Anglo-Saxon Chronology II – the female graves

A commentary on Chapter 7 of *Anglo-Saxon Graves and Grave Goods of the 6th and 7th Centuries AD: A Chronological Framework*.

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ABSTRACT: The book *Anglo-Saxon Graves and Grave Goods of the 6th and 7th Centuries AD: A Chronological Framework* (2013) is a major contribution to, and revision of, Anglo-Saxon chronology, the importance of which has already attracted considerable attention. The central Chapters 6 and 7 on the chronology of male and female furnished inhumations depend heavily on statistical and scientific methodology and reasoning that not all Anglo-Saxon scholars are necessarily equipped to follow. This assessment is based on comment from such scholars, who have also commented that the book makes for very difficult reading. This paper, and its companion on the male graves, was conceived as a 'reader's guide' to the analysis that tries to separate out the more important aspects from those that the less statistically informed reader might wish to avoid.

The statistical methodology developed in the book is, in an archaeological context, innovative and more advanced than other comparable methodology I am familiar with. The value of this extends beyond Anglo-Saxon studies and should be of interest to a much wider audience. This and the companion paper have taken a critical approach concerning the 'readability' of some of the text, but the methodological importance should not be obscured. This paper concerns Chapter 7 on the chronology of the male graves. A separate paper on Chapter 6, the male graves, has been written. The analysis of the female graves, the subject of this paper, posed problems of a different and – from a statistical perspective – more interesting nature.

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1 Introduction

A previous paper commented on aspects on Chapter 6 of *Anglo-Saxon Graves and Grave Goods of the 6th and 7th Centuries AD: A Chronological Framework* Bayliss *et al.*, 2013)which was concerned with the chronology of the male graves. This paper reviews Chapter 7 where the chronology of the female graves is developed.

The introduction to that chapter states that it is structured in a similar way to Chapter 6 'although the importance of different elements perforce varies'. It turns out that some of these 'different elements' have necessitated a slightly different approach to presentation in this paper. The fundamental difference is that an important modelling assumption used in Chapter 6 – that furnished graves are 'deposited' at a uniform rate over the time period involved – is patently not valid for the female graves, with non-trivial consequences for the modelling procedure. The authors dwell on this at some length in the book, particularly Section 7.2, but it is not immediately evident how they deal with the issues that they raise there. I did not think it necessary to discuss this assumption at any length in the previous paper on the male graves where it seems reasonable, but now do so in Section 2 as a prelude to discussion of the female graves.

More so than in the paper on the male graves I have thought it useful to comment on some more specifically technical statistical issues, and have undertaken some re-analysis of the data. This runs counter to my original intention, which was to produce a review for an audience I assumed might be uncomfortable with some of the statistical aspects of the book. Apologies, therefore for this; I have relegated some of the more technical comment to footnotes or to Appendix 1 or 2, and issued health warnings where such material is included in the main text.

I suggested in the previous paper that the book was not an easy read because far too much detail was presented for most readers' needs, and that this obscured the more essential points. That I wrote the paper, and this one, at all was because this kind of view seems to have been expressed by everyone I know of who has read the book. Subsequently I have looked a little more closely at sections of the book that describe what might be called the 'philosophy' underpinning it. Christopher Scull's Chapter 3, on the background and development of the project is especially revealing. He emphasises the iterative nature of the research process, for example, it 'was recognized from the outset that truly iterative and reflexive working would require constant re-evaluation and re-formulation of models throughout the life of the project' (p.92). Where I think a problem arises for the reader of the book is that they have been exposed to far too much of the detail of this iterative process. In effect the authors have erected a building (chronology) the beauty (or otherwise) of which the viewer would like to stand back and admire (then wait to see how long it stands up with usage). Unfortunately much of the scaffolding has been left standing; some of it is unsightly; and it obscures the view to the extent that some may not take much more than a glancing look.

I drew an analogy in the previous paper with the editing practices of film-makers in the 1920s, and in particular that the practice studios had of cutting the rather lengthy films of Erich von Stroheim to a more 'manageable' viewing length might usefully have been applied to the book. Others might disagree with this assessment. I did also note, with reference to von Stroheim's film *Greed*, that some critics viewed such editing as 'butchery' of a masterpiece, completely destroying the integrity of the film. My comments about what might be regarded as inessential to an understanding of the book, or as of secondary importance, might easily be construed in this light. I could, as an intellectual exercise, make the case myself, but would still argue that the narrative 'blow-by-blow' style of presentation could be reduced to a much more concise and readable format.

2 The male graves revisited

It is a statistical truism that 'all models are wrong' but 'some models are useful' (p.74). The idea is that models – Bayesian models included – are based on assumptions unlikely to be exactly correct, therefore the model is 'wrong'. If, for the purposes for which a model is intended, the assumptions are approximately true, then the model can, nevertheless be useful. If the assumptions

are seriously untrue then the model may be *importantly wrong*, an emphasis applied in the text when the issue is discussed.

Another way of thinking about this is in terms of the *robustness* of a model to the assumptions involved; that is, if an assumption can be wrong without the conclusions derived from it being much affected then the model is robust with respect to that assumption. Assumptions are necessary to make the mathematics needed to obtain a solution 'work'. Even with modern computational power the assumptions adopted are sometimes a necessary mathematical convenience and Bayesian modelling is no different from other forms of modelling in this respect.

An important assumption in some of the models used in the book is that the archaeological activity over the period modelled has a uniform distribution; that is it began and ended, and in between 'carried on at a relatively constant and continuous rate' (p.76). For the dated male graves the assumption of a relatively even rate of furnished male burials is judged to be satisfactory, an additional assumption being that the sampled graves are representative of the period (p.237). The study is unusual in having enough data to assess the validity of the uniformity assumption, either by summing the posterior densities after calibration or, more simply, by looking at the distribution of the median dates after calibration (Figures 6.3 and 6.4, p.240). This issue is stressed here because when it comes to the female graves (see below) the uniformity assumption turns out not to be reasonable and this introduces complications into the analysis.

For the male dates the issue is discussed at a little length in the context of Table 6.1 (p.239), constructed without the benefit of the phasing information inferred from the correspondence analyses used as prior information in later models. Table 1 below summarises the 95% intervals for the start and end dates of the sequence derived from some of the more important of the numerous models presented in Chapter 6.

