

Almgren and Chronology

A Summary and some Comments

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IT is a pity that one of the most interesting and provocative archaeological books to appear in Scandinavia for many years, Bertil Almgren's *Bronsnnycklar och Djurornamentik*—'Bronze Keys and Animal Ornament'—has been published in Swedish. The book has consequently been undeservedly neglected by non-Scandinavian scholars and it may be useful to summarize some of Dr. Almgren's arguments. I will here concentrate on his chronological arguments and only touch shortly on the factual discussion of the material studied—bronze keys—and on his highly controversial approach to the study of Vendel and Viking art.

Almgren catalogues some 450 bronze keys of post-Roman date found in Northern and Western Europe; he discusses their form and their function and describes the locks which they opened. The main problem which faced him was the determination of the date of the keys. The evidence of archaeological association proved to be practically useless, although the conventional dating by grave-groups points to a mean date in the ninth century for most of the keys, with a few dated to the seventh or eighth century.

Instead of letting his study rest at this point Almgren is stimulated by his vague conclusions to ask why it is impossible to date the keys more closely. 'It may seem' he says [*p.* 70] 'that this rich material has been misused from the chronological point of view and that, with the aid of the different styles of animal ornament and by means of archaeological associations, we might be able to obtain a more interesting and exact dating. So, for instance, one might say that the Scandinavian keys which most resemble the western Europe examples, i.e. those with a ring handle strengthened with an animal-head (usually of style D) and with large Anglo-Frisian bits, should be fairly early in the development of the series . . . and that they should be earlier than a key with a style III/E or gripping-beast ornament in the loop and a smaller, or more carelessly executed, animal-head on either side of the shank. As style D is an eighth-century style it may seem natural to presume that those keys with animal heads in this style were made in the middle of the eighth century while keys decorated with style III/E animal heads were manufactured towards the end of the period during which the style flourished, i.e. the end of the eighth century. Consequently succeeding types of key, with complete animals in styles generally dated to the period immediately before and after 800, i.e., III/E 'naturalistic' and gripping-beast styles, should be dated half a century later than their typological predecessor. Such an argument would be consistent with general practices of stylistic dating.'

Almgren points out however that the archaeological associations of the two types of decorated key do not altogether agree with this sequence. The conventional dating of these types, if based on the archaeological associations, would be fifty years out.¹ He goes on to say that there is no reason why a style proved to have been used at a certain period should not also have been used earlier or later, as is demonstrated on certain Oseberg carvings and on the series of mounts from Broa i Halla, where two distinct styles, gripping-beast and style III, appear together. He argues from the accepted dating evidence that the keys decorated with style D could belong just as easily to the first as to last decade of the eighth century.

He points out that only more specialized objects such as coins, ornamented objects, weapons and vessels can be used for dating by association. It is obvious that rare or unique objects are of less use for dating than objects of a well-known type: common objects will appear in many finds thus linking them together. It is however unlikely that the period of manufacture of a popular Scandinavian type of object should be short—a year, for example. The usual estimate for such a period of manufacture is a third, a half, or a whole century—and this estimate is based on usage in communities a great deal more modern than Viking Age Scandinavia.

It is not too daring to assume 'an average of one generation as the length of time of manufacture of a certain type of object in the whole of Scandinavia, especially if it took a little time to introduce a style or type into a new area'. Similarly he postulates a possible similar period for the use of an object before its deposition in a grave. Presuming these theories to be true there is a margin of error of two generations of thirty years in dating the deposition of a single object in a grave. An object could then be placed in a grave in the first year of manufacture or in the last year of a generation which bought it in the last year of manufacture—we have therefore a margin of uncertainty of at least half a century.

In a combination of two finds this margin of uncertainty must be even greater if we use the normal methods of dating type E by type A through the link $A+B=B+C=C+D=D+E$. In such a case the margin of uncertainty must grow with every link until exact dating statements are meaningless.

