviations	16	71021	4438.81	1.63
within a year	315	855827	2716.91	r= .819
Total	332	2817434	8486.25	3.12***
AR 4-Annas 1904-1940; y—2857.9=4.615 (x= 1928.098)				
regression	1	725568	725568	459.70***
deviations	21	56104	2671.62	1.69
within a year	224	353551	1578.35	r= .799
Total	246	1135223	4614.73	2.92***

TABLE 6.1: Continued

Source	d. f.	Sum-square	Mean sq.	F
AR 8-Annas 1905-1941; y—5764.83=5.949 (x=1928.5)				
regression	1	259759	259759	139.86***
deviations	21	31273	1489.19	(1.2472)—1
within a year	43	79865	1857.32	r= .837
Total	65	370897	5706.11	3.07***
AR Rupees[3] 1903-1920; y—11579.86=4.16 (x=1913.12)				
regression	1	15423	15423	674.67***
deviations	16	1130	70.63	3.0898***
within a year	2868	65563	22.86	r= .433
Total	2885	82116	28.463	
AU Sovereigns 1900-1931				
regression	1	72	72	2.382
deviations	11	776	70.54	2.333*
within a year	39	1179	30.23	r= .1885
Total	51	2027	39.745	1.315

The Poona pice fall into fwo classes, the weight of the denomination having been materially reduced in 1907, apparently fo 75 grains. In fact, all pice of my 1906 sample fall into either the 4-gram or the 6-gram group, without a single specimen of 5 grams; the mean for this year is very significantly lighter by the *t* ti st than for previous years, heavier than for succeeding years; the variance by the *z* tesc is significantly greater than those before or after. This seems to indicate that some of the 1906 pice were minted to the lower weight. Thus, the pre-1907 coins have been withdrawn for the greater part or have otherwise tended to disappear from circulation. Only the unworn specimens have managed to survive, whence neither the regression nor the deviation from it are of any significance. For the nickel one anna coins, the deviatiom from regression are caused entirely by the oldest issues; Edward VII, 1908-1910. For these, no less than 15 out of a total of 30 had illegibly worn dates, a proportion fourteen times that of the George V issues. The 23 coins retained were, naturally, heavier than the average for their groups, somewhrt after the fashion of III in Fig. 6.1. A precisely similar effect is to be seen in the Taxilan coins of more than ten reverse marks. A recalculation of the anna data discarding the Edward VII issues immediately reduces the deviations from linear regression to insignificance, so that the deviations are to be assigned to our mechanism of selection. We can thus state a law of wear for metal currency: *For coins in active circulation, the loss of average weight is proportional to the age. But the oldest coins of a series tend to be above the regression weight and for currency not in active circulation or an issue which is superseded, the significance of the regression tends to disappear.*

An even more striking result is that *the correlation coefficient for currency in active circulation over comparable periods of time is independent of the denomination.* Except the pies, the older pice, rupees, and sovereigns all the remaining correlation coefficients do not differ significantly from the population value of p = 0.838, estimated by pooling the observed valuer after Fisher's *z* transformation.[5] The correlation for the 4-anna bits is somewhat low, but there have been disturbing factors at work here: the 1917-1918 specimens show unusual wear and nickel 4-anna bits (not included in this study) were minted in 1919, 1920, 1921. In stating such a 'law' for currency weights, other things must be equal: minting variances must not be great in comparison with those caused by wear, the currency must have been minted over about the same period, and must have circulated in the same locality over about the same time. As a matter of fact, 2,886 rupees of 1903-1920 issue sampled at Poona in 1940 gave me a correlation of 0.43 and deviations from linearity were insufficient to explain this entirely different value. The reason for the difference, however, is very simple. It is known that r^2 is the ratio of sum square due to regression by the total sum square. Our theory requires that the variances increase with age, which means that for coin? longer in circulation, the residual sum square takes up a greater proportion of the total, thus depressing the correlation. Even the pice of our sample show a correlation compatible with that of the rupees when calculated only from the 1907-1920 issues in the sample. It is a feature of the data that when the calculations are made from year to year on the basis of the weights, the correlation coefficient is found to increase steadily wiJi the date of the last issue to its maximum value at the end; this holds for all denominations provided the oldest issues do not contain over-weight survivors in large proportion and the regression is really significant.

Whereas the samples show that the variances are in general decidedly greater for the older issues, the samples do not allow the question of linear increase of the variance with age to be effectively discussed except for the post-1906 pice. The only method I can see that would test this would be (1) to calculate the linear regression from the sample variances, giving each the weight of its degrees of freedom, (2) apply the x^2 test, noting that the ratio of the observed to a hypothetical variance should be distributed as x^2/n. From the total number of degrees of freedom, two have to be subtracted for the fitting. The pice variances only, when all are tested by this method, show linear increase with age; on the whole, the pice are statistically the most satisfactory denomination —in spite of evidence of heavy corrosion of three specimens by fatty acids—because no one rings them, counterfeits and hoarding are absent, change of hands regular.

Brass 1/2 annas, annas, and two annas of 1942 issue just reached circulation at the time of the study, so that no disturbing effect was obvious on the rest of the currency, whatever the future may show. The data gives: 1/2 annas: n =53, *m*=2.9125 gms, rho^2=786.88 $mgm.^2$; annas: n=38., *m*=3.8851 gm., rho^2 = 3934-51 $mgm.^2$; 2 annas: n =22, *m* = 5.8023 gm., rho^2=7773.6 mgm^2. The last two fit very wellinto their respective lines of regression and analysis of variance. It is not likely that the debasement will cause any disturbance due to hoarding, though the rate of wear will naturally change. For, the silver alloy had already changed nearly three years ago from 11/12 to 6/12 fine; even the nickel of George VI appears to differ from the older composition. Even with the pure metal used for each denomination, including the rupee, the currency would have a value of metal well below its denomination, hence the change to brass only emphasizes the most universal of all numismatic laws, the inevitable trend towards debasement in times of stress. For our purpose there is a far more serious effect visible in the samples. The minting since 1939 shows a decided increase in variance, and the occurrence of overweight specimens shows that the old legal remedy (from 1/40 for copper to 1/200 for silver) has been relaxed in practice, whatever the law at present. If this tendency was present in the coins struck during the last Great War (1914-1918), or during the depression years, it is certain to upset the linearity of variance increase, without affecting the law for mean weights. Whether the tendency towards cruder striking of the coins with regard co weight is manifested in other countries and periods before great changes of structure will also have to be studied with this example in mind.

I am grateful to the kind friends who saved me much of the labour of gathering the samples in an unusually hot summer. Special thanks are due to my geological colleague Prof. K.V. Kelkar for going out of his way to place the facilities of his laboratory at my disposal.

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4 R. A. Fisher, *Statistical Methods for Research Workers* (7th ed.), ex. 42.
5 *Ibid.,* ex. 33
6 The gold sovereigns have had almost no circulation, but if just two more specimens, dated 1887, 1897 (and used regularly for worship) are added to the sample accepted, the correlation takes the very highly significant value of .64, with very highly significant deviations, from regression.

7

Silver Punchmarked Coins with Special Reference to the East Khandesh Hoard

THIS HOARD OF silver punchmarked coins was described in part by Mr. S. N. Chakravarty,[1] who divided the specimens into three classes according to thickness and described 218 out of the total of 690. The hoard then came to me for inspection, by courtesy of the council of the Bombay branch of the Royal Asiatic Society.[2] I weighed and examined all the coins, and found that only about 73 were too battered for proper classification. A few had been misclassified in the original study, but this is inevitable in view of the difficulty of reading the marks. My most serious differences with the author cited arise, however, in his conclusions as well as general method, so that this note is devoted principally to the general problem, and to the advantages of studying coins in hoards.

Mr. Chakravarty describes the hoard as of 685 pieces and 5 fragments. Of the latter, three are really halves cut from 'round' punch-marked coins, which is particularly interesting in that this type of dichotomy seems to have been practised even at Mohenjodaro;[3] it amounts, as nearly as can be ascertained, to a method of making small change by cutting a coin into two roughly equal pieces. The remaining two fragments, however, fit nicely together to form a complete 'square' coin which was not cut but has been broken by design or accident into the two pieces at hand. All the coins are of the 5-mark-obverse type, as correctly noted by Chakravarty, but his three classes are not mutually exclusive. Therefore, his conclusion that the three groups represented different denominations* is the most curious of the considerable number of conjectures that overload so brief a note. Among the most glaring of his oversights, we may note the omission of the rhinoceros mark, which occurs on several of his 'classified' coins with the six-armed symbol (which I shall hereafter call the *Sadaracakra)* labelled by him 1. *b,* so that Chakravarty's Group *A,* Class V is definitely misleading. In this connection, we may note that the mark is correctly given by Walsh[5] in his own group M, but with the wrong *Sadaracakra.*

Chakravarty's symbols Nos. 10 and 12 seem to need additions, while his No. 30 should have a taurine symbol in front giving it the appearance of a bovine creature with head lowered and turned. Nearly 50 coins with his mark 47 were among the 'unclassified' portion, which shows incidentally that coins of the same fabric could belong to more than one of his three major classes.

Among the 'unclassified' coins of the East Khandesh hoard were four that are particularly difficult to read but seem worth publishing (see Fig. 7. 1 at the end of this book). These are shown in Plates I - 4, where the unpractised reader is warned that the punched area sunk below the rest of the surface appears darkest. Occasionally, the eye sees this as raised, so that one is led to read the conjugate area as meaningless meanders.

None of" these have any reverse marks worth noting, beyond very light single pricks which might be fortuitous, except No. 3 which has a small irregular rectangle on the reverse. No. i on the plate is undoubtedly the most interesting coin of the hoard, and its last two marks (lower right and top left) of special interest, being a female figure with child supported on her left arm; and an archer (all facing right). The sun-symbol gives no special information, being universal. The *sadara-cakra* cannot be specially identified, as its points have been obliterated. The third mark may be a crossed square, or the bottom half of a 'tree-with-railing'. If the latter, this could belong to some Mauryan emperor in spite of the absence of the crescent-on-hill (arches) mark, which characterizes all Mauryan silver coins in the direct line. But the strikingly graceful outline, of the female figure on the fourth mark (lower right) is unique; the coin being among the heaviest in the hoard (though worn) is presumably among the latest. Conclusions set forth in greater detail elsewhere[6] make it highly likely thafthe hoard itself was of coins punched as late as to the end of the Mauryan period, and still in use till its deposit a century or two later.

The second coin (Plate 2) may have the first three marks in common with its predecessor, but the remaining cannot be identified at all, as far as my own knowledge of such material extends. The third coin, (Plate 3) has the unmistakable outline of the humped bull, and what looks like the fore-end of a galloping horse (lower left). The last mark, but in a far clearer impression, was to be seen in 1941 on a silver punch-marked coin of the Srinath Sah collection at Benares. The fourth coin (Plate 4) causes difficulties only because two of the marks overlap fully; it approximates No. 80 of Durga Prasad's list *(K,* plate 16), but with a crescent-on-arches.

The most serious objection to the type of analysis presented by numismatists like Mr. Chakravarty is not that new types escape classification in spite of the attention concentrated upon these marks, but that not a single one of the coins was weighed accurately. The marks themselves, from their regularity, are heraldic; there appears to be no serious chance of ascribing alphabetic equivalents to them, though the sigillary copper band described recently[7] seemed to many to offer such hopes. But it will not be contested that silver coins are intended to present, lor purposes of general circulation, a certain amount of the precious metal weighed according to the standard set at the time. Each person who handles the coin rubs off a certain amount of the metal, which, though infinitesimal for each transaction, will reduce the weight of the coin notably, if it has been long enough in circulation. The precise effect of the circulation, as can be shown,[8] amounts to a regular decrease of weight with the years of use, coupled with an increase of variation in the weights of individual specimens. This holds for modern as well as for ancient currency, as has been varified by weighing thousands of specimens of known period. Thus, if the coins can be classified by their marks or legends, at least the chronological order of the classes is determined by the inverse order of the average weight per group. But it is essential for this method to be applicable that all the coins be of comparable manufacture and belong to the same hoard or period, i.e., have the same group history as regards circulation except, of course, for the differences occasioned by varying dates of minting. The method, for example, does not apply to Allan's British Museum coin list[9] in which specimens are presented which bear the same mark but have been found at widely separated spots, sometimes on the surface. In India, particularly, the statistical numismatist has to be rather careful.

A silver coin of Menander was purchased in the open bazaar of Poona by Mr. S. A. Joglekar five years ago, and would probably have been passed off for current coin fifty years earlier.

One other factor limits the use of the statistical method in the study of hoards: the natural variation of weight among the specimens. To eliminate the effects of this, it is essential to deal with large numbers of coins for calculating the average weight of each group. Only then does the sampling error cease to cast doubt upon the result.

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8
Chronological Order of Punchmarked Coins - I

A RE-EXAMINATION OF THE OLDER TAXILA HOARD

MOST OF THE METAL COINS known to us are either struck from dies, or, in older days, cast in moulds. Punchmarked coins differ from both of these in that they are bits of metal on which several different marks are stamped, each with its own separate punch; and stamped in such irregular fashion that no mark appears complete on all the coins. One has, therefore, to establish these marks by careful comparison of iheir visible portions on several different coins of the same type. This has been done by scholars like Durga Prasad [i], Walsh [2], and Allan [3], none of whom succeeded in making any contribution to the chronology of the groups that they had established. The principles on which the chronological order of these coins may be determined have been set forth in previous communications [4, 5, 6] to which nothing of theoretical importance need be added. The method rests upon the fact, verifiable for modern [6] as well as ancient currencies of known dates, that the amount of wear of coins is directly proportional on the average to the time of circulation. This seems obvious; the difficulty lies in its application. There is variation in the weight of the coins at the minting and this is further increased by the handling. The loss of weight in time, therefore, is just a process over an aggregate which is the more accurately measurable, the greater the number of coins observed in each group. A single coin or half a dozen coins of one issue compared to the same number of another issue might not show the correct age-weight relationship; but for a hundred coins each the effect is much more certain. In dating ancient coins from their rate of wear, it is necessary to have all specimens from one single hoard; these coins must not have been severely damaged by burial and the effect, of cleaning; nor must they be selected in some particular way as for example for the clarity of their marks. It is necessary that the entire hoard presumably deposited by a random selection from the currency actually in circulation at the time be observed without further selection or without irregular, severe loss of weight by further handling.