	Phasing	Calibration	Rate	Start	End
Figure 6.1	None	Terrestrial	Uniform	*cal AD 520-550* (95%)	*cal AD 660-690* (95%)
Figure 6.52	Leading Type	Terrestrial	Uniform	*cal AD 525-550* (95%)	*cal AD 660-685* (95%)
Figure 6.53	2-D CA	Terrestrial	Uniform	*cal AD 525-550* (95%)	*cal AD 665-680* (95%)
Figure 6.58	Leading Type	Terrestrial	Variable	*cal AD 515-555* (89%)	*cal AD 660-705* (95%)
Figure 6.59	2-D CA	Terrestrial	Variable	*cal AD 520-560* (95%)	*cal AD 665-690* (95%)
Figure 6.68	Leading Type	ISOSOURCE	Uniform	*cal AD 540-570* (95%)	*cal AD 660-690* (95%)
Figure 6.69	2-D CA	ISOSOURCE	Uniform	*cal AD 540-570* (95%)	*cal AD 665-690* (95%)

Tab. 1: *95% intervals for start and end dates of the sequence for male graves, for different phasings, calibration curves, and rate assumptions. For Figure 6.58 an 89% interval rather than the disjoint 95% interval is given.*

The ISOSOURCE vs. terrestrial model comparisons (Figures 6.68 and 6.69 vs. Figures 6.52 and 6.53) were discussed in the previous paper. The comparison between Figures 6.58 and 6.59 vs. Figures 6.52 and 6.53, the preferred models, was not previously discussed and needs some explanation. The latter figures employ the uniformity assumption; Figures 6.58 and 6.59 are part of a 'sensitivity analysis' where the uniformity assumption is relaxed by assuming uniformity *within* phases but allowing the rate of burial to vary *between* phases (Section 6.5.2, pp.300-301 and Table 6.6, p.304). That there are larger boundaries in some analyses is noted on p.301 in connection with Table 6.6 (e.g., for the end period compare the upper bound of 705 for the variable-rate model with that of 685 for the uniform model for leading type phasing). This is described as 'slightly later dating' and interpreted as a 'substantive difference' that is 'a matter of precision rather than chronology' (p.301)[2].

The conclusion to this part of the analysis is that there 'is no statistical reason to prefer' the variable-rate model to the uniform-rate model. The choice of wording is interesting; it is not the same as saying the uniform-rate model is to be preferred for statistical reasons or that the variable-rate model is worse.

The ISOSOURCE models produce comparable end-dates when compared to the preferred models but later dates by up to 20 years for the start of the sequence. As noted in the previous

[2] That is, the mostly wider ranges and later dates are attributable to less precision in estimates at the boundary arising from the need to include more parameters in the model.

paper the authors judged the terrestrial calibration to 'provide the most realistic date estimates for the study' (p.328)[3].

There is a sense in reading this that, having established preferred models in Figures 6.52 and 6.53, the authors are fairly keen to retain them. It is not clear how 'substantively different' the variable-rate, ISOSOURCE, or any other competing model would need to be to overturn this 'preference'. It is not based on any assessment that these other models are 'wrong' in some way. In a slightly different context, in Section 8.2.2 on 'The end of furnished burial in Britain', it is commented that it is 'probably unwise to interpret the date estimates in this section too literally' because we 'do not know enough about a number of scientific factors which may affect the accuracy of radiocarbon dating at this level of precision'. This thinking could have been applied to earlier discussion, with a more ready acceptance that different models with date estimates differing by up to 15-20 years exist, and that there is no overwhelming scientific reason for expressing a preference between them.

In passing, it can be noted that, if you went with the preferred models for the male graves and were only interested in the start and end dates, there is no substantive difference between the comparatively simple model of Figure 6.1 and those that emerge 50 figures later.

3 The female graves

3.1 Book Section 7.2

This rather lengthy prelude has been provided in order to introduce statistical issues that need to be discussed in the context of the female graves. The main issue, that archaeological activity is assumed to have a uniform distribution, was not a major problem with the male graves. It is a problem for the female graves. The furnished female graves manifestly do not occur with anything approximating to uniformity.

Analysis begins with Figure 7.1, the analogue of Figure 6.1, a model uninformed by any phasing constructed from the correspondence analysis. This is accompanied by a few paragraphs of text (pp. 345-346), some of it possibly unnecessary in the light of the announcement on p.346 that there is 'evidence that this [Figure 7.1] is not an appropriate model for the dating of the sampled female graves'. Technically this is because a bimodal rather than uniform model of deposition seems more appropriate; archaeologically, there are clear peaks in the occurrence of furnished female graves about the mid-6th and mid-7th centuries, but not much happening about the turn of the centuries. The model is described as *importantly wrong* on p.347.

Pages 346–356 demonstrate the problem and explore more precisely its nature. This section is statistically interesting but I was not clear about the conclusions drawn from it as regards subsequent analysis. Several issues are addressed, but to what end is not obvious. My initial thought was that analyses were geared towards finding a model, as an alternative to the uniform-rate model, that could be used as more realistic prior information for the Bayesian model.

Figures 7.5–7.8 present the results from nine simulations designed to investigate what the underlying distribution of the dated graves is. The reader unversed in this sort of methodology might wish to avoid a detailed reading but some important points emerge.

1. The underlying distribution needed to produce the observed results, in Figure 7.5, has to be even more sharply bimodal than that figure suggests (p.348).

2. This raises the possibility that there might be a discontinuity in the period of furnished female burials. This cannot be demonstrated 'unequivocally' one way or the other (pp.348–349). I will return to this in Section 4 below and Appendix 2.

[3] I disputed the logic that seemed to underlie this judgment. The same judgment is repeated for female graves in an identically worded passage on pp.447-448, and similarly worded passages in Chapter 8 comparing results for the male and female graves (p.462, p.468) and in Section 10.6.1 (p.554) on statistical issues to be addressed in the future. On this latter page, however, the discussion is accompanied by the comment that a 'dietary effect in the order of 10 or 20 years, however, cannot be entirely ruled out'. I would omit the term 'entirely'.

3. Attention is drawn to the fact that, because of selective sampling of earlier graves, the sample may misrepresent the 'real frequency-distribution of type and period of activity that we are investigating' and 'explicit consideration' is needed of this (p.351).

4. The more archaeological discussion on pp.353–355 addresses this last issue by using dating schemes devised by Helen Geake and Birte Brugmann to infer what the distribution of the sampled graves across time might be (Figures 7.9, 7.10). The distributions inferred from either scheme bear no relation to the bimodal distribution that prompted the investigation (Figure 7.10); it is noted that both schemes 'predict a relatively even rate' (p.355). The authors conclude that the Geake/Brugmann analysis is consistent with 'the premiss that our data matrix does indeed contain a continuous sequence of grave-assemblages'.

At this point it has been established that a fundamental assumption of the initial model (uniformity) is *importantly wrong*; that the sample of dated graves is unrepresentative; and that the distribution of burials over time as suggested by simulation is at variance with what other studies might lead one to expect. I was expecting an explicit resolution of these issues in order to allow modelling to continue but such resolution is not obvious.

A clear statement at this point that variable-rate models were henceforth to be used would have been useful (if this is what was done). The first models to follow the first seriation in Section 7.4.1 do not make this clear. I think that the comment that two groups of bead-types were 'modelled as sequential and abutting' (p.372) is intended to imply variable-rate modelling but was not sure about this. I'd understood the uniform-rate modelling of the male graves to involve sequential and abutting phases as well, so that the use of variable-rates is best made explicit. This is done later in Section 7.4.2[4].

Otherwise, If you fast-forward to page 415, after the CA has been completed with an associated Bayesian model attached (Figure 7.65, p.414), you read that because of the strong bimodality of the sample of furnished graves 'we had doubts that our modelling could estimate the period of dated graves reliably' and that this was dealt with 'by allowing a different rate of [uniform] burial in each of the phases derived from the seriation'. One can then fast-forward again to Section 7.5.3 (p.432) to find much the same statement about variable-rate modelling. As noted, this reader would have welcomed an explicit statement along these lines at the end of Section 7.2. I will comment further below in Section 4.