On this basis an object of type A can have been deposited in a grave either in 800 or in 850 (time of manufacture plus time of use). Another object found in association with A can, if there are no other dating criteria available, just as well be two generations earlier or two generations later than either of the two hypothetical dates 800 and 850. Type B can, therefore, if supported only by this single combination, be attributed to any period between 750 and 900.

Almgren next examines the possibilities of closing this gap. First it might be supposed that a large number of closed finds narrows the margin of error, but this is only true of finds with identical combinations of two well-defined types which are suitable for dating. Taking the commonest Viking artifact

¹ The only good associations of decorated keys are a key of style D at Ulvsunda, found together with a bridle mount decorated with a gripping-beast motif, and a group of three keys with style III/E, naturalistic, animals, associated with finds containing oval brooches decorated with gripping-beast ornament.

fulfilling these conditions—the Viking oval brooch—he shows that only two types, Petersen 37, from the early Viking period, and Petersen 51, from the later Viking period, have been found in sufficient quantities for us to be able to make judgments as to the typicality or otherwise of the find combinations: 535 brooches of type P 37 have been found in 364 finds and 982 examples of P 51 in 676 finds. P 37 occurs eighty times with other objects and P 51 a hundred and fifty times with other objects. These are the only two groups which can be used statistically; other types are only about a quarter as frequent. In the four largest groups only some 21 per cent. to 24 per cent. are datable through find combinations. Obviously a greater number of P 37 and P 51 were made than of the other types and this higher rate of production could be accounted for either by greater productivity or by a longer period of manufacture. Almgren assumes the latter, and if this is indeed the case the margin of uncertainty in dating common types can be roughly equated with the margin of uncertainty in dating less common types, although here the reasons of the uncertainty are of different origin. He shows graphically the likelihood that a manufactured object of the kind used for dating by association makes its most frequent appearance in grave-finds at the end of the period of manufacture, presuming either an even or an uneven annual production. He further argues that, on the basis of the usually accepted theory of a generation for manufacture and a generation for use of each object, the span of time possible for the deposition of an object is about a hundred years. He attacks the dating of graves by the coins found in them and reinforces this point by indicating the discrepancies in opinion concerning the length of the period during which a coin circulates.

Returning to the keys he points out that, despite the fact that one in every seven of known keys was found in association with other objects, the number of associations is probably not great enough to be used statistically. It is again unfortunate that, although about a third of the keys without animal ornament on the handles are found in association with other objects, the most frequent association is with the type of oval brooch which is the least distinctive in the series; even if these brooches were more readily datable it is doubtful if the number of finds associated with the keys could be used statistically to prove the date of one particular type.

After an excursus on the *Eldstålsformade* pendants, which have many stylistic similarities to the keys, he discusses the typological basis for the accepted dating of the oval brooches and demonstrates again that datings based on typology must not be accepted too readily and forced into appropriate pigeon-holes without thought. He draws attention to a fact demonstrated by Åberg² that within a certain period of fifty years in the Scandinavian migration period no brooches can be dated on the basis of typology and consequently no typological division is possible. This Almgren thinks is paralleled in the Viking age. He demonstrates that the oval brooches, which have been made the basis of the chronology of the whole of the Viking age, cannot be used as a chronological yardstick owing to major typological contradictions. He shows for instance that such features as the proportions of the brooch, the position of the pin, the de-

² N. Åberg, *Den Nordiska folkvandringstidens Kronologi* (Stockholm, 1924), p. 29.

generation of its animal form and the pronouncement of the curve are of little chronological significance. The typological changes (size, shape, decoration and mechanics) that take place between Vendel brooches and Viking brooches are very pronounced. He demonstrates the manner in which the typological evidence can be misread by taking two views of the length of time necessary for these brooches to change from the Vendel type to the Viking type. If the span of time needed for the changes to take place is relatively long, and it can hardly be more than the hundred years between 725/50 and 825/50, then the shortest period by which we can measure the archaeological chronology of these pieces must be a hundred years. This is conditional on it being impossible to subdivide the transitional type and he has shown that such a division is impossible. If we take this view of the period of manufacture, then the chronological uncertainty margin on every oval brooch of this type is ± 50 years. Such a result would agree with his conclusions concerning dating by combination, by association and by cross-dating. However, if the period of manufacture of the transitional type of oval brooches be interpreted as a comparatively short period, say 20 or 30 years, the typological development of the brooch must have been extraordinarily rapid. Such a short period cannot be proved by association with coins or other accurately-dated material. If the period was short it shows that typological changes were either always short or of uneven duration. The first conclusion cannot be demonstrated to be true as there are not enough typological changes in the development of the brooch in the Vendel and Viking ages. The other conclusion, which was expressed by Åberg in Ebert's *Reallexikon der Vorgeschichte*, is presumably correct.