The statistical procedure for the comparative study of coin groups is an adaptation of classical methods [4] which will not be re-explained here. It should be realized, however, that statistics by itself cannot group the coins; it is of use only in discrimination between the groups. This adds to the difficulty in dealing with punchmarked coins because the symbols are heraldic marks of unknown significance and there is no immediately obvious mark which corresponds to the date of issue on later coinage. It is essential to discuss the most reasonable methods of grouping first and then to see how these groups may be arranged in iheir chronological order by means of average weights. It is assumed that all the coin issues were mean t to be the same fixed amount of precious metal. An infinitesimal loss of weight occurs every time the coin is rubbed by handling. The longer the period of circulation, the greater the number of transactions in which the coin has figured and the greater the loss of weight.

There is no single unique system either of weights or of marks for all the coins under discussion. We must exclude for the present the 'minute' coins which served as small change, the long-bar (or more strictly the bent-bar) coins which were local currency in the Punjab and Frontier Provinces and the post-Mauryan systems which are to be observed in two unpublished hoards of the Madras Museum. These last, with the Paila Hoard at the Lucknow Museum, I hope to consider in some other note. We shall restrict ourselves here to the punchmarked coins generally found in the region from Taxila to Bihar and all based on a common standard. These coins have, with very few exceptions, 5 marks on the obverse and an irregular number of marks, or none, on the reverse. Of these 5 marks, one is a disc with (generally) 16 emanating rays which is often called the 'sun symbol' and is undoubtedly the *cakra* which we associate to this day with sovereignty. This being universal is to be ignored for the purposes of grouping. The next commonest sysmbol is some form of a wheel with 6 points or spokes which we agree to call the *Sadaracakra.* The remaining symbols present a considerable variety and offer the main difficulty in grouping.

One step in the right direction is taken by identifying one particular mark with the Mauryan period. This mark is that of a crescent on three arches. Accepting this identification, we note that the Mauryan coins on whjch it occurs are then associated with just one particular type of the *sadaracakra,* where the arrowlike points alternate with taurine symbols. This leads us to the belief that each type of the symbol is definitely associated with a dynasty. We have now accounted for three of the five symbols on the general issue of Mauryan coins. The question that arises is whether each 5-mark group represents one king, or whether some finer grouping should be carried out, or whether one may deny altogether that these marks are associated with kings. The last possibility is disposed of by the two *cakras* and also by some of the exceptional coins that we find on rare occasions. These exceptions have 5 symbols on the obverse of which atleast three are small 'homo' signs, i.e.,figures resembling human beings. These do not contain either *cakra,* the most plausible explanation being then of a tribal oligarchy or republic without the assumption of individual sovereignty. If each group of five marks were to be taken as associated with a single monarch, we should be led to the curious result that there are at least 60 Mauryan emperors and there is no possible authority for this in any historical reference. Contradictory as these records are in many details, the maximum number of Mauryan emperors cannot possibly exceed 10. Of these, the first three, namely Candragupta, Bindusara and Asoka had long reigns attested by the uniformity of all tradition. It follows, therefore, that we must look for further grouping in the remaining two of the five marks. On closer examination, it does turn out that of these two marks some are common to more than one group and some are individual types. We may for convenience call these the fourth and the fifth marks without necessarily asserting that such was the order in the stamping of these coins. Now, it is remarkable that the number of different fourth marks on Mauryan punchmarked coinage is about 9; this leads to the very plausible conclusion that *this fourth mark is the personal mudrd or signet of the king for his coins* while the fifth is that of the minister, issuing authority, or mint master.

The marks from the coins occur very rarely on inscriptions, usually at a much later period such as, for example, the Satavahanas who also imitated some of the coin marks. We have necessarily to proceed on the basis of logical consistency added to the plausibility of our conjectures.

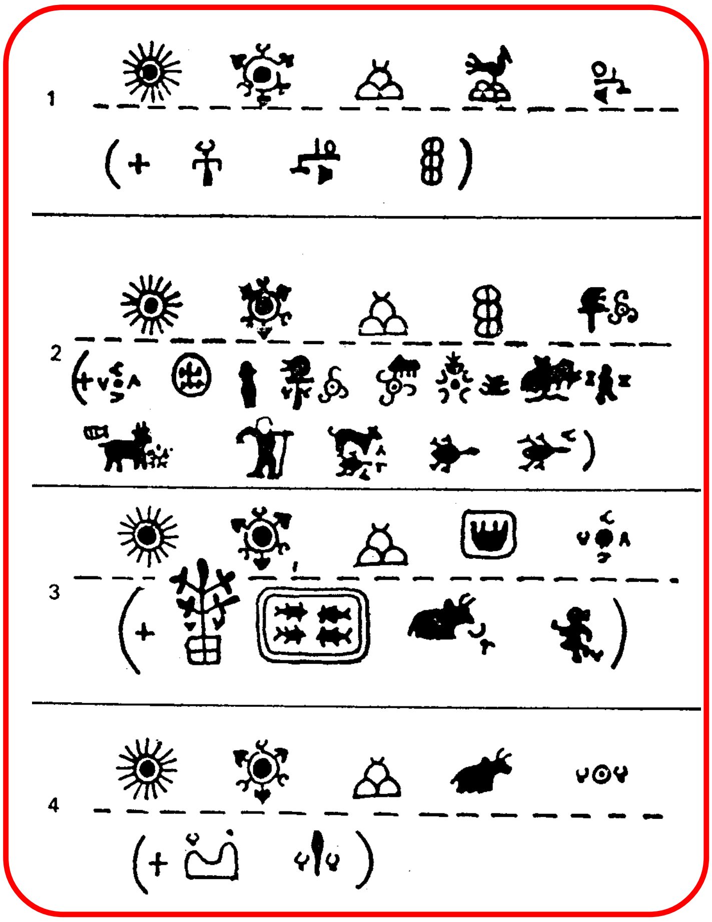


TABLE 8.1: Marks of Mauryan emperors after Candragupta. Additional fifth marks are shown below the dotted line in each frame. *Frame 1:* Bindusara (I.Nos. 105-107; 7.Class II.n'. *a-K). Frame2:* Asoka (7-Classes (i.Nos. 93-104). *Frames: ?* Dasratha (7.II.»ii»; i.Nos. 117-120). *Frame* 4: ? Samprati *(j.ll.ix-j-k;* i.Nos. 110-113).

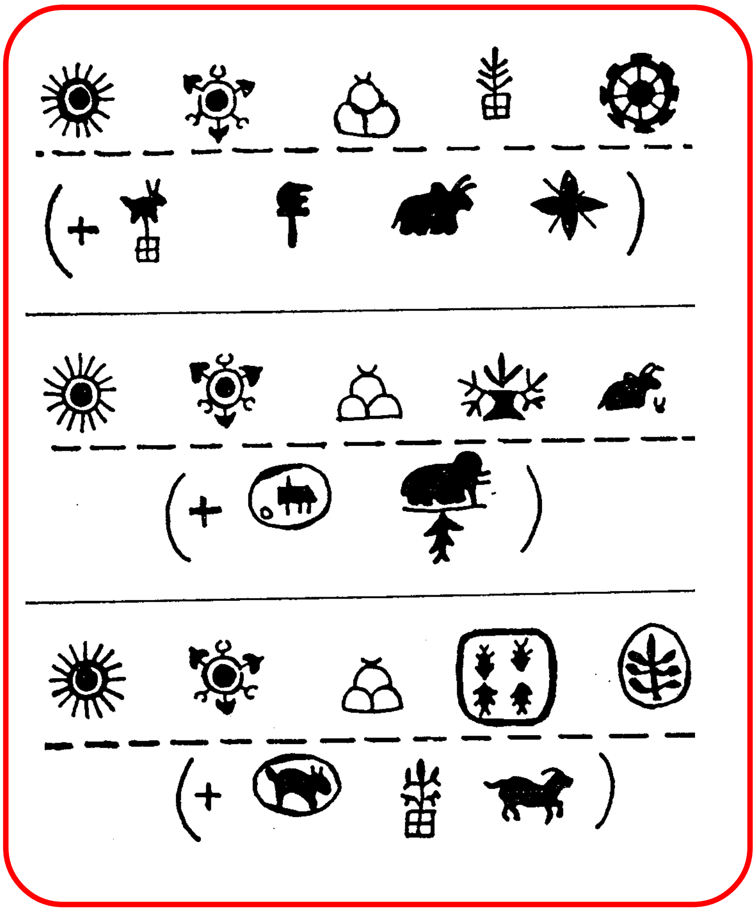


TABLE 8.2: Continued from Table 8.1. *Frame* 5: ? Salisuka (y.II.ix.a-e; Nos. 113-115; iai-182; see also 123-124). Frame 6: ? Devadharman (7.II.j.a-6; I.Nos. 128-130). *Frame* 7: Satadharman *(7II.x.b-f;* I. Nos. 108-109). Note: Durga Prasad's No. 131 has not been included here.

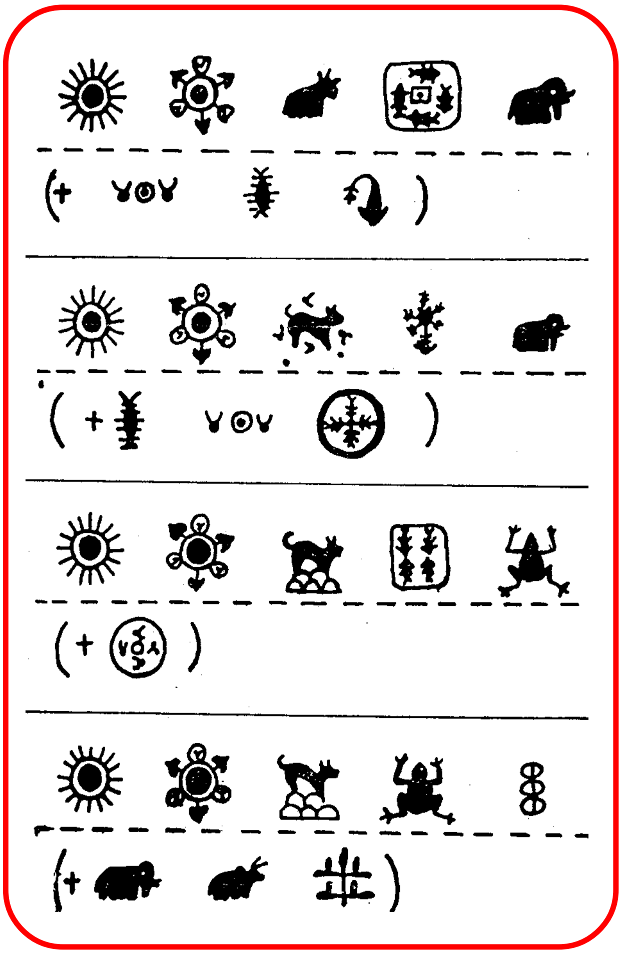


TABLE 8.3: Marks from Ajatsatru to Candragupta. Additional fifth marks are shown below the dotted line in each frame. *Frame* I: Ajatsatru (2.Class *M;* i.Nos. 11-15). Durga Prasad's No. 16 maybe a descendant. *Frame 2:* Susunaga (a.Classes *B.b* and *B.c;* i.Nos. 5,51,52,7,8). *Frame* 3: A Saisunaga (a. Classes *A.IQ* and *A.21;* i.No. 69). *Frame* 4: Kalasoka, son of A Saisunaga (a.Classes and 22; i.Nos. 65-68).

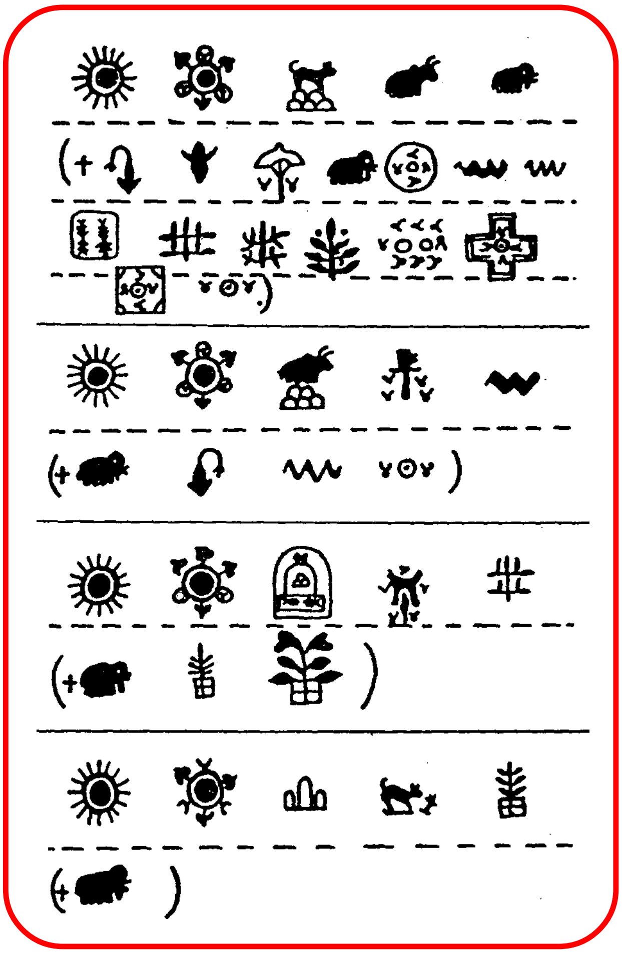


TABLE 8.4: Continued from Table 8.3 *Frame* 5: Nandivardhana (2. Classes *A.i* and 4.3-17; i.Nos. 53-64). *Frame* 6: Nandin [= Mahanandi=Nanda] (a.Class C; i.Nos. 45-47). *Frame* 7: Mahapadma (=nava Nanda] (2.Class *D;* i.Nos. 22-25). *Frame* 8: Candragupta Maurya (2.Class ***B.a;*** :.No.8g).