3.2 Book Section 7.3

Section 7.3 (pp. 356–372, on the currencies and phyletic seriation of individual artefact-types, proceeds assuming a uniform distribution although this is 'probably not appropriate for the overall assemblage of female graves' (p356). The use of the uniform distribution is rationalised on the grounds that tests have been used 'to determine whether this approach is appropriate' and that 'simulation has demonstrated that the underlying distribution has to be substantially non-uniform before the outputs of a model are *importantly wrong*'.

It might have been simpler to say that, given the obvious bimodality for the assemblage as a whole and a division into 'early' and 'late', the assumption of uniformity is only justified if an artefact-type falls only into one of the early/late categories. This turns out to be mainly the case; an exception is amber beads (BE3) which are excluded from subsequent analyses as a consequence.

3.3 Book Section 7.4

This is sub-divided in a different way from the analogous section for the male graves. A broad outline is provided below followed by a little more detailed comment. The bottom line is the seriation presented on p.409 – with some agonising about the continuity of furnished burial along the way and the occasional digression.

[4] Specifically, the latter reference on p.381 states that 'we again use abutting, contiguous uniform phases' to allow for the 'varying rate of burial'.

3.3.1 Book Section 7.4.1, pp.372–375

The main purpose of this section is to initiate the iterative process of seriation using 27 bead-types – the most common of the artefact-classes – one of which (millefiore beads, BE1-Mosaic) is discarded from subsequent seriations[5]. As with the male graves, once it is understood that the analysis of the beads here and then in Section 7.4.3 (ignoring Section 7.4.2) establish a pattern to which subsequent additions (of types and graves) conform, the reader not interested in the fine detail could omit much of pages 386–406 and resume with the final seriation and model in Section 7.4.4.

3.3.2 Book Section 7.4.2, pp.376–383

This section initially seemed to me to be a distraction. It would be of interest in its own right to those dedicated to bead-typology but is a distraction in the sense that it does not advance the cause of seriation. The comments on Section 7.2 above noted that the issue of discrepancies in the distribution of female graves implied by typologies of Geake and Brugmann, and that implied by dated graves in the sample, was left unresolved. On re-reading Section 7.4.2 I realised that it might be construed as a 'refutation' of Brugmann's dating that explained the discrepancies.

Essentially the section uses Bayesian models to explore and question aspects of Brugmann's (2004) grouping and phasing of glass beads (p.379). My reading is that after some omission of grave-assemblages, and some revision of Brugmann's phasing, phase dates proposed by Brugmann are shifted sufficiently to induce the bimodality of the dated sample (i.e. contrast Figures 7.40 (p.381) and 7.42 (p.383) with Figure 7.10 (p.355). If this reading is correct an explicit concluding sentence or so to this effect, at the end of Section 7.4.2, would have been useful, with a forward reference to it provided in Section 7.2. In the first draft of this paper I suggested it might have been better to write the section as a self-contained short paper placed in an appendix and still incline to this view.

3.3.3 Book Section 7.4.3, pp.383–409

The main business of constructing a seriation that contains as many dated graves as possible resumes. To the 26 bead-types are added (sequentially) 20 pendant-types (p.386); 6 wire ring-types three of which were later removed (p.390); 12 brooch-types (pp.394–395); 5 pin-types (p.395 and p.399); 7 buckle and belt-fittings (p.399); and 5 miscellaneous accessories (p.399). At each stage of the procedure the merits of including/excluding types/graves that are possibly problematic; 'anomalous' artefact combinations; and so on (as with the male graves) are assessed. The style of presentation and detail of the discussion is similar to that for the male graves, which I argued made for 'heavy' reading and which readers prepared to take the detail on trust could omit with reasonable safety.

By page 401 a total of 300 grave-assemblages and 86 artefact-types have been included in the seriation. Pages 401–408 are concerned with adding as many as possible of the dated graves to the seriation as possible, if they are not already in it. Unless readers have strong feelings about the inclusion of particular graves (about which they can do nothing) they might feel inclined to skip the detail. The decision is made to exclude some artefact-types and this, in turn, involves the exclusion of graves previously in the seriation. One ends up with a data-set containing the same number of graves (300) but slightly different ones and slightly fewer artefact-types (81) (p.408) with the preferred CA shown in Figure 7.62 (p.409).

[5] The rationale for subsequent omission (p.375) is that the three dated graves that include the bead-type 'fall at the very start of our sample of female graves, and so we may in fact only have later incidences of this type'. I'm not sure if I followed the logic here. You are referred to Section 7.3.1 for more detail to find that the three dated examples come from the same cemetery and 'seem to date to the early 6th century AD'. You have to wade though 15 pages of Section 7.3.1 to find the relevant reference about a third of the way down the first column of text on p.363 – the book lacks a proper index and cross-referencing between sections leaves a lot to be desired.

3.3.4 Book Section 7.4.4, pp.408–415

While all this is going on Bayesian models have been intermittently fitted with between three and five phases imposed on the seriation at any particular point. As with the analysis of male graves the view can be taken that these are only interim stages in an iterative process (the scaffolding), necessary perhaps for the analysts but which a reader mainly interested in the outcome might have been spared. Such readers need only start paying detailed attention at this point.

The presentation of the phasing in Figure 7.62a is confusing. Phasing is indicated in two different ways. The first uses coloured dots for the dated graves and indicates three phases, x, y, z. The second divides the map into zones using coloured lines, two solid and one dashed, thus dividing it into four zones (not to be confused with phases).

The dashed line represents an experimental attempt to subdivide phase x that did *not* work and was not used – it probably should not be shown as it simply confuses things. What was actually done was to divide the most thinly populated phase y – where the yellow dots occur – into two phases y1 and y2. The second of these, however, contained only one dated grave. To avoid this a phase boundary implied by the x, y, z (blue/yellow/red) classification is shifted to the left (the solid blue line), effectively reclassifying two dated graves originally assigned to phase z (red) as y2.

This reclassification is *not* reflected in a re-colouring of the dots, nor is the phase boundary between x and y1 indicated by a solid line as with the y1/y2 and y2/z boundaries. Thus, in terms of what follows the dashed line shown is irrelevant and neither the colouring of the dots or boundary placements adequately reflect the phasing actually used[6].

Once this is sorted the phases x, y1, y2, z are renamed p, q, r, s 'for consistency with the male series' (p.410). Leading-type phasing is discussed on pp.410–412 and turns out, conveniently, to be congruent with that derived from the two-dimensional CA. That is, the leading-type phases a, b, c, d contain exactly the same graves as phases p, q, r, s.

Given the leading-type phasing, and as with the male graves, it becomes possible to identify some graves not in the seriation with particular phases, on the basis of individual artefact-types (p.413) and these, along with four graves – undated by radiocarbon – for which coin dates are available, are added to the preferred Bayesian model in Figure 7.65.

