

The bishops and monks of Rochester: five years on*

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The bishops and monks of Rochester (Flight 1997) was published five years ago, as volume 6 in the KAS's Monograph Series. Somewhere in that book, I mentioned the possibility that I might write a supplementary paper, five years into the future, to put right any errors which I might have detected or had brought to my attention by then. The present article is, in a sense, the fulfilment of that prediction; but so much has been happening over the last few years that I have thought it best to let the scope of the article expand. For the reader, I hope, the result will be a more interesting piece than the one which I originally thought of writing.

Since about 1994 (which is when I stopped reading other people's publications and started writing the final version of the book), a surprising amount of new work has appeared in print relating to the history of Rochester Cathedral. Apart from my own, two books have been published in the last few years – a collaborative history edited by Yates (1996) and a monograph by McAleer (1999). A number of other recent books and articles, though not exclusively concerned with Rochester, are important in advancing our knowledge of some aspects of the church's history. Some of the points arising from these publications are commented on below, so far as they overlap with the relatively short span of time covered by my book.

Furthermore, since several years earlier than that (in particular since 1986, when he became the Dean and Chapter's archaeological consultant), some very useful work has been carried out by Tim Tatton-Brown, most notably in connection with the cleaning and conservation done recently on the two Romanesque façades – the one at the west end of the church, and the one along the eastern side of the cloister. At the time of writing, I have not seen any final report on the facts discovered and recorded in the course of this work. Those publications which I have read

*((This article was written in 2002 and submitted for publication in *Archaeologia Cantiana*. For reasons unknown to me (good ones, no doubt), the editor kept postponing this paper from one volume to the next until finally I thought that it had ceased to be worth publishing – barring some drastic revision, which I did not have time for. I withdrew it, therefore, and am sure that I was right to do so. But it does have some good points; so I make it available here, in a somewhat shortened but not updated form.))

are mostly of an interim nature,¹ and the interpretation, as far as I can judge, is still in an inchoate state. That Tatton-Brown's work will have an important impact on our understanding of the church's structural history is clear enough already, but it seems that the time has not yet arrived for an attempt to assess the implications point by point. In any case, Tatton-Brown is entitled to do that first himself.

Reading through my book again, after an interval of five years (during which I have consulted it only occasionally), I have found many flaws.² For the most part, the flaws are small and isolated, and should not cause any serious trouble. In writing the present paper, I have passed over any errors which seem to me innocuous, hoping that the reader will do the same. The errors which are noted in the following pages are those which are important in themselves, or which have some ramifications.³ All that I aim to do here is to make some corrections, to supply some omissions, and to stress a few points which – in the light of other recent publications – seem to be at risk of being overlooked or undervalued.

To keep this article short, I have had to assume that anyone who reads it will have access to a copy of the book. For the same reason, references given there have not been repeated: if an author's name is mentioned without a specific publication being cited, it should be understood that the details can be found in the bibliography at the back of *Bishops and monks*. Headless references – such as 'p. 100', 'fig. 10' – are references to the same book. Three manuscripts will be cited by short titles: Domitian = BL Cotton Domitian x, fos. 92–211; Vespasian = BL Cotton Vespasian A xxii; Privilegia = DRc/R1, fos. 119–235. The code DRc denotes the Dean and Chapter records now in Strood; DRb denotes the Episcopal Registry records now in Maidstone.⁴

¹ In fact, they have mostly appeared in the annual reports of the Friends of Rochester Cathedral. Without question, it is a good thing for the Friends to be kept informed of archaeological developments, but perhaps we should remind ourselves that their annual report is not a journal of record, and ought not to be allowed to become one.

² For a start, it has some typographical errors, a list of which I will be glad to send to anyone who writes and asks for it.

³ It has been pointed out to me that there is a mistake in the numbering of some of the paragraphs in Appendix 3 (p. 279), and that some consequential mistakes occur elsewhere. I owe this remark to Margaret Blount, who has noticed a number of other defects, especially in the list of the bishops' charters (Appendix 2, pp. 245–52). But I do not propose to try emending that list, because I hope that it will soon be superseded by the volume of *English Episcopal Acta* which she and Martin Brett are editing – another book which, when it appears, will mark a large step forward.

⁴ Some explanation seems to be needed with regard to DRb/Ar2, *Registrum temporarium*. When this register first arrived in Maidstone, it was mistakenly accessioned (as were the episcopal registers) among the Dean and Chapter records (as DRc/R3). That mistake was soon put right, but its repercussions continue to be felt. So it had better be said again that this book did not originate in the priory: it originated in the bishop's registry, specifically in the registry of bishop Hamo de Hethe (p. 6).