Now the basic conjecture about the Mauryan mark is very well supported by the earlier Taxila hoard [a]. This is roughly dated by a coin of Phillip Arrhidaios in mint condition which could not have been deposited at Taxila much after 317 B.C. The Hoard contains 1171 punch-marked coins of which not one has the Mauryan crescent-on-arches. About the fourth and fifth marks, we have further support and therewith a further added identification. Under the hypothesis of the preceding paragraphs, if the fourth mark be the signet of an emperor, then the emperor that ruled the longest in reasonably peaceful, stable, and prosperous times should presumably have the greatest number of different issues. For the Mauryan period, this means only one ruler, Asoka. His *mudra,* therefore, is the 'caducaeus', three ovals crossed by a line:

Now this mark is found on some of the coins in the earlier Taxila hoard, but there is always a clear distinction. The signet of Asoka has ovals that touch each other while the earlier has ovals which are not tangent. Corresponding to this is the hitherto unsolved riddle of the two Agokas which confuses all students of Buddhist records. Besides the great convert, there is an older 'Kalasoka' and this should be easily explicable if we remember that these older punchmarked coins were also current at the time Buddhist records were first written down. The people would be familiar with the signet of the great Asoka and also realize that there existed a far older emperor, whose name was lost in antiquity, who had precisely the same personal mark. Thus, Kalasoka is to be read as 'the ancient Asoka' and not as 'the black Asoka'. There is a further distinction between Mauryan and pre-Mauryan coins, the former being thicker, with more copper, while the latter are generally thinner and contain a greater proportion of silver. Statistically, I have proved [4] that the Mauryan coins are also much more crudely minted ; though the average weight was the same, the variation is much higher than that observed in the older Taxila hoard. In the mixed hoards deposited in finds of the Mauryan period, the contrast is quite obvious. There is another, less noticeable, distinguishing feature. The Mauryan coins have generally a single large mark on the reverse, while the earlier have an innumerable variety of reverse marks, to which a Mauryan addition might sometimes be stamped in the shape of the larger mark. The older reverse-mark system dies out during the Mauryan period.

The, greater debasement of the *karsapana* in the Mauryan period is attested by some late tradition, as for example by Dhammapala, commenting on the Mahavamso (Mhvs. 5.16 ff.), who ascribes it to the minister Canakya. Patanjali refers in passing to the cults established by the Mauryans for the sake of money (on Pan. 5.3.99). In fact, a vast territory had been opened up by the Mauryan conquest, which first brought the new trade and coinage to" the Indian peninsula. This suffices to account for the debasement actually found as a concomitant of the shortage of currency.

On the basis of the foregoing, it would have become comparatively easy to arrange the Mauryan coins in their chronological order if we had sizeable finds from a single hoard with accurate weight given for each coin. Unfortunately, the last condition is almost always neglected. The only evidence that might have helped comes from [7]. Even here, a selection has been made of the total number of coins, and the classification is not particularly intelligent. Worst of all is the removal [7, p. *iv of* the Introduction] of a copper coating supposedly made by the addition of molten copper to the original silver coin in order to raise the weight. This is not only a ridiculous assumption, for the normal procedure in plating is to use the more precious metal for the outer layer, but it is also extremely difficult to execute such plating with any accuracy. The fact of the matter is that electrolytic action due to centuries of burial *in* a damp soil has drawn the copper of the alloy to the surface; this does not seem to have been known to those who analysed the Purnea hoard; and their efforts, iherefore, have quite definitely damaged the evidence. We may, nevertheless, present a tentative chronological order as in Table 8.1. The five marks are followed in each case by varieties of the fifth mark, and the order is approximately that of weight. The last five kings are uncertain in order because the total number of coins in some cases is as low as seven. Comparison of the Puranic, Buddhist, and Jain records increases the uncertainty of nomenclature. The argument for identifying the most prolific coinage as that of Asoka has been given above. Bindusara is then identified by a lighter group of coins fairly large in numbers. The reasoning is further supported by the fact that Asoka's signet occurs as a fifth mark on some of Bindusara's coins. By mere comparison of marks, this might have signified at most a father-son relationship without saying which was which; that can only be said in the final analysis by comparison of weights for the two groups which is here perfectly clear. We know from the *Divyavadana* that Asoka was viceroy at Taxila during his father's lifetime. This type of relationship is also seen in other coin groups and is of considerable help in supporting our method. We further note that Bindusara's coins contain the peacock on five arches. Now the dynastic name is, strictly speaking, a Sanskritized form of *moriya* which means literally 'of the peacock' and the peacock-on-arches therefore must be regarded as a mark of origin or of a totem. The Jain encyclopaedia *Abhidhanarajendra* cites references giving a tradition that these kings originated in a *Mortya-grama* though in earlier Pali literature the only occurrence of the Mauryan name is of a tribe occupying the Pipphalivana. Thus, the crescent on three arches could signify a descent from the moon, which is also claimed by many Indian princelings to this day. In European heraldry, such arches are often taken to represent a mountain or a range of mountains. I may point out here that they could represent the Sanskrit *naka* which is the vault of heaven. Generally the expression is *tri-diva* which would necessitate three arches; the *Vajasaneyi Samhita, xvii.* 62 distinctly mentions five successive regions of the heavens and in the *Satapatha Brahmana, viii. 6. i nakasad* refers to the fifth layer of bricks in the fire-altar which thereby represents the home of the gods. The interpretation of five arches as *naka* is thus supported. It must be mentioned here that classification made by people like Walsh suggesting that these marks refer to areas where the coins were minted because peacocks or other animals were found on local mountains ii too childish even to be considered. As some of these marks go back to Mohenjodaro seals, and are also found described in tantric literature as symbols of mysterious potency, we may conclude, in view of the traditional usage of wearing certain types of coins as charms, that the marks possess some deep and mystic ritual significance.

The reasons for not starting Table 8. 1 with Candragupta will appear later. It must be understood that other Mauryan kings may have existed and issued coins which have not been included; my purpose in this note is merely to arrange the better known groups in their proper order. These can then serve as points of reference for future work. In addition to the imperial coins, the signets of these Mauryan emperors occur also on coins without a *cakra* but with homo signs. These are to be taken as tribal coins issued under the hegemony of the corresponding Mauryan ruler. The Mauryan fourth-mark *mudras* are also to be found on a parallel coinage, namely that of the type which appears at the very end of Table II, and which I ascribe to Gandragupta. The *sadaracakra* is generally identical with the Mauryan, sometimes the crescent-on-arches mark is also carried over, and the coins are clearly contemporary with the corresponding Mauryan coins because the spread in weight is just as much as the spread for the totality of Mauryan coins. In a few cases there is reason to believe that the *cakra* is slightly different (Walsh's 1.u) but this is not certain. I suggest the explanation as of a coinage begun by Candragupta and continued by his successors; the main imperial system is as depicted in Table 8.1.

We now come to the earlier coins which I study here from the Taxila hoard alone. The unique importance of this hoard was not realized before studies of several latter hoards showed much rougher minting, and much greater variation of weight due not only to crude manufacture but also to the stripping of encrustations and decuprified surfaces. My previous analysis could not go very far because Walsh's published data was full of errors and misprints which showed themselves as incompatibilities in his statements without making clear just what the correct statements ought have been. Fortunately, by courtesy of the Archaeological Survey of India, I had a chance of re-examining this hoard at Bombay in 1947. The coins have been somewhat disturbed in that several were missing from their original envelopes while 22 had been found without any envelope at all. Assigning these after considerable difficulty to their proper envelopes, there still appear to be some coins missing. Trusting Walsh's data and description in these few cases, a close examination of the remaining coins enabled me to assign many of Walsh's unidentified coins to their proper groups. Moreover, the weights of the coins were roughly checked at the Prince of Wales Museum's balances by Mr. W. Banavalkar; these weights generally tallied with the original weights entered on the envelopes of the coins themselves, enabling us to correct important misprints in the Memoir. My principal charge was in the counting of the reverse marks. Walsh had counted as proper reverse marks only those that appear on the reverse. But some of these appear also on the obverse, particularly among the older coins. Now previous work [4] has shown that these reverse marks were undoubtedly put on at regular intervals of time. For, the coins of this earlier period (in strong distinction to the Mauryan coins) are found with blank reverses, or with one, two, or more marks. The variety of these minute reverse marks is far greater than that of the obverse marks. Counting them regardless of the actual symbols, it was easily proved that the average loss of weight per reverse mark was quite regular, and moreover the number of coins per reverse mark decreased in a very regular geometric progression. This could not possibly have resulted from any other mechanism than a regular periodic check. In other words, these reverse marks by themselves would afford some indication as to the date of the coin. Unfortunately, this cannot be applied immediately for the simple reason that the obverse-mark system and the reverse-mark system appears in two different regions; the reverse mark system is probably used by traders, not kings. The evidence in support of this is that an earlier coin has been described by Durga Prasad (1, plate VII) with blank obverse and 13 reverse marks. The tradition of such minute secret 'shroff-marks', on tested bits of precious metal, which could be read only by members of an exclusive guild, continues in India to the present day; but so far as periodic testing of coinage is concerned, it dies out in the Mauryan period. Moreover, these reverse marks are also found on Persian sigloi which shows that they belong to the Frontier region.

In the pre-Mauryan period, the only royal authority which is strong enough to issue coins on a sufficiently large scale is unquestionably the expanding kingdom of Magadha. All records are uniformly silent about any other kingdom of comparable size at the time of Alexander. In fact, at about the time the Taxila hoard was deposited, Magadha also absorbed the little kingdom of Taxila, the conquest being facilitated by Alexander's destruction of petty tribal oligarchies which had hitherto formed buffer states. The bent-bar coins represent the common Frontier currency, so that the bulk of the Taxila hoard comes in the courses of, trade from Magadha. My recounting the totality of the reverse marks on each coin might have contradicted former conclusions. However, it turns out to support the older findings in a very satisfactory way. These coins are divided for convenience into two types: the square coins which were made by clipping a plate and rubbing down the piece very carefully to the standard weight; and the round coins which are flattened from a pellet, being somewhat less accurately minted than' the square though more regular in appearance. The square class is the more numerous and yields far more satisfactory statistics because of its accuracy of minting. It now turns out that the loss of weight per reverse mark is almost exactly one-fifth of a grain on the average. Moreover, for the square coins, the linear regression explains virtually all the loss of weight. It must again be emphasized that it would be quite impossible for an ancient money changer to measure such a loss of weight on his scales and then to allow for it by punching a reverse mark on the coin; this is seen immediately from the considerable overlapping in weight that we observe between any two groups of coins. A further support for my thesis that the two systems belong to different regions may be derived from separating these coins into groups by obverse marks as was done for the Mauryan period. In each group, even in the oldest, we seem to get coins without any reverse mark, at all. Now periodic checking, had it been over the entire region of circulation of these coins, would have made it extremely difficult to find any older group of coins with blank reverse. The coefficient of absorption is not the same or is the loss of weight identical for each individual obverse group. It is easily seen that if a king died or for some other reason stopped issuing coins at Patali-putra his coins would continue to reach Taxila for a considerable number of years afterwards and would then still be with blank reverse while their weight would be lower than later coins, thus showing a lower loss of weight per reverse marks. I used this fact to compare all obverse groups with blank reverses and was then able to arrange in chronological order four major groups: Walsh's *A.I, C, D,* and *B* (e)2. The inaccuracy of the data did not justify any further refinements at that time.

We now have two methods for daring, namely average age in reverse marks and also average weight. The former is less accurate because the oldest coins tend to disappear more rapidly in circulation. We have already seen that the system of reverse marks was not universal, whereas loss of weight by circulation is independent of any system of marking. Moreover, we do not know what period should be assigned to a reverse mark; but the existence of a very old 12-year cycle throughout East Asia inclines me to take that as the most plausible period.

There are not less than eight prominent kings represented in-the hoard; with coins having as many as 20 reverse marks. The lustrum of four or five years would give at most 80-100 years for this hoard and that seems decidedly too short both from what is known generally of longer imperial reigns. No calculation of the reverse mark period is possible from modern coins because loss of weight depends both upon the alloy and the rate of circulation, the latter depending essentially upon the total amount of available currency. We have no information on this score for the coins under consideration. It must be emphasized that mere random shroff-marking would not suffice to account for all the observed features of the coins, particularly loss of weight and reduction in number, without periodicity in time. The correlation coefficient for reverse marks against weight in the Taxila hoard is 0.46, i.e., the same as for British India Rupee dates against the weight of the rupees, as was found by my weighings in 1940-1941.

Having arranged the major coin-groups by weight, the minor coin-groups can to a considerable extent be assigned their proper position. The difficulty lies with the precise identification. Starting from the bottom of Table II, it can be seen that Candragupta's is a reasonably safe identification, though I was not able to make it previously. In the first place, all the coins of that group with a single exception have blank reverses, the exception having one mark according to Walsh. But on my own re-examination this 'reverse mark' is only a misapprehension on the part of Walsh; the coin is actually the heaviest in the entire hoard. It may be pointed out here that some coins show a peculiar type of *raised* mark on the reverse. This might have been due to 'ghosts' raised by heavy stamping on the other face, or to a blow received when in contact with some other coin. Generally, they do not show in the photographs published. To revert to the Gandragupta coinage which Walsh has labelled *B.e. 1.2,* I have already remarked that the characteristic marks of the three parallel arches, the central one being higher than the other two, and of the animal with young are continued with Mauryan signets right through the succeeding age. Since the Mauryans traditionally wiped out their predecessors, the Nandas, it follows that this coinage can only be associated with the Mauryans. Walsh's *D* is an immediate predecessor because no other group is seen to intervene, on calculating average weight as well as average number of reverse marks. This class *D* can certainly be ascribed to Mahapadma Nanda, and the legend of the 9 Nandas is then to be explained after the Jain tradition by taking *nava* to be' new'. Before Mahapadma we have a king whose mark of descent is that of a bull on 5 arches. Among these earlier Taxila coins the elephant mark is common to almost all the rulers so that it must have had some special significance, perhaps the principal issue or the first issue of each particular king. The *cakra* of this king Nandin is common to several of his predecessors and is therefore presumably of the same or a closely related dynasty. Now these predecessors claim descent from an animal which is not a bull. Their common mark, called by Walsh 'hare-hill area' is not of a hare (because of the curly tail) but of a dog or a frisking puppy on 5 arches. This seems to me to be the oldest such mark known, and I am tempted to read in it the hieroglyph *sisu* (the pup) plus *naka* (the arches of heaven) equal to *sisunaka* which is one variant of the name *sisunaga* (which also means earthworm in Jain Sanskrit) in our records. There seems to be no immediate interval between the last of these and Nandin whose fifth marks are also common to the preceding. But the last of these has as his own personal mark the bull and seems to be the puranic Nandivardhana. He is a ruler comparable in numismatic prosperity to Asoka himself for his coins are by far the biggest group in the present hoard while being one of the biggest in almost all the hoards laid down even in Mauryan times. The biggest single 5-mark group is of the elephant mark associated with the bull; according to my explanation above, the principal coinage of this particular ruler. This is Walsh's A. I. A king with the long reign implied by over a dozen other issues is unlikely to be succeeded by his son, and therefore his successor (possibly a grandson), has some right to claim descent from a Nandi. Of the further predecessors Kalasaka has already been explained before on the basis of the caducaeus *mudras.* This brings us to Sisunaga himself, Walsh's *B. b.* and *B. c,* who has no mark on arches. There is a chance of several other little kings coming at about this time, but the matter cannot be cleared up effectively without further evidence. The position of Walsh's *A.* 23, a Saisunaga with tree and elephant, is doubtful. The remarkable thing about Sisunaga is that he has groups of coins with at least two different types of *cakras.* Moreover, his obverse marks appear on the so-called double-obverse coins made by counterstriking older coins of previous rulers. In later times, we see exactly this phenomenon, as for example in the Joghaltembhi hoard, where coins of Nahapana appear counterstruck by the obverse marks of his conqueror Satakarni. Sisunaga having counterstruck so many coins shows certain political disturbance and .it is this that has led to his identification, for the Buddhist records definitely say that the fifth ruler after Ajatasatru was deposed by the people, and his *amalya* Susunaga put on the throne by the people. The fact of a sudden change is certainly well supported by our coins. This counter-striking, as well as wear, has obscured the coins of the predecessors of 'Susunaga'. I might also emphasize that there is always the possibility of some of his successors with short reigns not having issued any other coins to be discovered in this particular hoard. Incidentally, we further see that subsidiary coinages might on occasion be issued by or under the suzerainty of a king with a *cakra* distinct from his own principal *cakra.* This is to be seen also in the Mauryan coins of East Khandesh hoard; and the Taxila hoard, coinages *J, K, L,G.* 1.2 and the last two are feudatories *of A;J.* 1.2 of *C.* In this connection, one may recall that though Anga and Magadha were originally two distinct countries, they had a joint name Anga-Magadha at the time of the Buddha while the same is happening for the already hyphenated kingdoms of Kasi-Kosala where we hear of no king after Pasenadi's son, the usurper Vidudabha.