3.3.5 Book Sections 7.4.5 and 7.4.6 pp.415–422

Section 7.4.5 (pp.415–416) addresses the issue of the distribution of female graves. The 'business part' is Figure 7.68 (p.418) showing the estimated frequency distributions for the four phases, weighted to allow for the fact that the distribution of *dated* graves in the phases is not representative of the distribution of *seriated* graves in the phases. Figure 7.67 is similar but for the unweighted data and seems unnecessary[7]. I will return to the issue of distribution in Section 4.

Section 7.4.6 (pp.416–422), on assessing the accuracy of the female seriation, because of the 'blow-by-blow' account of how the analysis was conducted, is harder to follow. Two ways of deriving a chronological ordering are examined leading to assessments that 'the chronological accuracy of the seriation produced' by CA (p.420) is 62% and 67% respectively, the latter being described as an 'obvious improvement'. Another way of putting this is that, using either approach, about a third of the dated graves are not represented with 'chronological accuracy' in the sequence.

[6] That is you need to view the dots and lines together, along with a careful reading of the text, to see what is going on. It would have been much simpler to re-colour the two dated graves 'moved' from z to y2 and insert a boundary between x and y1. In e-Figure 7.3 graves appear to be re-coloured so it is not obvious why this is not done in the printed text.

[7] On a point of statistical pedantry the results of the chi-squared test on p.416 should be expressed as 'the two samples are not significantly different at the *5% level of significance*' rather than 'at the 95% confidence level'. This kind of 'misuse' – though the reader will doubtless understand what is intended – occurs elsewhere in the book (e.g., p.299, p.424, p.426). This is interpreted as implying that the sample of dated graves and the sample of seriated graves are 'not significantly different' but a weighted estimate of the distribution is produced anyway because the proportion of dated graves sampled in phases looks different (i.e. the result of the chi-squared test appears to be ignored). I haven't, at the time of writing, been able to reproduce the results of chi-square tests in the text but will comment more on this below with an example in the discussion of regionality.

The authors take on this is that the 'results demonstrate the broad chronological validity of the sequence'. This is discussed in more detail in Appendix 1 of this paper, along with a suggestion for a simpler method of measuring 'chronological agreement', discussed also in the Appendix to the paper on the male graves.

3.4 Book Section 7.5, pp.422–449

3.4.1 Book Section 7.5.1, pp.424–431

The prelude to this section (pp.422–424) addresses aspects of the representativeness and reliability of the sample of dated graves. It is concluded that the artefact-types in the dated sample are 'probably representative of the artefact-types' in the seriated data matrix (p.422), but that there is a serious 'imbalance' between the regional distribution of dated graves and those within the sample as a whole (p.424)[8].

There are, additionally, and unlike the male graves, regional differences. Regionality is explored by carrying out separate CAs for each of the three regions and fitting Bayesian models to each. The main finding is a discontinuity of furnished burial in the Saxon graves for which only two phases are identified, as opposed to three phases for the other two zones. This can be seen, perhaps more neatly and economically, simply by labelling graves in the seriation by cultural zone as in Figure 1 below.

It is readily seen that the Saxon graves lie in the earliest and latest parts of the seriation with a clear gap in the middle. The other noticeable features are that the earliest part of the sequence is dominated by Kentish graves and that burial in the three zones tends to end at about the same time. This kind of thing can be seen from the estimated phase boundaries for the Bayesian models of Figures 7.74, 7.76 and 7.79 summarised in Table 2 below. Note that the phases p, q, r for the Kent and Anglian graves, identified in the book, are not coincident, nor are they coincident with phase p and r identified for the Saxon zone. None of these phases are exactly coincident with the finer phasing defined using all the data; the relationship to this finer phasing can be seen in Figure 1.

Figures	Cultural Zone	p start	p/q boundary	q/r boundary	q end
7.73, 7.74	Kent	*cal AD 475–540*	*cal AD 545–610*	*cal AD 590–650*	*cal AD 655–695*
7.78, 7.79	Anglian	*cal AD 530–565*	*cal AD 545–575*	*cal AD 585–645*	*cal AD 630–695*
7.75, 7.76	Saxon	*cal AD 460–570*	–	*cal AD 640–665*	*cal AD 655–680*

Tab. 2: *95% intervals for start and end dates of the sequences for female graves within cultural zones. For Saxon graves only two phases, p and r were identified so the boundary is p/r rather than q/r.*

3.4.2 Book Sections 7.5.2 to 7.5.7, pp.429–449

Section 7.5.2 (as with the corresponding section for male graves) concluded that there is a need for 'project-specific calibration when attempting to produce chronologies accurate to within a decade or two' (p.429). I will use Section 7.5.3 on 'uninformative' prior information as a peg on which to hang some thoughts, in Section 4 and Appendix 2, of a more general and statistical nature. The main immediate points to make are that the use of variable-rate modelling is explicitly noted

[8] This is pretty obvious just looking at the proportions of dated graves, (22%, 32%, 10%), within three cultural zones (Anglian, Saxon, Kent). The chi-squared test cited on p.424 isn't really needed to 'confirm' the imbalance, but I can't reproduce it so will spell out what I did. The three cultural zones have a total of dated graves given by (27, 19, 12) and of undated graves by (96, 41, 105), giving 300 in total. Combine this into a table of two columns and call it `data`, then using `R` software `chisq.test(data)` or the equivalent `prop.test(data)` produces a chi-squared value of 12.6 which differs from the value of 9.9 given on p.424. The use or otherwise of Yates correction does not affect this; I explored the possibility that the 9.9 was based on using totals for all the graves as opposed to just undated ones, but still do not get the same value. It happens in this case that the assumption of 'geographical representativeness' can be rejected – 'at the 1% of significance' if properly reported – in both cases, so the conclusions are the same, but rejection is more emphatic with my numbers.

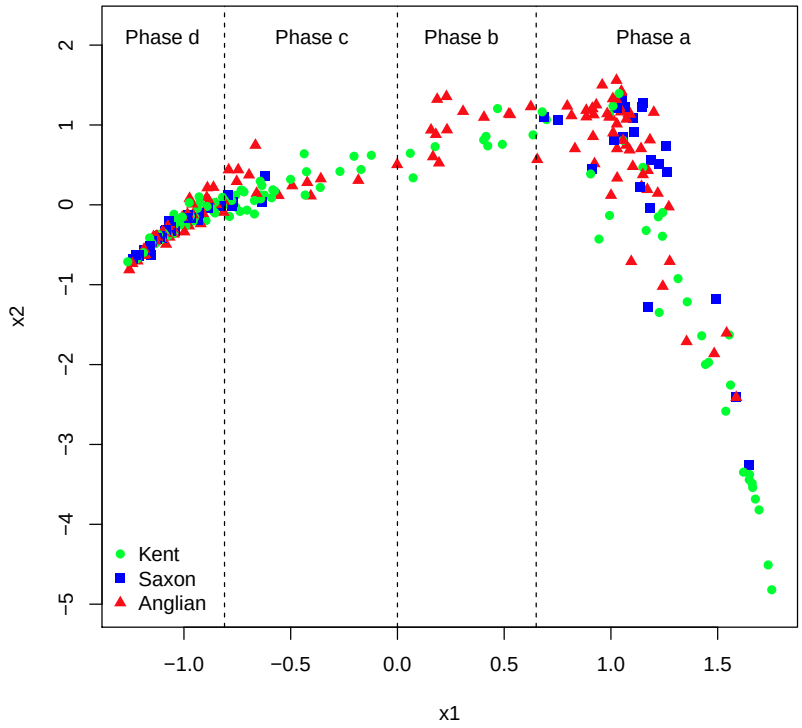

Fig. 1: *The preferred correspondence analysis for the female graves labelled by cultural zone.*

in the first paragraph of the section (p.432) and that the possibility of a 'complete break in the sequence of furnished graves' is 'implausible'. I will reserve comment on the reasoning leading to this for Section 4.