Now, if typology is to be used for dating purposes, we must have two fixed points, a known starting point (prototype) and a known point later on, preferably at the end of a phase. If we have not these two points we cannot use typology, for differences may be contemporary branches from the main root. Between these two points one presumes, or finds, a development which is tabulated in an evolutionary manner, i.e. earlier features before later features. But if one presumes a varying speed of development it may be difficult to prove that the steps in the development of a type take place at regular intervals. In other words (taking the classic example of Montelius in 1899) if two phases, A and C, are securely dated through find-combinations, B, which has features common to both, must be laid in an intermediate position; but it cannot be proved that B is halfway between them, for the development may have taken place just after A or just before C.

He further belabours any presumption of even-paced typological development and eventually reaches the conclusion that the curve of development is not continuous but step-shaped.

Dating by typology is an interpolative technique. But interpolation which takes place between two points which, as is always the case in prehistoric absolute dating, have a margin of uncertainty, is chronologically useless. In the case of the brooches these conditions pertain and any attempt at typological dating is completely subjective and inaccurate. A study of typological methods leads us to the conclusion that if the period of transition between one type and the next was short, dating cannot be proved either by typology or association; if the

transitional period was long the margins of error are too great to allow dating. In the available framework it is therefore useless to attempt any dating at all to within a century.

Almgren turns next to analysis of style. It seems impossible to obtain from the oval brooches any definite relative chronology of the different styles of keys, either by association or typology. Similarly it has been impossible to settle the relative chronicles of the various Oseberg styles (Shetelig and Lindqvist came to opposite conclusions from each other). The same is true of the Broa series of mountings. Almgren then examines the possibility that the three styles at Oseberg (III/E, the 'naturalistic' animal style, and the gripping-beast style) are not *styles* but facies of the one style—different motifs executed in the style of the period.

Banishing all preconceived ideas of what constitutes each style, certain common features can be seen—not the usual criteria of Salin (heads, feet and legs) but the shape of the curves of the main lines of construction. In the Broa mount he sees that the animal ornament is based on a design made up of softly curving lines of continuously changing radius, which are usually smallest towards the ends, the lines are never parallel and rarely symmetrical, always diverging and converging with a shifting axis. This definition applies to all the ornament, animal and otherwise, on the Broa mounts and what is more it applies to all three 'styles', III/E, naturalistic, and gripping-beast. What we have is not a 'style' but a 'style conception'. He alleges that different 'style-conceptions' are based on different curves and demonstrates the difference between the Broa and Oseberg styles by characterizing the former with softer, more regularly wavy curves and the latter with harder, more restless, curves. Both groups of ornament are built on the principles of continuously diverging and converging lines with diverging axis, avoiding symmetry and repetition. The discussion of this approach to the art-history of the period is continued at some length but the discussion is so detailed as to make summary impossible.

In this last chapter Almgren examines the historical basis for the dating of keys and comes to the conclusion that the sources are so slender as not to allow any useful chronological conclusions.