This leaves us then with just the first coinage on the list which I am forced to assign to Ajatasatru. It may be objected that at his time there were other kingdoms in existence which may also have issued coins. But as already noted Magadhan expansion was complete well before Alexander and it is known that it was Ajatasatru himself who was its principal agent. In his days, we hear of no other powerful king except that of Avanti, who is too distant and too legendary to be considered seriously. The one great power surviving at that time was the Licchavi-Vajji oligarchic federation, which could not issue coins with a sovereign's *cakra.* Moreover, we know that it was Ajatasatru who finally brought these tribes under his absolute rule. Possibly, coins of the Paila type might represent the coinage of Kosala, as the system of weight is three-fourths that of the general *karsapana,* the obverse system being of four marks in place of five. One rather faint support for the identification of Ajatasatru might be seen in one of his personal marks, the rhinoceros. His name in Jain records is Kunika and kuriika means the horn of an animal in Sanskrit; whether the name suggested the horned beast or vice-versa is not clear. The *cakra* contains three trefoils (not ovals as reported by Walsh) which may be blank, or with a dot, or a taurine.

In conclusion, we may note that the actual weight standard at the time of issue of these *karsapanas* is determined by that of the freshest group of the hoard, namely the one which I ascribe to Candragupta. From the 18 square coins of this group, it would be seen that the precise weight of issue is 54.18 grains on the average and this may be taken as established beyond any doubt, whether or not the identification of the coinage with the name of Candragupta be accepted.

NOTES AND REFERENCES

1 Durga Prasad, 'Classification and Significance of the Symbols on the Silver Punch-marked Coins of Ancient India', *Journal and Proceedings of the Asiatic Society of Bengal, New Series,* Vol. XXX, 1934 (Numismatic number). See also Durga Prasad, *J.A.S.B.,* 1935, Num. Supplement 45.

2 E.H.C. Walsh, *Punch-marked coins from Taxila,* Memoirs of the Archaeological Survey of India, No. 59; Delhi and Calcutta, 1939. For numerical inaccuracies, see my note in the *Mew Indian Antiquary* 1940, III, pp. 15-18.

3 John Allan, *Catalogue of the Coins of Ancient India* (British Museum), London, 1936.

4 D. D. Kosambi, 'The Study and Metrology of Silver Punch-marked Coins', *New Indian Antiquary* 1941, IV Nos. 1-2. My arguments will be found there with full documentation at greater length.

5 D. D. Kosambi, 'On the Origin and Development of Silver Coinage in India', *Current Science,* 1941, X, pp. 395-400.

6 D. D. Kosambi, 'The Effect of Circulation upon Metallic Currency', *Current Science,* 1942, XI, pp. 227-230.

7 P. N. Bhattacharyya, *A Hoard of Silver Punch-marked Coins from Pumea,* Memoirs of Archaeological Survey of India, No. 62, 1940.

Statistical Note: Dealing with the revised data for the earlier Taxila hoard, we find for square coins alone, the following: 18 coins of class *B.e.z,* average weight 54.10 gr.; 118 of *D,* 53-77 gr.; 79 of class 0,52.78 gr.; 38 of class *M,* 50.80 gr.; 44 double-obverse, 49.37. For 385 of type *A,* the mean weight is the same as for *C,* whence separation into subclasses and more refined arguments must be used. The variance for the 18 *B.e.z* square coins, in grains, is 0.1779 which shows very fine workmanship, lost in Mauryan times. Finally, the regression calculated from 769 square coins gives a loss of weight of 0.1999469 grains per mark, and deviations from linearity as measured by analysis of variance are no longer serious. Coins no. 114, 179, 269, 558, 818 are too light, probably having been clipped or damaged in antiquity. Discarding these increases the loss of weight slightly, so that 0.2 grains per mark is not an excessive estimate. Thus, the oldest coins of this hoard are 25 reverse marks old, and at 12 years per reverse mark, go back to 600 B.C. or earlier. Coins with at least 20 reverse marks (counting those on both sides) are actually found.

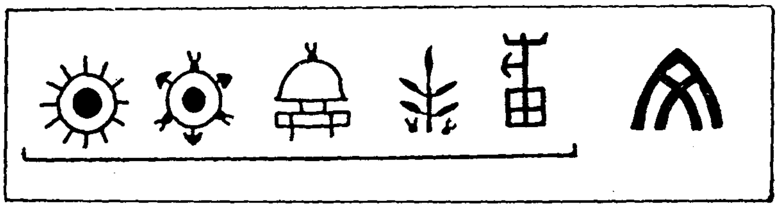
9
Chronological Order of Punchmarked Coins - II

THE BODENAYAKANUR HOARD

TWENTYFIVE COINS of this hoard were sent to me nine years ago for preliminary examination. They had not been cleaned, but it was immediately evident that the marks as well as the weight (1.47 grams) belonged to a new system of punchmarked coinage. The entire hoard was sent to Bombay in 1950, by courtesy of the Madras State authorities, and of Dr. A. Aiyappan, Curator of the Madras Museum. The total number amounts to 1138 silver pieces, of which 500 had been cleaned at the Museum, the rest by me at Bombay. The process consisted of soaking overnight (or longer if necessary) in a 10 per cent solution of formic acid, washing in pure water, and scrubbing carefully with a soft tooth-brush. The Museum chemist's cleaning was more thorough than mine, and he coated the cleaned specimens with celluloid varnish, which we could not get at Bombay. My chief care was to remove the encrustation, but therewith as little of the original coin as possible; so, the pieces cleaned by me show an occasional thin film of copper in parts. The coins themselves are silver alloy. Centuries of immersion in a damp soil pulls out, by electrolytic action, some copper of the alloy to the surface. This further reacts with salts in the surrounding medium to form the encrustations; but occasionally, the encrustation does not penetrate through the whole layer of decuprification. The presence of copper can be verified in the crystals formed upon evaporation of the cleansing bath; it is to be hoped that the Museum authorities will determine the exact nature of the alloy by sample analysis.

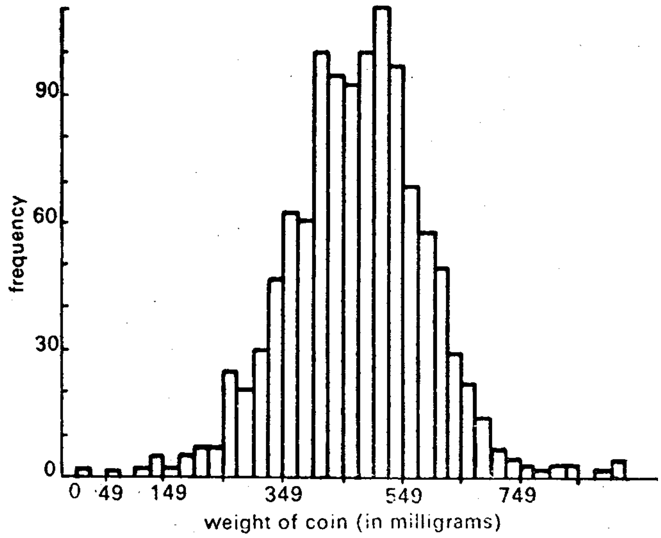
Of the 1138 pieces, 13 are blank square bits about 0.5" on a side. These are all underweight when compared to the stamped pieces, the heaviest being 1.037 grams, the lightest 0.610 grams, with an average of 0.8655 grns. The entire group is incompatible, by any modern statistical test, with the remainder. Nevertheless, these do not form a separate weight system or type of coin, but show how the coins were minted. That is, the pieces were first cut out of a flat silver plate to the approximate weight, stamped, and trimmed to a more precise weight. No underweight pieces were stamped at all. The lightest punchmarked coin of the hoard was 0.019 gm and its exceptionally low weight is due in all probability to flaking. The punches themselves show excellent workmanship and beautiful design, so that the crude first appearance of the coins is due to the final trimming. The mint seems to have been unable to produce silver plates of uniform thickness, but the coiners did have sufficient practice to vary the size of the first pieces in such a way as to minimize the number of rejections. These remarks apply to all hoards of punchmarked coins that I have seen. The one noticeable difference here is that the standard coins are all of the 'square' type, whereas the Taxila, the East Khandesh, the Paila, and other hoards of comparable size always contain a proportion of 'round' coins.

Just one coin is of the Mauryan period, as is seen by the first paper in this series *(JBBRAS,* 24-25, 1948-9, pp. 33-47). This lone specimen shows the Mauryan dynastic *Sadaracakra* and the characteristic Mauryan crescent on three arches quite distinctly, though it is otherwise too worn for any identification of the two marks that would determine its group. The weight is 2.99 grams, which brings it near to the standard of such coins found generally in the south, though these heavier coins are of northern fabric. Examples may be seen in the Hyderabad Museum collection. The question still remains open whether the lower weight is due merely to wear by circulation, or to the pre-existence of a lower standard in the south. I incline to the former view. The northern issue weight was close to 3.5 gm. so that most of the loss of weight was due to other causes than decuprification and cleaning.



The remaining 1124 coins of the hoard belong to just one major type with five obverse and one reverse mark, as in Fig. 9.1. It will be seen that there is considerable resemblance, as regard the actual marks, with Mauryan coinage. The sun symbol differs only in having 12 rays as against the 16 on northern coins, both Mauryan and pre-Mauryan— with 20 possible on some in the north. The *sadaracakra* is unquestionably Mauryan. The third mark is a stupa-like symbol which can be derived from the Mauryan crescent on arches. The third differs in the absence of a railing—though even this is not uncommon in the north— and the peculiar orientation of the two taurines. The fifth mark differs from anything else known of the type, and in this hoard occurs in two varieties, namely one where the crescent-axe projection is to the right, thus forming a mirror-image of the other. The proportion of this variant type is not less than 5 per cent nor more than 15 per cent at a rough guess, which is all that is possible because it is very rare to find even one of the five marks complete on any one coin. Finally, the solitary reverse mark is again reminiscent of Mauryan practice, for the older system of issue with blank reverse and regular (in time) punching of many different reverse marks vanished with the Mauryan coinage.

Taking all these points into consideration, I offer the suggestion that the hoard represents coinage of one of those late 'Mauryan' kings whose local existence in the peninsula is known as late as a thousand years after the imperial dynasty had vanished in the north. Unless the king claimed descent from or at least derived his authority from these Mauryans, there is no reason to preserve the Mauryan *cakra.* For that matter, we know that the loss of weight at Taxila was 1.5 grain per reverse mark, presumably per 12 years, which would make the single Mauryan coin found in the hoard about 400 years or more old; the guess is rather dangerous, made for lack of a better. The punchmarked coins had long gone out of fashion by then, having been replaced by cast coins. Therefore, the very fact that this hoard was punchmarked, and that the one stray found in it is Mauryan seems to speak for my conjecture. The actual deposit of the hoard could not be before the and century A.D., and could easily be as late as the 4th century. There are no field-notes of the find available which would enable us to verify or refute this.



Statistical analysis of the weights, when the whole material consists of a single class, would not be expected to give any special results. Nevertheless, one further fact emerges: the hoard is a mixture of coins made in two approximately equal but separate lots. That is, though all the coins seem to be more or less in mint condition, they were either not minted at the same time or not at the same place, or at least not weighed against the same weight. Had the process been uniform, one would expect the weight distribution to have the familiar normal (Gaussian) bell-shaped curve. A look at the actual distribution shows (Fig. 9.2) two peaks (bimodality) instead of just one. The exact statistics, in technical language, are: mean= 1.4728 gm. which is new, for the northern issue weight was approximately 3.5 gm. and even allowing for the loss by decuprification and cleaning, this weight is too low for the *karsapana* standard. The variance in milligram units is 13477.5, so that both the heaviest blank piece of the hoard and the *half-karsapana* lie well outside the 5 per cent fiducial interval, for a single coin; .one must remember that the variance of the mean will be the above variance divided by 1124.5 so that the estimate of the mean is very sharply separated from any possibility of being any plausible fraction of the northern *karsapana.* The departure from normality is measured by g1=0.0181, g2=0.998, with their standard deviations 0.073 and 0. 146 respectively. Thus the first of these is not significant, and the weight distribution is virtually symmetrical about the mean; the second, however, is very highly significant and shows that the curve could not possibly be normal, nor of the usual mixed type with coins of different ages (as in the earlier Taxila hoard), for in the latter case the distribution would have been skew-negative and platykurtic. The only explanation that I can think of for the dimodal leptokurtic distribution observed is that given above. The small percentage of variant fifth-mark coins would not account for the difference, and in fact none was observed at the time of weighing. The difference between my cleaning process and that at Madras would also not account for the observed distribution, being by actual modern statistical tests quite insignificant both as to mean and variance.