Section 7.5.4 (pp.432–436) includes Continental parallels with calendar dates concluding, much as with the male graves, that the Continental data are 'completely compatible' with the dating suggested by the English sequence but that 'the model shown in Figure 7.65 is preferred as this keeps the English sequence as independent as possible' (p.436).

Section 7.5.5 (pp.436–438) adds to the primary data-set a grave-assemblage with dated coins that was originally 'cautiously excluded' from it (p.436), it being concluded that the coin dated assemblage is compatible with the model developed. This section might be viewed as a distraction, albeit a short and minor one. Had it not been found compatible the reasons for the original cautious exclusion would, presumably, have been judged to be sound.

There are 10 grave-assemblages in the seriation with coins with 'numismatic' dates; five of the graves additionally have radiocarbon dates, the other five not. Section 7.5.6 (pp.438–440) investigates how these fit into the preferred chronological model. The short answer is that some of them don't – in the authors' words there is an 'incompatibilty' between the preferred chronology of Figure 7.65 and 'current numismatic dating' with the latter being 10-15 years later. This, the subject of much more detailed discussion in Chapter 9, points to possible 'inaccuracies' in the chronological model which, as I understand it, have yet to be resolved.

Use of ISOURCE as opposed to terrestrial modelling, discussed in Section 7.5.7 (pp.440–449) on dietary effects, would push some of the model dates later without, however, resolving this issue. In fact, the results from the ISOSOURCE model that incorporates the coin dates are described as reinforcing 'doubt about the accuracy of the existing numismatic coin-dating of these coin types' (pp.443). Otherwise the analysis follows much the same route as that for the male graves,

reaching more-or-less identical conclusions expressed in identical words (pp.447–448). To wit, on 'archaeological and historical grounds' there is 'little reason to state a preference between' the terrestrial and ISOSOURCE models (p.448), but the former is preferred for reasons the logic of which I queried in the paper on male graves, and will not repeat here.

3.5 Book Section 7.6

As with the corresponding Section 6.6 for male graves this section revisits the dating for individual artefact classes *after* the CA. As with the male graves I shall comment only briefly. The (relative) chronologies are derived directly from the first axis of the CA and are 'unlike' the chronologies that have been influenced by the radiocarbon dates. For some artefact-classes, particularly bead-types (Figure 7.88 and Table 7.18), quite complicated models are developed for the implied temporal relations and this informs the subsequent modelling of the dated graves. The modelling adopts the uniform-rate assumption, not considered to be 'entirely appropriate' because of the 'strong bimodality of the assemblage of dated female graves' (p.450). This comment occurs in connection with the bead-types but also affects the analysis of pendant types, so that the models do not show any highest posterior density intervals.

I must confess to being a little puzzled by this section, since the 'angst' induced by violation of the uniform-rate assumption, on display in earlier sections of the chapter, is much less in evidence here. For two of the main artefact classes, beads and pendants, it is concluded that date ranges for types are not 'incompatible with the dating inferred from the Bayesian chronology produced for the overall female seriation' (p.452 and p.452).

4 Concluding thoughts

Having commented that I thought reader comprehension of the book would have been aided had some sections been placed in an appendix I am following my own advice by placing my more detailed concluding thoughts in Appendix 2. They are lengthier than originally intended. The thoughts summarised here can be pursued in more detail in the appendix.

4.1 Precision

Analysis in the book seems to be driven by a desire for 'precision' that is possibly unattainable. This manifests itself, for example, in the desire to include as many dated graves in the preferred Bayesian models as possible; a preference for terrestrial over ISOSOURCE calibration; a preference for uniform- over variable-rate models for the male graves; the concern with continuity of deposition of the female graves and so on. Competing models give different date ranges so that an unequivocal preference for any one model is not possible. Therefore the precision associated with any single preferred model overstates the 'true' precision.

The authors are aware of this – witness, for example, the discussions on pp.468–473 and in Section 10.6.1 (pp.554–558) – but these more 'reflective' commentaries aren't always mirrored in some of the commentary in Chapters 6 and 7. Had 'imprecision' in estimation, associated with the lack of an unequivocally 'best' model, been acknowledged at an early stage some of the sometimes repetitive commentary in the central chapters could have been avoided.

4.2 On the continuity of burial

I can see where the concern with the continuity of burial in the female graves comes from, but think the emphasis and analysis devoted to the question is unnecessary. From the evidence of Table 7.1, from the simulations of Section 7.2, and from the discussion there of chronological frameworks established by other archaeological studies, the authors might legitimately have concluded at the end of Section 7.2 that the evidence for continuity of burial *for the sample as a whole* was reasonably overwhelming.

Possibly of more concern is the clear evidence of regional differences in the female burials. This can be inferred from the seriation without too much need for Bayesian modelling (see Figure 1 above). On p.76 the importance to Bayesian modelling of assuming that a group of radiocarbon dates are 'archaeologically related' is stressed. The question then arises about whether the clear regional differences are sufficiently great to cast doubt on this assumption. Regional modelling is undertaken in Section 7.5.1 (pp.424–429). The results (see Table 2 above) are less 'precise' than those obtained if the 'archaeologically related' assumption is used, but they are also rather different in places.

4.3 A four-phase model for female graves?

Figure 7.68 (p.418) shows the (weighted) estimated distribution of the burials within the four phases. It is asserted that 'the distribution of dated graves in each phase is relatively even', which to my eye is not obvious. From Figure 7.68 you might read phase b as just the right-hand tail of phase a, and phase c (in particular) as the left-hand tail of phase d. That is, you could posit a two-phase model allowing the phases to overlap.

On seeing Figure 7.1 and the bimodality of Figure 7.3 (p.346), a thought was that on this basis alone it would have been possible to divide the sampled graves into two groups and, without worrying initially about seriation, produce two separate models assuming uniform rates *separately* for the two possibly overlapping groups. I am guessing that results compatible with the end-date obtained for the preferred model in the text would be obtained but with less precision.

For this simpler model how might you obtain dates for the overlapping middle that includes much of phase b and c in the more 'exact' analysis? Chronologically sensitive artefact-types can be identified from the seriation. You would know, from the Bayesian modelling of the two phases separately, the kind of dates to associate with the chronologically indicative artefact-types. It's not possible to pin a 'precise' date range to these but you have some idea. Would these be, in reality, that much less 'precise' than the dates implied in the text?