Although in many places Almgren seems to have stated what may seem to be obvious, he has done dark-age archaeology a great service in demonstrating the falsity of many fine chronological distinctions currently made. For the first time he has attempted to apply statistical judgements to archaeological chronology. The amount of material of a reasonably homogeneous nature at his disposal has simplified his study and draws attention to the falsity of many judgements made in cases where the material is neither so rich or so homogeneous. Dr. Ole Klindt-Jensen in a recently published paper demonstrates how some of Almgren's judgements can be applied to Danish Roman Iron Age,³ and in this country the Anglo-Saxon archaeologists, and especially those dealing with the pagan period, may take these lessons to heart. One might for instance examine the statement of Professor Hawkes 'Of the bow brooches of purely continental origin, . . . he (Kühn) finds 20 specimens with localities in England, all in Kent except two: seven (one

³ *Aarbøger*, 1957, p. 127.

Cambs. one Lincs.) certainly made after 550, one probably so, and one (Thuringian) of 525-75; and eleven made before 550, three of which he dates 525-550 and eight 500-550, including two—the pair from Chatham Lines grave 2 . . . which he suggests were made *c.* 500 or before 525, but deposited rather after, since its contents as a whole belong to Werner's group II, 520-550. It is about 525, in fact, that Kühn reckons the Frankish-Kentish relations thus displayed to have begun. It seems, therefore, that the datings on which Leeds suggested that in the time of Clovis, who died in 511, Franks immigrated into Kent, cannot now be held within that line.⁴

Let us first look at Kühn's statement in the light of Almgren's arguments. Kühn⁵ examines most of the major types of bow brooches found in western Europe in the migration period on the basis of those found in the Rhineland. Numerically the commonest type quoted is that of which the examples from Chatham Lines, grave 2, are representative. Kühn quotes 134 of these brooches in 124 finds—of these 24 are closed finds, i.e. 20 per cent. of the whole. The key find, according to Kühn, is one of the Weimar graves which contains a copy of a coin of Zeno (474-491) adapted as a pendant adding that 'The grave was *therefore* deposited between 500 and 550'⁶ (the italics are mine). However, let us look at other graves which contain coins and copies of coins in this group:

Chatham Lines, coin of Valentinian (364-75 or 375-92) and coin of Anthemius (467-72).

Böckingen I, a necklace of 14 silver Roman coins, the earliest of which dates between 98 and 117, the latest between 235 and 249.

Lavoie, copy of a triens of Anastasius (491-518).

Except on the chain of cross-dating, represented by Almgren as $A+B=B+C=C+D=D+E$, the only statements that can really be made about this group is that one of the graves (from Weimar) must have been laid down later than 491. There is no reason why the group should not be dated between for instance 475-575. Further there is no reason why all the brooches in this group should not have been made before 500—their presence in later graves being explained by the fact that they were old when they were put in the graves. In this connexion we might remember that Almgren has demonstrated that the type of object used for typological dating makes its most frequent appearance in grave finds at the end of the period of manufacture. After all as Kühn admits, presumably on 'common sense' grounds, that the coins in the Böckingen grave are useless for dating purposes, why do we have to accept the Weimar *copy* of a coin as a more valuable piece of dating evidence? Similar arguments can be used on the other grave-groups quoted by Hawkes. It is, indeed, impossible to accept such fine dating on typological grounds.⁷

⁴ C. F. C. Hawkes, 'The Jutes in Kent,' *Dark-age Britain* (ed. D. B. Harden, London, 1956), p. 101.

⁵ H. Kühn, *Die Germanischen Bügelfibeln der Völkerwanderungszeit in der Rheinprovinz* (Bonn, 1940).

⁶ *Ibid.*, p. 121. By parallel with another group of brooches he narrows this date to 525-550.

⁷ It is interesting to note that both Werner's and Kühn's dating systems have been challenged by Voss, on similar grounds to those used here, in a new study of the Høstentorp find. O. Voss, 'The Høstentorp silver hoard,' *Acta Archaeologica*, xxv (1954), 171 f.

Turning next from Kühn's statements, which were, not unnaturally, accepted by Hawkes, let us examine Hawkes's statements in the last two sentences of the passage quoted. On the basis of these grave-finds, and particularly Chatham Lines grave 2, Hawkes says that the Frankish phase in Kent began about 525. The brooches could just as easily have been buried in the year they were made as in the year of the death of the purchaser's grandchild and they could just as easily have been brought to Kent on either of these two occasions.⁸ Finally, for Hawkes to alter the date of the beginning of the Frankish phase in Kent by sixteen-odd years on the basis of a handful of brooches is statistically a little unwise.