The coins were weighed one by one to the nearest milligram, on a differential torsion balance specially recommended by Messers Oertling of London, the manufacturers. This had to be serviced constantly, and developed a backlash of about three milligrams; I cannot recommend it to numismatists even when many coins of the same type are to be weighed, as the pans do not allow rapid handling of the coins. Our grouping interval of 25 milligrams takes care of the error (which was minimized by constant checking), and loses less than 1 per cent of the total information. My special thanks are due to the authorities of the Tata Institute of Fundamental Research for purchasing the balance and giving workshop facilities. The calculations were made by Mr. S. Raghavachari of the Institute.

10
Chronological Order of Punchmarked Coins-III

THE PAILA HOARD

THE HOARD originally consisted of 1245 silver punchmarked coins found at the village of Paila in the Kheri district of U.P. in 1912 (misprinted 1922, W. 15). Of these, 1014 are now deposited in the Lucknow Museum. The group is in its own way unique and important because the coins have an unusual weight-standard, are punched on a rare system of four (instead of the far commoner five) obverse marks, have a reverse mark system which is undoubtedly shared by pre-Mauryan coins found at Taxila; and finally because of the large numbers which make an accurate statistical analysis possible.

1 PRESENT STATE OF THE HOARD

The coins are now kept in the U.P. State Museum at Lucknow. They were sent on loan to the Tata Institute of Fundamental Research at Bombay, by order of Sri (formerly Sir) Homi P. Mody, then Governor, of Uttara Pradesa (formerly the United Provinces), for accurate weighing and close study. His kind intervention, for which I am particularly grateful, ended ten years of futile correspondence with others which had rarely succeeded in getting so much as an answer. The coins were sent in two separate lots, with a typed description which seems to be identicalwith the list published on pages 39-78 in a paper by E.H.G. Walsh (W, J. JVam. *Soc. Ind.,* 2, 1940, 15-78). Walsh completed his examination of the original coins in 1928, his report being made to the museum at that date (W. 17) though his name does not appear in the copy sentto me, nor in any of the letters received from the Museum authorities till the time of writing (December 1951).

Walsh states that he returned the coins to Lucknow in 1928 “with each coin placed in a separate envelope, with the Glass, Obverse Marks, Reverse Marks, Weights, and other particulars noted on them” (W. 17). As received at Bombay, however, some envelopes contained more than one coin, though in such cases the descriptions agreed, being generally coins with the same obverse and blank reverse; on such envelopes, just one weight is entered, apparently a crude average for the coins. The minimum unit for the weighing seems to be a quarter of a grain and only 436 of the coins had been weighed (W. 27). None of the weights given are reliable even when the envelope contains just one coin so that reweighing would-have been necessary in any case. But there is one very serious additional reproach that has to be made; in spite of Walsh’s careful work on the coins, and in spite of the six months taken at the Lucknow museum to make an inventory, the coins do not always match the descriptions on the containing envelopes. The most probable explanation is that some of the coins have not been put back into their proper containers; this seems all the more likely because the coins displaced are generally those which were set aside for making casts, or for the excellent photographs published by Walsh in the last three plates of his paper. This also suggests that the interchange was completed at the time the coins were finally packed for return to Lucknow.

In most cases, the coin can be identified by means of the descriptions on its own or some other envelope, but one misplacement is serious. This refers to the coin now under No.1005, the last envelope of the collection. The official description is "defective coin, obverse surface, flaked off'. Only mark 2a (Taurine in a shield) identifiable". The weight as printed is 37.5 grains, as entered on the envelope 27.25 grains. But the actual specimen is 'round' and the heaviest coin in the hoard. It should have weighed 55 or 56 grains on Walsh's balances for it weighs 3.618 grams on mine. No coin of the entire hoard is given as having this weight, though the piece now in 983 fits the description on envelope 1005. It would be of the utmost importance to know whether this specimen (whose marks are unidentifiable) really belongs to the Paila hoard, or is a stray intruder by exchange during the period of the hoard's reappearance; it looks much the same as the rest.

Still worse is the fact that only the few specimens set apart for special examination, casts, and photographs have been carefully cleaned, to the extent of presenting a burnished appearance, though the surface in some cases shows (under magnification) regular scratches that one can only ascribe to the cleaners. The rest of the coins have either not been cleaned at all or cleaned in a perfunctory manner. For, they are so blackened as to soil the hand, and many show encrustation in small decuprified patches, some of which may conceal reverse marks. This lack of uniformity in cleaning wastes a great deal of the available statistical information. The heavy penalties agreed upon for tampering with the coins made it impossible to risk cleaning the specimens at Bombay. I weighed each coin separately on (Oertling) chainomatic analytic balances, to *0.1 of* a milligram, and entered the weight as well as my description of the coin upon its own index card. After that, the coin was replaced in the envelope wherein it was received at Bombay, unchanged except for dirt rubbed off in handling, and returned to Lucknow. These painful details are given only as warning to any successor who may attempt a further examination; another necessary warning is that Walsh's table A cannot be trusted.

2 RESULTS OF STATISTICAL ANALYSIS

The main purpose of the careful weighing of every coin is to determine the chronological order.of the major groups. My hypotnesis is that the hoard was deposited at one time, from coins in actual circulation at that time; the loss of weight would *ceteris paribus* tell us the relative age of the groups. Walsh differs as regards this main assumption (W. 24), apparently treating this hoard, perhaps hoards in general, as "rather in the nature of a family bank in which deposits were made from time to time and may have been made for successive generations, and from which money would be withdrawn as it was required, and that was not the order in which it was deposited in the hoard". The statement seems unjustified here, seeing that the Paila hoard was contained in an *earthen* pot, (W. 15), which could hardly last over many generations of deposit and withdrawal. It seems to me that such hoards were mostly buried at the time of some impending catastrophe to the man in possession; flights due to invasion by a foreign army and perhaps to epidemics would be major causes tor burial and non-recovery.

The weight will not be exactly the same even for two coins of the same date of issue, nor the loss due to circulation identical from year to year for any two coins. However, the effect on the average of *groups* is quite marked, and a good indication of chronological order, provided the coins have not suffered unusual damage. The important point. here is *that the reverse marks, as for the Taxilan pre-Mauryan coins, represent a system nearly as regular in time though independent of the obverse.* We take the weights of coins individually (discarding damaged or doubtful specimens), tabulate the coins according to the number of reverse marks, and try to see how much of the weight variation is explained by a regular fall against each additional reverse mark; this is the 'linear regression'. There will be some unexplained variation left over, some of which is due to differences at the time of minting and to other unknown effects, the rest being ascribed to deviations from regression. The results are summarized in technical language in Table 10.1.

TABLE 10.1 ANALYSIS OF VARIANCE

Source	*d.f.*	*sum sq.*	*mean sq.*	*F.*	*reg. coeff.*
A: (all coins less 24 damaged)					
regression	1	75903		7.26**	—4.37
deviation	10	138384	13838.4	1.32	
residual	937	9797559	10456.3		
total	948	10011846	10561.0		
B: (type *a* coins, groups I-III)					
regression	1	24375		2.42	—4.14
deviation	8	143036	17879.6	1.77	
residual	445	4483808	10076.0		
total	454	4651219	10245.0		
C: (type *b* coins, groups I-V)					
regression	1	94822		8.98*	—6.98
deviation	9	162924	18012.7	1.71	
residual	407	4298321	10561.0		
total	417	4556067	10925.8		

This tells us that the reverse marks are in all probability regular in time but that they behave differently for groups *a* and *b*. This division occurs only in groups, I, II, III, *a* being that lot where the elephant faces right, and *b* to the left. It is a feature of the Paila coins that every major class has animals facing left, whereas elsewhere the animals almost invariably face right. Now the *a* division, which does not occur in all classes, is decidedly less regular with respect to the reverse marks, and the coins (particularly *Ia* with blank reverse) have been roughly handled in antiquity, so that the variation within Ia-blank reverse isnot normal (Gaussian), as would otherwise be expected. Some seventeen coins at least have to be discarded as being far underweight with respect to the average and norm of variation for the subgroup. As this procedure is rather dangerous, it would be safer to omit division *a* altogether when comparing averages. The reason for this unevenness seems to be *that the trade guilds responsible for the reverse marks operated with greater frequency in the region of issue of left-facing Paila coins, than of the right-facing.*

We now have three criteria for chronological order, namely average weight, average number of reverse marks, and maximum number of reverse marks. But for the irregular flaking and encrustation observed, the first would be the surest guide for such large groups; here we cannot take it unsupported. The average number of reverse marks is suspect at least in group *a,* as indicated. Finally, the maximum number of reverse marks is a good indication where the groups are large in the number of representatives, but not to be taken by itself where just a couple of coins survive, by accidents of sampling. In that case, the average number of reverse marks would be somewhat better. The average weight is again not sufficient, inasmuch as worn specimens tend to disappear rapidly from circulation, so that the survivors of old issues are usually far above their proper average weight, and often show less than the due maximum number of reverse marks. My criterion is a linear index figure, calculated by standard methods, from both weight and reverse marks.

3 THE CHRONOLOGICAL ORDER OF MAJOR GROUPS

By the criteria of the previous section, the chronological order is to be settled with the aid of another table:

TABLE 10.2 STATISTICS BY GROUPS (DIVISION *b* ONLY)

group	*no. coins*	*wt. (mgm)*	*variance*	*max. r.m.*	*mean r.m.*	$\sqrt{r.m.+0.5}$	*index*	*b-index*
I	211	2614.7	13037.0	7	1.9	1.4	2.0	2.9
II	128	2665.7	6796.1	8	3.3	1.8	2.5	3.4
III	53	2686.8	7811.9	2	0.2	0.9	1.6	2.4
IV	17	2673.4	9153.7	5	2.3	1.6	2.2	3.1
V	9	2609.2	5181.4	12	6.1	2.5	3.1	3.7

(If $x=\sqrt{r.m.+0.5}$, $y+2$=weight in grams, the index is $x+2.3y$; *b*-index $x+y$, approximately. Data for the full index is not given here. All calculations by Mr. S. Raghavachari.)

Here, only the first five groups, which are the only ones having enough members for trustworthy statistics, can be included. The chronological order I get is then (see Fig. 10.1)

X-XII, IX; VI, VII-VIII, V; II, IV, I, III.
The order preferred by Walsh is (W. 23):
IX-XIII.V, II, VII, VI, VIII, IV, I, III.

It is surprising to find that we agree in the relative order of I-V. Walsh's criterion seems to be the *maximum* number of reverse marks on any coin of a group, though he contradicts hinvelf in the actual arrangement; mine is the index-figure criterion. Now IX differs from X-XIII in having a wheel quite clearly marked, whereas the rest are older, with some sort of a whorl. Also, IX is represented mostly by double obverse coins, which I take to denote a violent change of dynasty, the classic historical model being the counterstriking of Nahapana's coins by his conqueror Satakarni in the Joghaltembhi hoard. So, the change occurred soon after IX reigned, whereas X-XIII were of the longer past than IX, whether those four groups represent one king or more. Now VI is the most frequent counterstriking authority, so we can logically place him in the next dynasty, plausibly as its founder, under the assumption that these successors had themselves no other coins before the conquest that are here represented. The change is also proved by changes in the obverse marks, the taurine symbol hereafter being enclosed in a shield. As for VII and VIII, it is not clear to me that they are two different coinages, not just two varieties of the same coin. I place them just after VI. The index worked out for groups I-V when applied to the two coins of VII-VII places them earlier than IX, but this means nothing as there are just two coins, both unusually heavy.

The remaining five groups are in a totally different category, having enough representatives each for statistical treatment. In counting reverse marks, I generally prefer Walsh's classification to my own, because of his greater concentration upon their characteristics, and should have accepted it without question but for unmistakable evidence that the coins have been mixed. I have taken the data from my own observation. Group I is the largest of all, and would have been easy to place, but for the damage to the coins; one may conclude that the king had a long and prosperous but nevertheless somewhat disturbed reign. No admissible number of discards from this group will make the average weight heavier than for IV.

One bit of evidence for regularity of the reverse marks is lacking altogether in the Paila hoard, in great contrast to the earlier Taxilan hoard. This is the regular absorption per reverse mark, which we do *not* find at Paila, whether by my own count or by Walsh's. The regularity is upset by the intrusion of group *a,* and does not exist even in *b,* the issues seeming to be quite uneven. My explanation is that the traders who put the reverse marks on the coins were nearer to the issue territory (in the sense of regular communication by trade-routes, not air-line distance On the map) of *b* than of a, but further away from both than for Taxila. This is again proved by the reverse marks here being of much the same type as at Taxila, with possibly a slightly larger fabric for the marks themselves. Moreover, such marks occur even on Persian sigloi. On the'other hand, the Paila obverse marks have not hitherto been found away from the territory of ancient Kosala.