4.4 An alternative conceptualization of the analysis

The overtly 'iterative' and 'relexive' approach to analysis in the book means that the Bayesian modelling is intended to both inform and validate the seriation as it develops. I'm not sure that Bayesian modelling is of overriding importance in developing the seriation, except at the margins. That is, much of the seriation might be developed iteratively, as in the text, without reference to much of the Bayesian modelling described. I am guessing that such a 'front-loaded' seriation could then be refined using Bayesian models with much the same end results as in the text. That is, you could view the more fundamental purpose of the Bayesian modelling to be the provision of calendrical date ranges for phases, derived from a seriation developed without initial reference to the 'absolute dating' evidence.

I present this as a 'conceptualization' of how the analyses might have been undertaken, with the advantage that many of the intermediate analyses described in the text might then be avoided. Regardless of what was actually done I've suggested that a reader of Chapters 6 and 7 might usefully employ this kind of conceptualization to 'validate' a reading that omits large parts of the detailed 'narrative descriptions' provided of the analysis. Such description, to repeat an earlier analogy, is 'scaffolding' needed by the constructors but a distraction for viewers of the construction.

References

Bayliss, A., Hines, J., Høilund Nielsen, K.H., McCormac, G. and Scull, C. 2013 *Anglo-Saxon Graves and Grave Goods of the 6th and 7th Centuries AD: A Chronological Framework.* London: Society for Medieval Archaeology.

Appendix 1

On the chronological accuracy of the seriation

A strict chronological ordering is obtained in two ways. The first method is based on values of the seriated graves on the first CA axis; that is the data points in the 2-D CA plot are *projected* onto the first axis. The second method of ordering involves projecting some graves onto the second axis and others onto the first, a perfectly reasonable approach. Given a reasonable seriation another method might be to fit a smooth regression model through the data points and obtain a relative chronology by projection onto the smoothed curve. I do not recall seeing such an application in the literature, but believe the latest version of Torsten Madsen's `CAPCA` program, an `Excel` add-in for CA, allows something like this. As described (http://www.archaeoinfo.dk/) second-order polynomial fits are used. Non-parametric regression models would be more flexible since they provide an informal way of assessing the appropriateness of a polynomial fit (the ideas are the same). I understand Madsen's software was used for the book, but the latest implementation will not have been available.

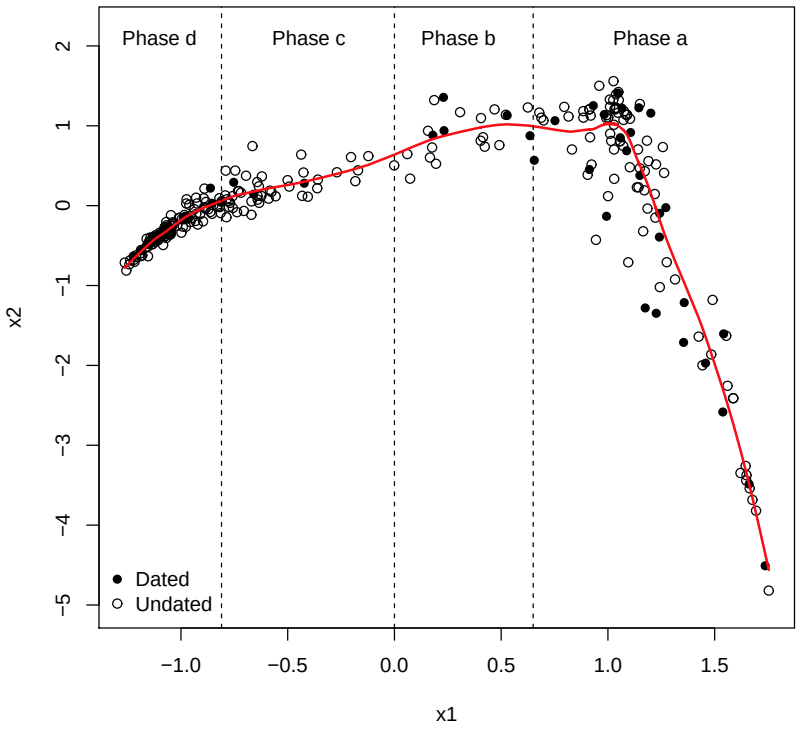

Fig. 2: *The preferred correspondence analysis for the female graves labelled by whether or not a grave is dated, with a fitted non-parametric regression curve through the data.*

Figure 2 is a CA based on data for the preferred seriation taken from e-Figure 7.3. Points are labelled according to whether or not the graves are dated. The line drawn through the points is based on a non-parametric regression fit[9]. The fitted curve approximates to two straight lines joined by an 'elbow'. They are sufficiently aligned with the two axes of the plot to justify the

[9] I used the defaults for the loess method implemented by the `loess` function in `R` except that the `span`, which controls the level of smoothing, was set to 1/4.

second method of inferring a strict chronological sequence from the data, but also indicate why the 'chronological accuracy' is not especially high.

The idea behind the development of the index of chronological accuracy is to project the dated graves either onto the fitted curve, onto the first axis, or onto one of the first two axes depending on the position in the seriation. The problem is that the strong bimodality of the date distribution leads to strong 'bunching' of the graves (dated and undated) near the elbow of the curve and at its left end. It is possible to obtain a strict chronological ordering, but in the areas of bunching this may be highly sensitive to small 'perturbations' in the data points and good 'chronological agreement' with a fixed ordering produced by any other method, itself subject to 'perturbation', need not necessarily be expected. This is illustrated in Figure 3 where the rank-order obtained from the first axis of the CA[10] is plotted against the rank-order inferred from the ordering of grave-assemblages in the preferred Bayesian model of Figure 7.65.

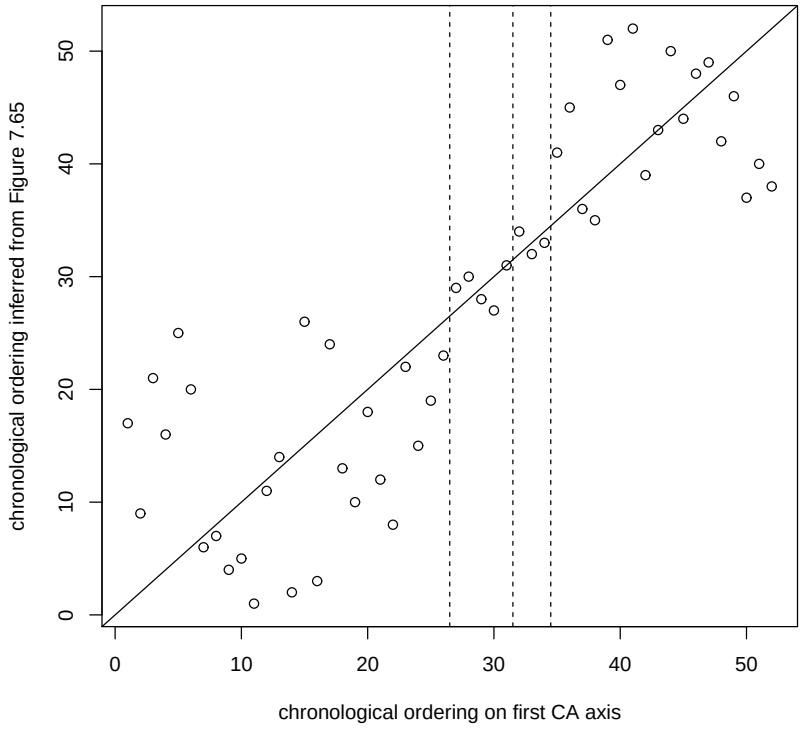

Fig. 3: *The chronological rank-order of grave-assemblages inferred from Figure 7.65 plotted against the rank-order inferred from the first axis of the preferred correspondence analysis. Dashed lines indcate the phasing a–d reading from left to right.*

The solid line is that on which points should sit were the chronological accuracy perfect. The dashed lines indicate the phasing; that for the Bayesian model is determined by that for the seriation so the rank-orders are not independently determined[11]. Distances between phase boundaries cannot be interpreted in terms of calendrical dates – they simply reflect the number of dated grave in each phase.