Hawkes and Kühn, who have been used almost accidentally in this context, are two of the most respected archaeological scholars who deal with this period, but nearly all of us have made similar judgements based on *ex cathedra* dating statements by experts.

The above discussion illustrates, perhaps as well as any other, Almgren's arguments on the subject of dating graves by the coins found in them. It is on the subject of coin dating that I feel Almgren has been too dogmatic. There is a great deal to be said in Almgren's favour and, despite Professor Werner's great work on coin-dated graves, many people feel a little doubtful of a chronology constructed on such grounds. In certain instances however coins in graves do give, I believe, an accurate dating statement. Sutton Hoo immediately springs to mind; in the light of Mr. Grierson's careful examination of the problem I think that few can remain unconvinced of the validity of the date 550-560 for the deposition of this great Anglo-Saxon treasure.⁹ Such dating however is only useful when a hoard of coins is found in the grave, and such occurrences are rare. However, in the Scandinavian area in the Viking period and in the British Isles from the turn of the eighth century we have a large number of coin-hoards, many of which contain objects of a non-numismatic nature. There are more coin-hoards in Sweden than there are in the other Scandinavian countries and England put together, and now that Professor Stenberger has completed his great work on the hoards of Gotland¹⁰ we should be able to make some chronological judgements of archaeological material on the basis of the coin-hoard evidence. The work of the English numismatists, and particularly of Mr. Dolley and Mr. Blunt, has enabled us to date coins with accuracy not previously dreamed of. Certain hoards can be dated to within a few months and most to within a narrow margin of years. On this basis the margin of error in the date of deposition of the object is brought down to a minimum.¹¹ But even in these finely-dated hoards we must be extremely careful in passing judgements of date on the asso-

⁸ It is unfashionable to draw modern parallels, but I might quote one, with all reservations. On looking at my wife's trinkets I find that she has a dozen or so ornaments which are more than thirty years old and the story of one of them is worth quoting: it was bought in Norway in 1894 by her grandparents and did not enter this country until 1954; it is still worn. This in an age when fashions change more quickly than ever.

⁹ P. Grierson, 'The dating of the Sutton Hoo coins,' *Antiquity*, xxvi (1952), 83.

¹⁰ M. Stenberger, *Die Schatzfunde Gotlands der Wikingerzeit*, II (Lund, 1947), and I (Uppsala, 1958).

¹¹ Cf. the examples quoted by R. H. M. Dolley, *Some reflections on Hildebrand Type A of Aethelred II* (Antikvariskt Arkiv 9, Stockholm, 1958).

ciated archaeological material. An awful warning can be seen in the two very similar brooches, executed in the same style and differing mainly in their comparative sizes, which occurred, one in a coin-hoard at Beeston Tor, Derbyshire dated 872-5, and another in a hoard in Inedahlgatan, Stockholm, dated 1006.¹² We remember, too, that it was pointed out long ago that the Roman coins found in the Danubian area bore little relation to the known history of the Roman provinces.

Almgren's criticism of the dating of material has been largely destructive, but archaeologists cannot and should not follow him with an absolute obedience. The refining of dating evidence for dark-age material over the whole of Europe during the last century has built up a comparative chronology which is a useful slave. In the present state of our study this chronology can never become absolute. A passage from the will of the Atheling Athelstan, who died in 1015, should be remembered by every Anglo-Saxon archaeologist: 'And to my brother Edmund I grant the sword which belonged to King Offa'¹³; this sword was more than two-hundred-and-twenty years old when the Atheling died.

¹² R. L. S. Bruce-Mitford 'Late Saxon disc-brooches,' *Dark-age Britain* (ed. D. B. Harden, London, 1956), pp. 171 ff.

¹³ D. Whitelock, *English Historical Documents*, 1 (London, 1955), 549.