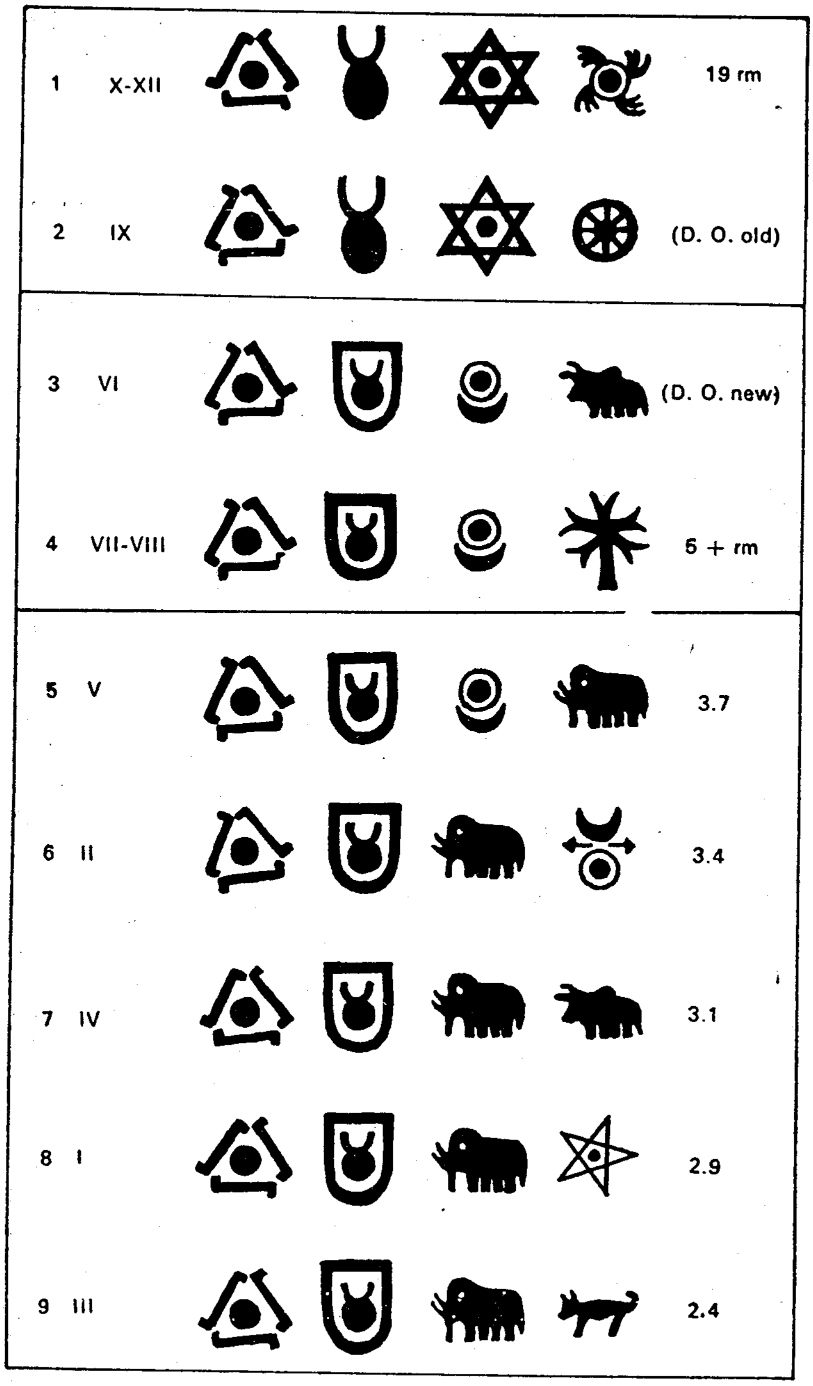


Fig. 10.1 Chronological order of the coin groups.

4 TIME AND PLACE

These coins have been taken to belong to the ancient kingdom of Kosala by Durga Prasad (Num. Supplement XLV to the *JASB,* 1934, XXX)" in which opinion of Walsh seems to concur (W. 28, 15). There seems to be no reason to doubt this, for the district of Kheri certainly belonged to Kosala at the time of the Buddha and earlier (F. E. Pargiter, *An. Ind. Historical Tradition* , pp. 278-9). The question is of the actual time, which must be settled because, at certain periods, Kosala seems to have denoted a region very far away from the Nepal frontier. Daksina Kosala of about the third or fourth century A.D, was the modern Chattisgarh including Baster state (Pargiter, *Dynasties of the Kali Age,* pp. 51-2, 73); However, that is long after the period of punchmarked coins, which on all known evidences cannot have lasted beyond the early Sungas in UP; on the other hand, coinage as such is not known to have begun before the 7th century B.C. in any part of the world. Thus the land may be taken as definite, on the evidence of the find, and the parallel though negative datum that four-mark coins are not found beyond Mathura, if indeed the coins acquired from that place by Durga Prasad were actually fabricated there.

The time of deposit of our hoard cannot be as late as the Mauryan period. For, by then, the Kosalan kingdom had long been an integral part of the Magadhan empire, and the five-mark system as well as 54-grain standard would have to be expected, whereas the freshest group in our Paila collection, namely III, (206 coins) has an average of 2.67 *gm* with blank coins of average weight 2.68 (90 coins) and 2.70 (44 coins) *gm* for groups IIIa and IIIb respectively. This would give the weight standard of the Paila coins as f that of the general standard for silver punchmarked coins current elsewhere, which latter norm corresponds to Mohenjodaro weight class D, and almost certainly to the classical *karsapana* of *32-raktika* (seeds of the *Abrus precatorius)* mass. The four-mark system, wherever found, seems to be of 24 *raktikas,* apparently a special Kosalan standard.

The single aberrant coin described as now occupying No. 1005 is of extreme importance here. It has been hammered out (presumably for recutting to the Paila weight) *after* being punched, but is undoubtedly a round punchmarked'coin of the. Magadhan (54-grain) standard. Its occurrence shows at least one point of contact between the two systems, and the solitary coin would indicate that Magadhan encroachment had not gone far. If the piece be admitted in evidence, we have Support for the approximate period of deposit given above. If, on the other hand, this piece is one day proved to be an intrusion into the original hoard, the guess as to the date of the rest of the hoard is still reasonable. The coin may be that of Ajatasatru or one of the later Saisunagas, from what can be seen of the marks.

Group *b* with its left-facing animals is constant for all the kings I-V, and so must be associated with the earlier developed northern capital, Sravasti; the other should be of the southern, Saketa. If the river were the proper boundary between the two regions, we might take the right and left facing animals to denote the regions of right and left banks of the river. This is a conjecture, that seems to fit all the meagre data. Certainly, the earlier Kosalan trade route passed along the Terai, perhaps through Delhi or Mathura to the west; only later would the expansion to the South take place, presumably with a new set of traders to whom the reverse-mark guilds were still closed. The major trade settlement nearest to Paila would definitely be Sravasti. With Mauryan punchmarked coins, the reverse marks die out, to be replaced in general by a single mark of issue at the mint; this would be incomprehensible if the marks were the insignia of Magadhan traders. This also fits in with the reverse marks being so regular at Taxila and being found further west in the Persian empire, though Magadhan later becomes synonymous with trader and the peninsula proper was opened up by Mauryan armies.

5 THE KINGS

We are now left with the task of restoring king-names, on the reasoned and reasonable assumption that the coins belong to Kosala of the 5th century B.C. or earlier.

The first step is to identify each four-mark group with a king. With the five-mark system starting from Mauryan coins, I have shown that each five-mark group cannot belong to one king, but that each major group, even there, of four marks, does indicate one ruler, the fifth being that of the issuing authority, mint, or lieutenant. Here, such minor authority may be indicated by differences in the individual punches of the same general shape, apart from the right and left-facing elephant which we have already tried to explain. The first mark is common to all the coins, much as the sun-wheel is common to all royal five-mark issues, hence has no individual significance beyond the possible indication of sovereignty. By the significance of a mark is not meant its inner, mysterious, perhaps magical and certainly beneficial meaning, or general heraldic content, but special association with some particular individual. The mark is a development of the *triskelis,* three running legs, here hot starting from a point but pushed out into the form of a triangle by the central dot; it can be verified from Walsh's photographs, as well as the original coins, that there are two forms of this, as of the *svastika,* where the movement indicated by the legs would be clockwise or anticlockwise. Both occur on the coins. I have not been able to separate their groupings to any purpose. The second mark is the taurine symbol, the Brahmi letter *ma,* which is unenclosed in groups IX-XIII, but in a shield on the rest; this is, on a very few coins, stamped twice presumably by inadvertence. Its universality again frees us from paying too much attention to its role, which could not be intended to distinguish individuals. The third marks—and it is understood that the numbering is in the order of relative frequency—fall into just three groups: on the oldest, the hexagram of two interpenetrating equilateral triangles with a large central dot; then the crescent with a hollow circle above it which may be taken as 'sun-and-moon'; and in the freshest and by far the largest set, the elephant, with various fabrics, facing to the right or to the left, but in each further group always with one left-facing, subgroup.

The choice before us, therefore, is to take each three-mark group that of one king, which would give us just three kings; or of one dynasty, which would then give us three dynasties. I take the latter view. lu such a large number of coins, finding only three kings would indicate that coinage was a comparative innovation. But the intruding coin of 32-*raktika* standard, and in any case the diversity and regularity, of the reverse marks shows that a general coinage and trade system was prevalent, which is again supported by the large numbers in each later issue. Finally, from the experience of the Taxilan hoard, where the weights were far more accurately preserved and the coins show virtually no damage, we know that it is difficult to separate the individual issues (fifth-mark groups) of one king, whereas the kings themselves generally show separation which is unmistakable. Here, we should expect the separation to be far fainter, because the reverse-mark system is more irregular as proved by the absorption. The lower incidence of circulation, and therefore of trade is proved by the lower loss of average weight per reverse mark than at Taxila. Seeing that the marks have so much in common in these two hoards, it is difficult to believe that the placing differed as to periods of time, hence the difference must be due to decidedly *less* circulation than at Taxila. This would increase the irregularity observed.

The main conclusion, therefore, is *that each four-mark group of the Paila hoard represents one king.*

The single group offering the greatest number and variety would be the king with longest rule. This fits group I of the coins, particularly when note is made of the fact that the pentagram has many distinct types, i.e., with no dot visible in any of the angles, with dots in all the angles, and a third variety at least with docs in some but not in all the angles. This should indicate king Pasenadi (Sanskrit, Prasenajit) of the records, if our guess as to the time of deposit is admitted. The elephant mark for the dynasty is good confirmation in this case, for we have seen that the family was descended from the Matangas, which would be the clan totem mark. In that case, the last group is III. and must be of Vidudabha Senapati. That this hoard was buried during his reign is possible, but a. better reason would be the invasion of the kingdom after his death. Thus the negative evidence of the hoard combines with the negative evidence of the records to make it very likely that there was no Kosalah king after Vidudabha, and the kingdom was soon annexed to Magadha, during or at least not long after Ajatasatru's reign. Group IV must then be Mahakosala, though the name is of a tribal leader, not a personal name.

What the coins tell us about Kosalan dynasties is that a far earlier line with at least two kings striking coins, is also represented. It has the hexagram as its dynastic mark. The king whose presonal mark was the six-spoked wheel was the last, and there was a violent change in the succession, after which some of the older coins were reissued with marks of the new king, our VI. The new dynasty has the crescent-and-circle for its mark, perhaps the sun and the moon, reminiscent again of the solar and lunar lines of ancient Indian kings. This has ah uncertain number of kings, for it is not clear that VII and VIII are different coinages. King V is next in the legitimate line of succession, but it is remarkable that this personal mark, the elephant, becomes the dynastic mark of successors. Presumably, he came to the throne by right and not force; this should represent some sort of marriage alliance between a Matangaclan and the previous rulers. The earliest superseded dynasty *would not* be that of Kasi, which was supposed to have conquered Kosala at one time, to be defeated in turn and permanently absorbed, unless such coins are found in the Pancakrosi of Benares. A deep pit, say for a well, scientifically excavated in the fort area at Benares might give the answer to this, but in any case more archaeological evidence is needed. For the present a reasonable guess would be that the oldest coins of the hoard represent the last of the real ancient Iksvakus, to be distinguished from successors like Pasenadi and the far later, even more mixed, southern Iksvakus whose inscriptions are found in the distant south near the Mahanadi.

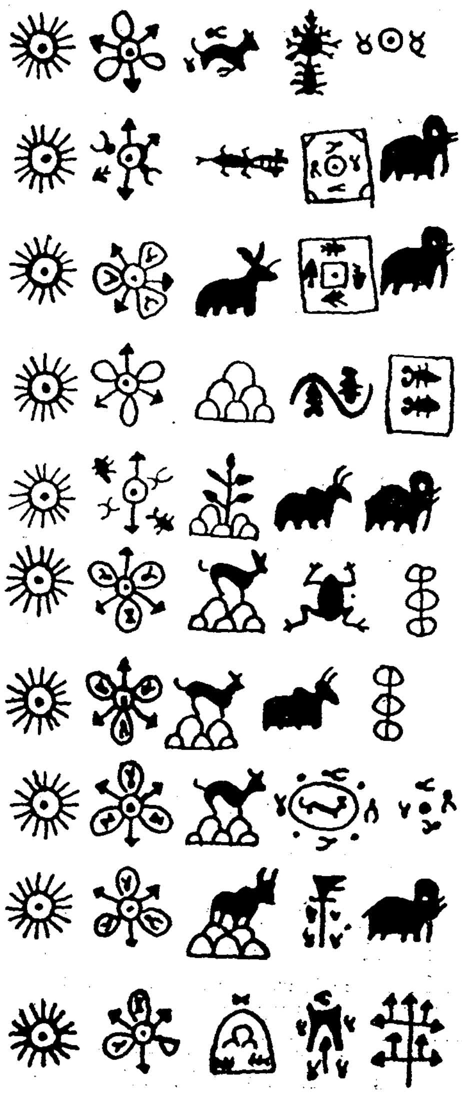
The figure gives the coin, groups in chronological order. The first column represents the order as I have determined it, the Roman, numerals in the second column give Walsh's groups; the next four represent the actual marks found, where it must again be said that there is no special regularity in the right- or left-turning *triskelis,* that the elephant in nos. 6, 8, 9 is found facing to the right also. The last column gives the data for ranking, with nos. 5-9 being given one index number each which represents the mean age. For the horizontal lines, that after 2 represents a violent change of dynasty, from the real Iksvakus to some conquerors; from 4 to 5 is a peaceful change of dynasty, possibly to the Matangas by marriage alliance, No. 7 should be the Mahako-sala of the Jatakas, 8 the great Pasenadi, coeval with Buddha, and 9 the last Kosalan sovereign Vidudabha Senapati.

11
Punchmarked Coins of the Amaravati Hoard

THE METHODS used to reach the conclusion given here have been explained in other papers of mine. As a coin is used some metal rubs off by handling. Not all coins issued at the same time are used in exactly the same manner. Therefore, the effect of circulation is to decrease the average weight but also to increase the variation. If a large number of coins of the same denomination but different issues are found together in a hoard, their circulation obviously stops at the time of deposit. The average weight will be less for the older issues in the hoard than for the newer ones. This simple fact gives the chronological order.