What is most evident is the scatter about the ideal line in the earliest and latest phases. This is just a reflection of the strong 'bunching' of dates within these phases. Given that both the

[10] This is for illustrative convenience; the rank-order obtained by any of the other methods suggested could be used.

[11] That is, and for example, rank-orders in phase b are constrained to be greater than those in phase a, etc.

seriation and the Bayesian models are subject to what may be loosely described as 'error' a close correspondence between the rank-ordering is not to be expected in these regions. A natural way of measuring the 'agreement' between the chronological orderings is Spearman's rank-correlation coefficient which is 0.82, which is much simpler to determine than the method described on pp.416–422.

This method described on pp.416–422 involves fitting a Bayesian model subject to the constraints implied by the strict relative chronology; identifying and removing the graves most obviously not in 'agreement' with the model; and repeating this process until the graves that remain are in reasonable agreement. For the chronology inferred from the first axis of the CA this involves, if I've counted correctly (p.418), seven iterations leaving 32/52 or 62% of the graves to survive. This measure of chronological accuracy cannot be directly compared with the rank-correlation coefficient of 0.85. This latter value is better expressed, as a measure of accuracy, in terms of it's square which can then be re-expressed as 72%. Readers familiar with regression analysis will recognise this as the R^2 value that would be obtained if a regression analysis were to be run, of one ordering against the other.

Appendix 2 – more detail about 'general thoughts'

Precision

A thought is that some the analysis in the book (or its presentation) is driven by what one might call a concern with 'precision'. This is worthy but possibly leads to unrealistic expectations in places.

1. The more dated graves that can be included in a Bayesian model the more precise will be the estimates of phase boundaries, so a quite explicit effort is made to include as many dated graves as possible in the final models. The potential danger is that some graves may be 'forced' into the model when they don't really fit, possibly only after the artefact typology is modified, or 'anomalous' artefacts are removed from the grave-assemblage[12].

2. Other things being equal the more phases you have the better, provided they are compatible with a model. The potential danger here is of 'straining' to create more phases than the data merits. I have wondered if this might be the case with the female graves and elaborate on this below.

3. Both for the male and female graves, results from the terrestrial calibration are preferred to those from ISOSOURCE, even though arguments on archaeological and historical grounds do not allow discrimination. The preference seems to be driven by the perception that the terrestrial results are more 'precise' even though logic dictates that they must, to some extent, be 'inaccurate'. The ISOSOURCE results may be less precise but may also be more 'accurate'. This is conceded, but not emphasised, at various points.

4. For the male graves the authors conclude there 'is no statistical reason to prefer' the variable-rate model (p.301). This is another judgment based on precision. Mathematically a uniform-rate model is a special case of a variable-rate model so theoretically cannot be better and, in principle, can be somewhat worse. The judgment between them therefore rests on practicalities – do the results from the two models differ much, or is the more complex model compromised by a loss of precision in estimation? There are some differences between models, and the variable-rate model pushes some dates later. For the female graves circumstances force the adoption of a variable-rate model.

[12] I'd like to emphasise the word 'potential' here; the authors are scrupulous is describing how individual graves are dealt with so the information is there for anyone – and it is an archaeological judgment – to challenge any particular decision.

On continuity of burial and the female graves

The advantage of the uniform-rate model is that, if true, it means that the distribution of burials across time is rather 'flat' (e.g., Figure 6.3, p.240, for the male graves). It is a convenient assumption to be able to make for the software used but has the important consequence that it is easier to 'chop-up' the time-scale involved into a sufficient number of phases to make the Bayesian modelling more 'interesting' and informative.

With the very strongly bimodal distribution apparent for the female graves (Section 7.2) an immediate division into two phases is obvious, but further sub-divison is more problematic. Much of Section 7.4 can be read as an heroic struggle to both achieve and justify such a sub-division. The opening to the concluding Section 7.7 of the chapter (p.454) implies as much – 'In the face of considerable and valid doubts, we conclude that a continuous sequence of female artefact-types, and of female burial with grave goods, has been traced across the 6th and 7th centuries in Anglo-Saxon England'.

Whether such 'considerable and valid doubts' were really necessary is something I have wondered about. It is the last quarter or so of the 6th century and first third or so of the 7th century where any discontinuity occurs. If one returns to Table 7.1, before modelling has taken place, and looks at the 68% confidence intervals there are about a quarter of the graves that contain dates in the period cal AD 575–600, some of which include this entire range. A similar exercise can be undertaken with a similar kind of result for dates from the early 7th century. By no means all these graves find their way into the central phases after seriation and modelling but, informally, it seems to me that sufficient graves have a sufficiently non-zero probability of being in the less densely populated regions for it to be unlikely that any convincing statistical evidence would be forthcoming in favour of a clear break in furnished burial.

This conclusion is independent of both the seriation and subsequent Bayesian modelling, and some of the latter might have been avoided if continuity had been accepted at the outset. The treatment of the distribution of graves inferred from the work of Geake and Brugmann, summarised on p.355 in Section 7.2, concludes with the statement that the authors are happy to 'concur with Geake and Brugmann ... in defending the rationality of the premiss that our data-matrix does indeed contain a continuous sequence of grave-assemblages'. They do not immediately reconcile their bimodal distribution with the rather 'flat' ones of Geake and Brugmann. Section 7.4.2 can, I think, be read as rejection of Brugmann's dating and thus of the 'flatness' of the distribution of furnished burials through time, but not of a rejection of the continuity that this flatness implies.

The simulations of Section 7.2, designed to investigate the underlying form of distribution needed to obtain the observed data, are a nice illustration of the use of this kind of methodology. The conclusion to this analysis, at the end of p.355, amounts to saying that the simulations could not rule out the possibility of a break in the continuity of furnished burial. I think this conclusion is both over-cautious and over-scrupulous. To my eye the most pertinent comparison is between the results presented in Figure 7.8b (p.351) and Figure 7.7c (p.350). Had it been concluded on this basis that the evidence was overwhelmingly in favour of continuity, albeit with a diminished rate either side of AD 600, I would not have demurred.

A four-phase model for female graves?

The point about the above is that it might have been easier all round had the authors unashamedly opted for continuity near the outset. Other aspects of the modelling might also have been eased – the suggestion in this section is tentative. Table 3 lists results for the estimated phase boundaries – 95% highest posterior density intervals and *cal AD* – for the preferred (terrestrial) model of Figure 7.65 and what I think is the 'competing' ISOSOURCE model of Figure 7.64[13].