TABLE 11.1 CHRONOLOGY OF PRE-MAURYAN ISSUES

PRE-MAURYAN

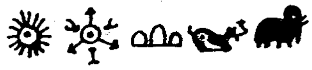
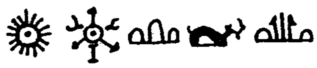
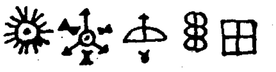
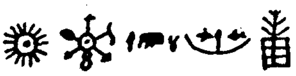


class	symbols	number	weight
III. 1.5		77	898
III. III. 2		22	9
III. II. 6b		105	4
IV. V. E. 1		26	205
IV. IX. A. 3		126	212
IV. VIII. A. 1		52	238
IV. VIII. F. 4		645	253
IV. VIII. E. 4		25	299
IV. X. 2		134	212
IV. XII. 3		134	320

The appended illustrations give the order in time. First comes P. L. Gupta's classification. Then the punchmarks themselves. Then the total number of the particular coins (including other varieties of the fifth mark besides the one actually illustrated) as identified by Gupta in *The Amaravati Hoard;*[1] and finally the average weight in milligrams *above* three grams. The order now given for Mauryan coin differs from my own findings from the Patraha hoard. However, the said hoard was never properly cleaned, so that the published coin-weights were totally unreliable.

TABLE 11.2 CHRONOLOGY OF PARALLEL MAURYAN ISSUES

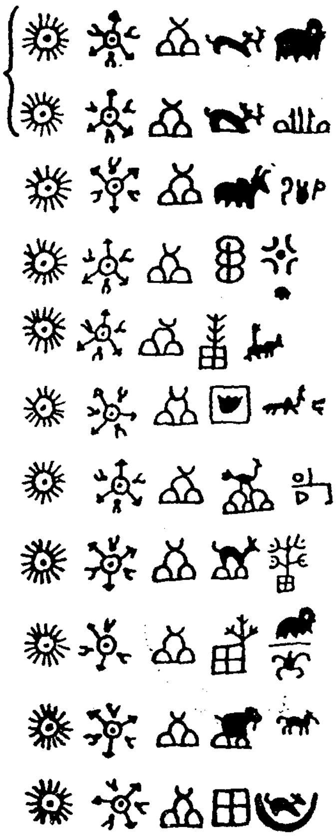
PARALLEL MAURYAN

class	symbols	number	weight
V. 1.1		111	360
V. 1.2		683	314
V. IV. A.1		63	329
V. II. A.1		85	339
V. IV. B. 1a		924	349
V. V. B.1		111	355
V. V. A. 1a		118	362

One important and new fact emerges from the statistical analysis. Debasement of silver currency began in the reign of Chandragupta Maurya. The argument is simple: VI. III. *A.* i is the last of-the older series by weight and appearance. These coins are thinner and apparently contain more silver. The coinage is also the newest issue in the earlier Taxila hoard published by E.H.C. Walsh in *A.S. Memoir* No. 59. But VI. III. *A.2,* obviously in the same four-mark group, is lighter though at the same time it fits in as the earliest of the Mauryan series. The Mauryan coins, particularly of this group, are paralleled by Gupta's group V. Here again the same phenomenon is to be observed between V.I.I, and V.I.2-8. Density measurements seem never to have been thought of, though they would not damage the coins.and would give a great deal of information about the alloy. At my own suggestion some densities were"measured at Patna and Benares, though *not* on coins of the Amaravati hoard. The results are still a bit puzzling, not to say conflicting, but for group V.I. my conclusions have been confirmed.

TABLE 11.3 CHRONOLOGY OF MAURYAN ISSUES

MAURYAN class	symbols	number	weight
VI. III A. 1		196	404
VI. III. A. 2		985	315
VI. III. C		88	320
VI. III. 1. 1a		759	376
VI. III. B. 1		114	392
VI. III. H. 1a		348	402
VI. III. J. 2		624	414
VI. III. E. 1		23	456
VI. III. K. 1		1	557
VI. III. D. 1		4	345
VI. III. F. 1		3	437



For two of the Mauryan issues, namely VI, III. D.1 and VI. III. F, the groups are too small in number for reliable weight statistics. But. VI. III. K.1 can be put at the end of the main series, because it occurs in large numbers of the Patraha hoard. One further conclusion, therefore, is that the Amaravati hoard was probably deposited during the early years of the last Mauryan Emperor Brhadratha.

NOTES AND REFERENCES

The reference is to P. L. Gupta, *The Amaravati Hoard of Silver Punch-marked Coins* (Andhra Pradesh Government Museum Series, No. 6, Hyderabad, 1963).

12
Scientific Numismatics

THE TERM 'numismatics' is normally associated with the avocation of collecting coins. It is well known, of course, that the study of coins also plays an important role in archaeology. Here I should like to set forth in general terms a mathematical approach that can make numismatics more of an exact science. With this approach the archaeologist can add a degree of precision to his stndy of coins, and modern governments can improve the procedures with which they control their coinages and even their paper currency.

The archaeologist finds coins useful because they are normally issued by a governing authority and hence constitute a form of official document. The archaeological value of coins arises from the fact that they survive to an extent unmatched by most other documents, both because they arc physically durable and because they have value for the members of the society and so are likely to be put away in hoards. As a result coins have revealed the existence of cities and even kingdoms that are not mentioned in the old literary histories.

Even when an ancient society is known from other sources, coins can be helpful in various ways. Dated coins—or those that can be otherwise identified with a particular epoch—help the archaeologist to fix the age of the level in which he is digging, to organize the chronology of rulers and to establish the dates of events commemorated by certain coins (see Fig. 12.1 below and Figs. 12.2 & 12.3 at the end of this book). The designs on coins reveal something of the society's religion or mythology and reflect the evolution of its art. Often the only indication of what prominent persons of ancient times looked like is provided by the portraits on coins.

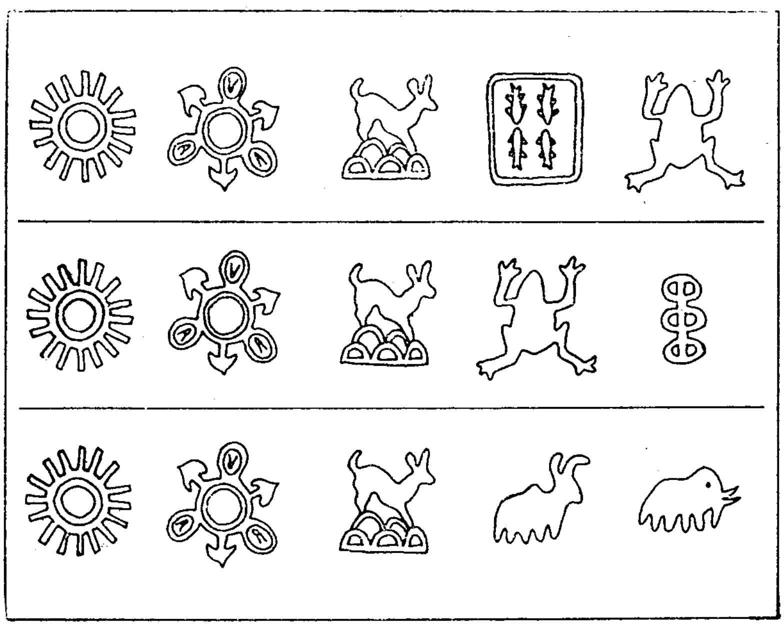


Fig. 12.1: Five-mark system on the obverse side of coins from the Taxila hoard. In each set of marks, the first four represent the king; the fifth, an issuing authority such as a crown prince. Often the fifth mark in one set becomes the fourth in another set, indicating the accession of the crown prince to the throne.

This kind of information is obtained from coins by the scientific techniques of description and classification. A somewhat more complex scientific procedure involves assaying the metallic content of a coinage over a long period of time in order to obtain information about the economic history of the society. Such an investigation reveals that the French sou, which is today a synonym of worthlessness began its career as the solidus, a gold coin of the emperor Constantine the Great (see Fig. 12.4 at the end of this book). This coin's steady degeneration through 16 centuries is a considerable tale in itself. Similar work shows that the gentle art of inflation was practised just as ably in antiquity. Plutarch says, and archaeology confirms that Solon the Wise reduced those Athenian debts that he did not cancel altogether by making them payable in drachmas debased by 27 percent.

The mathematical approach I have in mind goes beyond the scientific procedures I have described so far. The basic theory that makes such an approach possible was developed only a generation ago. This theory, known as 'the homogeneous random process', applies to numerous different types of natural phenomena that are under the influence of many factors of a random nature, for example the diffusion of molecules of one substance through the molecules of another substance, the unceasing zigzag movement (called Brownian motion) of small particles suspended in a fluid, and even the behaviour of a stock exchange.

As applied to numismatics, the theory embraces several factors relating to the weight of coins. In antiquity it was easier to control the weight of coins than the alloy. The reason was that, given a good pair of balances, anyone could weigh the coin quickly and accurately, whereas an assay of the metallic content of the coin called for an expert. Hence the weight of a society's coinage tended to remain fixed even when the coinage was systematically debased in metallic content.

Yet no matter how carefully coins are struck in one minting, by the same craftsmen using the same technique, there is an unavoidable variation in their weight. This variation is restricted somewhat by the traditional practice of melting down the coins that fail to come within a certain margin above or below the 'true' weight. The amount of tolerance allowed has been termed 'the legal remedy'. In India before World War II, for instance, the legal remedy was 1/200 of the weight for the standard coin: the rupee of silver alloyed with 1/12 copper and weighing 180 grains. The differences in weight of carefully minted coins can be determined only with very delicate balances.

When a coin is put into general use, it loses a tiny amount of weight whenever it is involved in a transaction. Mechanical abrasion removes a bit of the coin, and the metal is also subjected to the chemical action of various common agents, including the acids produced by the glands of human skin. In short, the coin loses weight by wear. Such losses would vary from coin to coin even if every coin were handled in practically the same way. The fact is, of course, that not every coin put into circulation at one time is handled in the same way over the years, and so the variation among coins is increased (see Fig 12.5 at the end of the book).

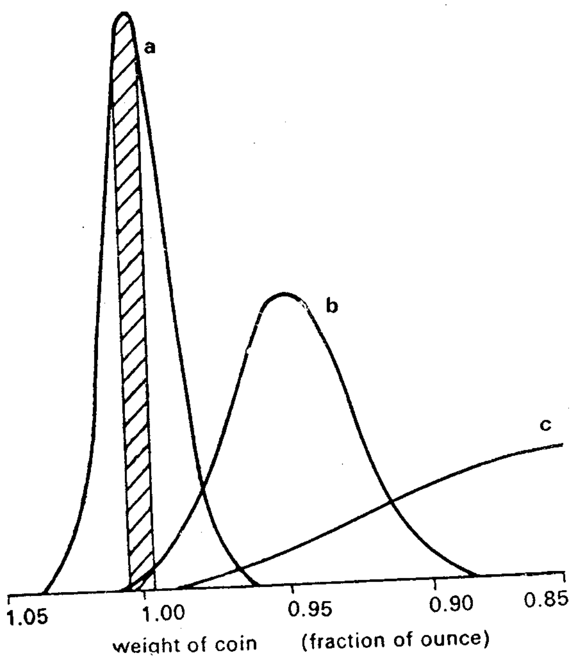


Fig. 12.6 Effect of circulation on weight. This is based on Fig. 6.1. The shaded area under curve (a) indicates the preponderance of coins around the same weight in the mint condition.

The net effect of all the variations in weight, from both minting and wear, can be dealt with by the mathematical theory of the homogeneous random process. The result of the application of this theory to a set of coins in circulation is reflected in the three curves in the Fig. 12.6. The first of them, denoted *A,* is the familiar Gaussian curve of normal distribution: it reflects the fact that a group of coins minted by some uniform process has such a normal distribution of weights. The distribution is truncated because coins weighing more or less than the tolerated margins of error have been excluded by the application of the legal remedy, but in practice the resulting deviation from a normal distribution is negligible unless the minting has been singularly slipshod.

After the coins have been in circulation for a time, the curve of weight distribution is still normal but has changed shape as is apparent in curve *B* in the illustration. The middle of the curve has moved to the right, indicating that the average weight of the coins has decreased and the curve is flatter, showing that the variation in weight among individual coins has increased. The decrease in the average weight and the increase in the variation are each strictly proportional to the length of time the coinage has been in circulation.

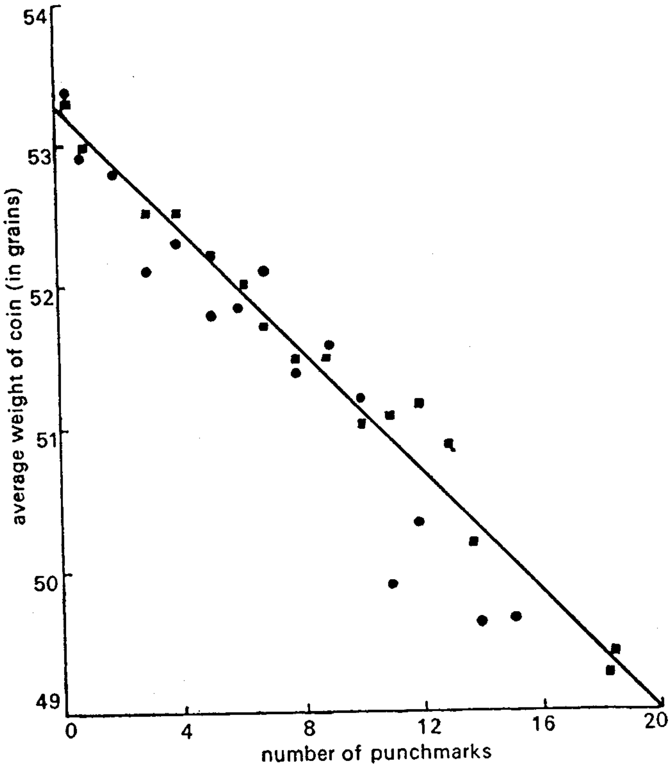


Fig. 12.7 The average loss of weight of a coin due to circulation, for the Taxila hoard. The square marks indicate square coins, and the round marks indicate round coins.

As time goes on the center of the curve moves steadily to the right from the loss of weight. The average weight declines regularly because the average loss of weight is the same per unit of time, say a year. The curve becomes flatter much more rapidly, however, because the square of the standard deviation increases regularly with time. The standard deviation, is the distance from the center of the curve to the point on either side where the curve begins to bend the other way. Curve *C,* reflecting a considerable period of circulation for a coinage, shows the mean weight to have declined further and the variation in weight to have increased so much that only half of the distribution can be portrayed.

Thus far, I have described a situation involving a single group of coins minted and put into circulation at the same time. Suppose one were to take from circulation a random sample of coins of the same denomination, alloy and mint weight but of different date. The curve showing the distribution of weight in the sample would be asymmetric with a long tail to the right, reflecting the fact that the older coins have lost more weight than the newer ones. The shape of such a curve can be calculated theoretically, given the proportion of coins of each minting together with the general rate of wear and the rate of increase in variation in weight among individual coins. These three constants cannot be predicted theoretically; they must be obtained by observation for each country and each denomination of coin.

There is one more factor that must be considered; I have termed it ‘absorption’. Coins of a group tend to disappear from circulation in a regular way that is proportional to the number circulating, provided that the rate of disappearance is not affected by some abnormal situation. The rate of absorption is represented by a statistical law—the same law that applies in the familiar geometric progression (2, 4, 8, 16 and so on). This is also the law of absorption for radiation, the simplest law of biological mortality, the law for the healing rate of wounds and the law of growth by compound interest.

Certain conditions have to be met if these mathematical principles are to be applied successfully. The coins must have been minted accurately enough to show only a slight initial variation in weight. If this is not done, as was often the case with ancient coins of the less valuable metal such as copper, pewter and billion (gold or silver heavily alloyed with a base metal), the effect of wear on the weight distribution is blurred. Secondly, the circulation of the coins must have been normal enough to have the proper effect. This condition usually excludes gold coins, which are often hoarded with a minimum of handling and are also likely to be clipped by owners seeking to amass a private store of gold. Finally, a sample to which the mathematical principles are to be applied must consist of a fairly large number of coins with comparable histories. Ideally, they should be from a common hoard. In keeping with this condition it is useless to compare similar coins of widely divergent histories. For example, a coin of the Greek adventurer Menander, who founded a kingdom in India about 125 B.C., turned up in a *bazaar* at Poona in 1942 and was accepted as currency; its history was plainly different from that of Menander coins dug up by archaeologists in various parts of India or that of a specimen found in Wales.

Even if one has access to a hoard that fulfills all these conditions, it is necessary that the hoard be reasonably well preserved so as to be free of encrustations that would produce variations in weight unrelated to the variations attributable to circulation. If a hoard of coins made of a silver-copper alloy has been buried in damp soil (as happens more often than not in India), the moisture slowly draws the copper to the surface of the coins, leaving a spongy silver underneath. Some Indian numismatists have proceeded to strip away the copper on the technically impossible supposition that molten copper had been poured onto the silver to bring up the weight of the coins.

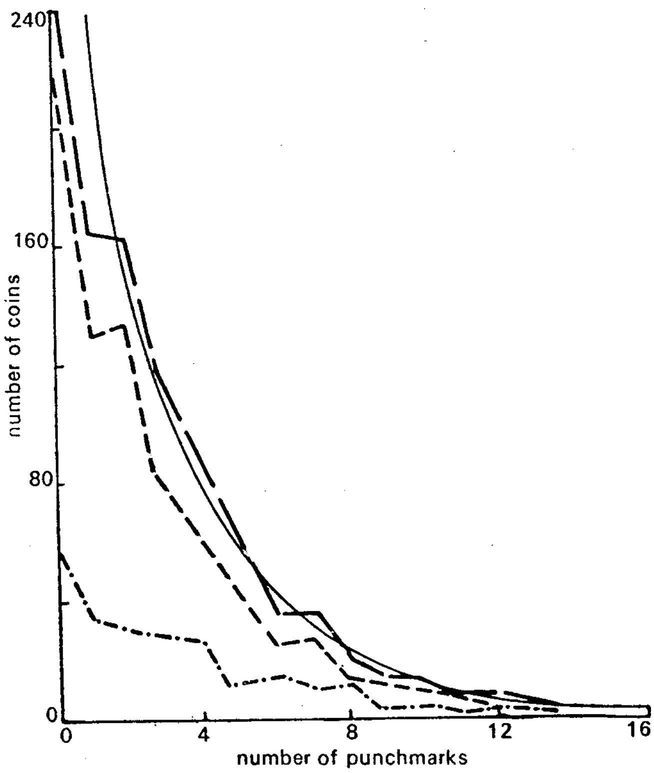


Fig. 12.8 The absorption of coins (i.e., their loss from circulation for various reasons). The solid curve is theoretical; the broken curve overlapping with it all the Taxila hoard coins taken together. The other two curves indicate the absorption of square and round coins, respectively.

Some years ago I was fortunate enough to have access to a hoard that met all these conditions. It was a collection of about 1150 pieces of silver found in a bronze jar that archaeologists had dug up in 1924 in the ruins of Taxila, an ancient Indian city in what is now Pakistan. The jar and the dry climate had preserved the hoard from damage. It was possible to date the hoard approximately by the presence of an unworn coin of Alexander the Great, who invaded India in 327 B.C., and to date it more precisely by a mint-condition coin of the emperor who briefly succeeded Alexander—his half brother Philip Arrhidaeus (see Fig. 12.9 at the end of this book). The condition of the coin, taken together with the facts that Arrhidaeus issued few coins and that. Taxila was far from his actual domain, justifies the assumption that the coin found its way into the jar soon after issue. The hoard can thus be dated close to 317 B.C., when Arrhidaeus was imprisoned and assassinated. There was nothing in the archaeological context to indicate that the hoard had been deposited slowly over a long period of years, as is the case with religious deposits such as the 'Peter's pence' collection at the Vatican, consisting of donations left annually by generations of Anglo-Saxon pilgrims. Therefore it seemed reasonable to assume that the Taxila coins had had a comparable history of circulation and that this history had come to an end at about the time Alexander's local successor Seleukos Nikator lost Taxila to an invading army from the east.

Beyond this the coins at first yielded little information. Most of them had on their obverse (heads) side five punchmarks with much in common from coin to coin. The reverse (tails) side in many instances had an entirely different system of much smaller punchmarks. Nothing else was readily evident. The hoard therefore presented a difficult problem of classical numismatics: the arrangement of unidentifiable coins in their proper chronological order. It was in addressing myself to this problem that I began to work out the application to numismatics of the mathematical principles I have described.

By way of background to the story of my investigation it will be useful to describe briefly the historical setting of India in the period to which the hoard is ascribed. Alexander, whose army was reluctant to follow him farther in his effort to conquer Asia, retreated from India soon after invading it. After his departure a Hindu king, Chandragupta (called Sandrocottus in Greek records), extended his rule over the entire northern part of the Indian subcontinent, expelling or destroying the Alexandrian garrisons. Chandragupta established the seat of his empire at the capital of the kingdom of Magadha in what is now the Indian state of Bihar. Thus was founded the dynasty called Maurya, which lasted from 312 to 178 B.C. The most illustrious figure of the Mauryan dynasty was Chandragupta's grandson Asoka. who reigned for many years, expanded the empire as far west as the Greek kingdom of Bactria and by supporting Buddhism gave it a powerful impetus in India.

In the immediate pre-Mauryan period the only royal authority strong enough to issue coins on a large scale was the Magadha kingdom. In fact, it is evident from a comparison of the punchmarked coins of the Taxila hoard that they are of the type found in profusion in Magadha and wherever Magadha influence penetrated. The system of five marks on the obverse side of the Taxila coins is the same as that of the Magadhan royal coinage (see Fig. 12.1). The first four marks represent a king; the fifth, an issuing authority such as a crown prince or provincial governor. Often the fifth mark in one series becomes the fourth in another, indicating that a son had succeeded his father.

These marks made it possible to group the coins. Then the average weight of each group gave its relative chronological order. It might also be thought that the marks would make it possible to set each marking system opposite the name of a king and thereby solve the whole problem of chronology.

The difficulty here lies with the limitations of Indian historical records. There are no known or decipherable royal inscriptions before Asoka. The records of the Jains, the Buddhists and the Brahmans often call the same kings by different names. In any case the names of kings usually appear in such records almost incidentally, as part of the inflated, semimystical story of some religious figure such as the Buddha. There is no mathematical theory for this kind of history.

Considerably more useful were the punchmarks on the reverse side of many coins. These marks, unlike those on the obverse side, bore little resemblance to one another. They showed all the individuality of the ticket punches used by conductors on American railroads. A group of coins with identical obverse markings might include some specimens with blank reverse sides and some with from one to more than 20 of the tiny reverse marks. (When the number rose above 20, the marks were difficult to count.)

If one ignores the obverse side and merely groups the coins according to the number of marks on the reverse, a striking fact emerges: the larger the number of marks the lower the average weight of the coins (see Fig. 12.7). In other words, the reverse marks supplement the loss of weight as a sign of age. Evidently the reverse marks were correlated in some systematic way with the circulation of the coins.

These marks indicate a system of regular checking of the coinage, probably by trader's guilds. The traders were simultaneously financiers, bankers and dealers in precious me:als; they would have had good reason to assure themselves that a given coin continued to adhere to a standard of weight. Presumably the checking was done at more or less regular intervals, apparently about 12 years, indicating that the practice began about 500 B.C.

Both the obverse and the reverse marks were valuable as corroborative evidence, when I undertook to check the weight, variation and absorption rates of the Taxila coins. Interestingly enough, about 95 percent of the coins fell in the range of some very accurately cut stone weights found in the excavations of two cities much more ancient than Taxila: the cities of Mohcnjodaro and Harappa in the valley of the Indus River. This great civilization, comparable to that of Sumer and probably contemporaneous with it, had vanished without apparent trace until it was revealed by digging in 1925. Yet in spite of the complete silence of all Indian tradition on the existence of the earlier civilization the weight standard of the Indus valley survived unchanged into Mauryan times.

The weight curve of the Taxila hoard has the expected distribution with a tail to the right. Moreover, the number of coins compared with the number of reverse marks follows closely the theoretical 'absorption' curve (see Fig. 12.8). About 70 percent of the coins survived after each checking period, however long that period was. These observations suggest two things about the economy of Taxila: (i) it was stable for more than 200 years, as indicated by the regularity of circulation reflected in the curves of weight loss and absorption of the coinage, and (2) the balance of trade ran in favour of Taxila, as indicated by the presence of Magadhan coins in the community— coins that could only have come in payment for goods originating in or shipped through Taxila.

The same Taxila mound in which the hoard I have desc bed was found yielded another hoard that was deposited at a higher evel and hence was associated with a later time. The coins looked cruder. The system of obverse marks remained but the reverse marks had disappeared. Although these coins were almost in mint condition, the variation in weight was decidedly larger than that in the blank-reverse (and therefore little-used) coins of the earlier hoard. In addition, the alloy was notably debased.

This second hoard was dated to about 250 B.C. by a coin of the Greek king Diodotus I, king of Bactria. Thus the punchmarked coins of the hoard were contemporaneous with Asoka. Although his coins had never been identified until these new methods pinned them down, it was known that his empire was prosperous and spread over the whole of modern India and Pakistan and most of what is now Afghanistan. The architecture and sculpture of the age were outstanding. Why, then, should the coinage be crude? Was there perhaps some mistake in my theoretical approach?

I was able to answer these questions by considering Indian currency that was in circulation in 1940 and 1941. At that time the rupee was still silver, but its copper content had been raised from a twelfth to a half. Moreover, the legal remedy had been abandoned. Although the same mints and machines had fabricated both prewar and wartime coins, I was able to pick out new rupees in 1941 that were above the standard weight by more than twice the old tolerance limit. Obviously Asoka's empire had been short of currency for everyday use, as the government of India was during World War II. Silver currency in both periods was progressively debased, although the ancients—unlike the wartime regime of India—never resorted to tokens and had no paper currency.

Having developed my idea of applying mathematical theory to the study of coins, I tested it with coins that were circulating in India in 1940 and 1941. Much of this work was drudgery; it took me about three minutes to weigh each of some 7000 coins on a slow analytical balance. The tedium was somewhat relieved by the fieldwork involved in obtaining a representative sample of coins from active circulation. Except in three readily explainable cases, the weighings produced curves that closely fit the theoretical curves.

The first exception had to do with gold sovereigns. They had not been legal tender since 1931, although they did change hands occasionally between hoarders. Even so the fit would have been good had it not been for the two oldest pieces, which had been used only for worship by the family that lent them for weighing. Placed in front of the family's image of Lakshmi, the goddess of wealth, the two sovereigns had remained untouched for two generations.

Another exception was the humble copper pie, which was then worth 1/192 rupee and could be found only in a few places such as Benares where pilgrims distributed them as 'charity' to beggars in order to gain merit at the lowest possible cost. The friend who had supplied me with 1000 pie coins did not have the patience to take them all from the marketplace; he supplemented those that had been in regular circulation with a large number from a bank vault, where they had lain unused for years.

Thirdly, the anna pieces were of soil metal and in common use. Hence they wore more rapidly than any other Indian currency. Wear made it impossible to read the dates on some of the annas, and these pieces had to be removed from the sample. They were the most worn and therefore the oldest coins; they were also the lightest, and their removal from the sample raised the average weight of the rest of the sample.

The mathematical approach to numismatics has application, beyond archaeology. In this age of vending machines, pay telephones, turnstiles and whatnot the rate of wear of metal currency could readily be determined by weighing samples, preferably on a much larger scale than my own modest efforts. Given the rates at which weight decreased and variation increased and the weight limit below which a coin would be regarded as useless, the rate of replacement for every denomination could be calculated and the necessary replacement coins minted on a systematic schedule. Allowance would have to be made for changes in demand, such as those resulting from new cons-sumer products and from population growth, and for loss of coins.

A second application would be to keep track of the number of coins in active circulation. This can be tested by the same procedure used to estimate the fish population of a lake. The fish are caught in a net, tagged and thrown back. The number of recaptures is plotted against the total catch; a simple formula then allows a calculation of the population. This number, of course, represents not all the fish but all the fish that are susceptible to the particular method of capture.

The ‘netting’ of coins could be done, by sampling post offices, banks, vending machines and stores on a certain day throughout the country. Tagging might be done by making the coins harmlessly radioactive. Then the recapture could be detected by a Geiger counter. A similar procedure would make it possible to estimate the amount of paper currency in active circulation. The tagging could be done through the serial number, which a computer would read by electronic methods. The same computer would be programmed to provide an estimate of the total currency in circulation. By these methods a scientifically minded treasury could decide on the basis of wear and loss whether it was better to issue coins or paper for any given denomination.

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