[13] I think there are some problems with the captioning of both figures and with Figure 7.62a, the preferred CA, to which both captions refer. The caption for Figure 7.65 refers to 'phase boundaries shown in purple on 7.62a' while that for Figure 7.84 refers to 'phase boundaries shown in brown and red on 7.62a'. The colouring and number of phase boundaries noted in the two captions do not correspond; the colours mentioned do not correspond to those in Figure 7.62a; and for the four-phase model used you would expect reference to three phase boundaries. See also the discussion of Section 7.4.4 in Section 3.3 above where some problems with the presentation of Figure 7.62a are

Model	Calibration	Start a	a/b	b/c	c/d	End d
Figure 7.65	Terrestrial	*510–545*	*555–585*	*580–640*	*625–650*	*660-680*
Figure 7.84	ISOSOURCE	*530–570*	*560–600*	*605–655*	*630–660*	*665-695*

Tab. 3: *95% intervals (cal AD) for boundaries for the sequence of female graves, for different calibration curves.*

The dates for the ISOSOURCE calculations are later than for the terrestrial calculations (as expected) and the date-ranges are mostly greater. If you concede (as the book occasionally does) that the alternative ISOSOURCE model cannot be ruled out then it seems obvious that date estimates have to be rather more 'uncertain' than they are sometimes treated. That is, and for example, the end date might plausibly be between 660 and 695 if you take the limits of the two 'competing' estimates. The 'uncertainty' associated with this combination of the two possibilities can't be neatly encapsulated in a comforting and numerically 'precise' statement like a 95% higher posterior density estimate. You don't have a 'tidy' result and this is possibly why a preferred model is 'promoted'. This can be interpreted as a concern with precision, but does not do due justice to the real degree of imprecision involved. The issues are raised in Section 10.6.1 of the book, but could be emphasised more in the earlier stages.

The other point to note is the mostly greater lack of precision at the boundaries associated with the central phases b and c. It might be remembered that such 'precision' as can be associated with the c/d boundary was achieved by transferring two dated graves that more naturally belonged in phase d to phase c, in order to 'populate' phase c with more dated graves. This, and the general lack of precision, makes one wonder how strictly necessary and valid the central phasing is.

I shall take as the basis for further discussion Figure 7.68 (p.418) which shows the estimated distribution of the burials within the four phases, weighted to allow for the fact that the dated graves are not entirely representative of the graves included in the seriation. This is a rescaled version of Figure 7.67 (p.415) which does not change the shape of the distributions within phases. It is asserted with respect to Figure 7.67 that 'the distribution of dated graves in each phase is relatively even', presumably intended to imply that the assumption of a uniform-rate in each phase is reasonable. To my eye this assertion is by no means obviously justified, though I will not press the point as I wish to pursue a slightly different line of thought[14].

In Figure 7.68 you might read phase b as just the right-hand tail of phase a, and phase c (in particular) as the left-hand tail of phase d[15]. The posterior densities for phases c and d overlap. Timely completion of the project was hindered by computational problems that involved, among other things, several weeks or even months being needed for some of the Bayesian modelling. I am assuming that this was because of the bimodality of the date distribution for furnished female graves; because of the assumption of continuity; and because of the desire for 'precision' (i.e. to include as many graves in the modelling as possible). Hindsight is a wonderful thing, but given Figure 7.1 and the bimodality of Figure 7.3 (p.346), reinforced by the above observation, I have wondered what would have happened had the sampled graves been divided into two groups to produce two separate models assuming uniform rates[16].

It would surprise me if this was not experimented with but I'd like to have seen the results. There is a fairly obviously later group of graves and a fairly obvious earlier group with little or no overlap in their probability distributions. There is an intermediate set of 10–12 graves whose

noted.

[14] Phases a and d have (for practical purposes) quite sharply unimodal distributions, and those for b and c are quite clearly skew. None of these can really be described as 'relatively even' as I suspect this term would normally be understood. It is stated elsewhere in the text that simulation has shown that models are robust to departures from the uniform-rate assumption. Give the concern that has been shown elsewhere about bimodality you would need to add to the assertion about robustness a caveat along the lines 'so long as the true distribution is unimodal' to sustain the use of the term 'relatively even'.

[15] Remembering that phase c was partly defined by transferring two graves to it that were initially and more obviously phase d.

[16] This differs, I think, from those analyses where such a separation is posited but the graves are included in the same model. Two-phase models are experimented with at various stages of the proceedings but, unless I have missed something, the models explored are based on sequential phases that may or may not be abutting.

allocation might have been experimented with and which might have been allowed to belong to both groups. I'd surmise that this would have been computationally less demanding and easier to report so the issue is whether much would have been lost in the way of precision.

Of particular interest is what the estimate of the end date (of furnished burial) would be for the later group – a concern in Section 8.2.2 (pp.464–473). The first model after seriation is started, using bead-types, produces a 95% interval for the end-date of *cal AD 660–685*. All that changes as other arterfact-classes are added is that the upper limit changes by 5 years to 680. I am guessing, and prepared to be wrong, that results compatible with this would be obtained but with less precision. A little additional speculation on dating with this simpler model is provided in Section 4.3 above.

An alternative conceptualization of the analysis

The four-phase model in the book, based on the seriation, offers a more precise dating scheme. I've wondered above what you lose if you accept the bimodality and do not abandon the continuity assumption, but proceed nevertheless with separate modelling of two groups without the modelling in one group informing that of the other. The role of the seriation, however developed, is to produce a chronological ordering that can be used to develop a relative chronology based on artefact-types. The Bayesian modelling informs and validates the seriation as it develops; I'm not sure that it is overridingly important in developing the seriation, except at the margins where individual grave-assemblages or artefact-types are included/excluded or re-defined in the interests of including as many dated graves as possible in the seriation.

That is, you could view the more fundamental purpose of the Bayesian modelling to be to provide calendrical date ranges for phases, derived from a seriation developed without initial reference to the 'absolute dating' evidence[17]. The point of the footnote is to suggest how this might be done explicitly; while it might still be described as an 'iterative' and 'reflexive' process it involves a different conceptualization of the methodology likely – I suspect – to have resulted in much the same outcome. The advantage of this conceptualization is that it is more simply described than the form of iteration presented in the text. I have suggested that the text could be read from the perspective of this conceptualization, allowing the reader to bypass much of the detailed exposition attached to the intermediate Bayesian models presented.

[17] This is not at all how the research was undertaken or is presented. The idea would be to select a sample of graves covering the chronological range of Anglo-Saxon burial of concern (more-or-less what was done); develop a typology (what was done); establish a seriation based entirely on the typology (not what was done as presented, but perfectly possible); select a sample of graves 'evenly spread' throughout the seriation, both to check on its validity and provide 'absolute' dating (not a practical proposition perhaps, but conceptually possible). For practical purposes it's likely there would be little doubt that the seriation has a chronological interpretation (archaeologists know about this kind of thing), so the main point is the dating. With the dating evidence in place the seriation is then re-visited and revised according to the principles adopted in the text. The main difference from what was actually done would be the 'front-loading' of the CA with the Bayesian modelling then serving as a 'refining process' after completion of the CA.