As has been said, a collaborative history of Rochester Cathedral was published in 1996, as part of the Kent History Project (Yates 1996). It covers the entire history of the church, from 604 till almost the present day (till 1994, to be precise), and there are only two contributions which overlap with my book to any large extent. The first chapter (beginning in 604 and ending in 1185) is a survey of the church's history written by Martin Brett; the penultimate chapter (beginning in 604 and ending in 1540) is a survey of its architectural history written by Philip McAleer. This latter chapter (McAleer 1996) has already been superseded by McAleer's (1999) monograph, which will be discussed below; so Brett's chapter alone requires any comment here.

There is much in this chapter that I might wish to have said myself, very little that I would think of disagreeing with. In places the evidence is thin, and leaves room for some difference of opinion. Brett (1996, p. 19) is more inclined than I was (p. 243) to believe in the existence of an archdeacon Anshetil, 'presumably not the same man' as Anshetil archdeacon of Canterbury (whose existence is not in doubt). He may well be right. Among the incoming payments said to have been earmarked by Gundulf for the monks' clothing, this item occurs: 'From the archdeacon, ten shillings' (*Privilegia*, fo. 196r);⁵ a few lines later there is a reference to Anshetil archdeacon of Canterbury, and it seems to be implied by the context that this is somebody else. So it may be true that Gundulf had an archdeacon of his own, whatever the man's name may have been.⁶

On one fairly important point, I feel sure that Brett's reading of the facts is mistaken. That is to do with the church of Saint Mary of Lambeth. Brett follows previous writers in thinking it likely that Lambeth church – with all the lands and other assets which constituted its endowment – was given to Rochester by countess Goda,⁷ and that the grant was confirmed by Willelm II, 'after a long delay' (Brett 1996, p. 13). There is no credible evidence for that, as far as I can see (p. 90). The king's charter (no. 507) represents the gift as one which he is making spontaneously.⁸ It mentions Goda, but only as someone who had owned the church in the past: the new owners are to enjoy the same rights that were formerly enjoyed by

⁵ Many of the donations from which these rents arose are described in more detail in the preceding pages, but this one is not, as far as I can see. Nor can I find an entry which matches this one in any later listing (such as the papal privilege of 1155 or the early thirteenth-century rentals).

⁶ As Brett remarks (1996, p. 9), the title *archidiaconus* occurs once at Rochester in the ninth century, but in that context it surely denotes a status (chief deacon), not an office comparable with that of a twelfth-century archdeacon.

⁷ This is Godgifu, king Eadward's sister.

⁸ A forgery (no. 512) which makes its appearance in the early thirteenth century (p. 91) still uses exactly the same language with respect to Lambeth.

her, and that till now have been enjoyed by the king.⁹ The author of the *Vita Gundulfi* says, and makes a point of saying, that the king gave Lambeth to Rochester out of his own generosity, *propria munificentia*, of his own volition, *ex propria uoluntate sua* (fos. 63v–64r). After 1196–7 the monks ceased to be the owners of Lambeth, but they did not lose interest in it (p. 95). As late as about 1220, the list of benefactors says plainly that Willelm II ‘gave’ Lambeth, not that he confirmed it or gave it back (Vespasian, fo. 82r).

There is no entry here for countess Goda herself. What we do discover is that she had been a generous benefactor to Lambeth church, and that the treasures donated by her were still to be found there when a Rochester monk by the name of Radulf arrived to take possession (fo. 85r). One by one, they were carried off to Rochester – and countess Goda thus became, after her death and against her wishes, a benefactor to Rochester. By about 1230, Goda’s anniversary (10 May) was being celebrated at Rochester, on a par with that of Odo bishop of Bayeux (DRc/R2, fo. 68r). By the 1320s the rewriting of history was complete. Countess Goda, whether she liked it or not, was enrolled among the church’s benefactors, and Willelm II was deprived of the credit that he deserved. She gave Lambeth; he only gave it back (DRb/Ar2, fo. 1r–v).¹⁰

Reading Brett’s chapter, I realize that it was a mistake on my part to think that the bishop’s knights could be left out of the picture. Briefly stated, the situation was this: the bishop of Rochester had to find ten knights for the archbishop, and these ten were counted towards the total of sixty knights which the archbishop had to find for the king.¹¹ Though simple enough in theory, the arrangement has a complicated history (Brett 1996, pp. 21–2).

Before the thirteenth century, the evidence is sparse. A list of the bishop’s knights was added into *Privilegia* by a mid twelfth-century hand (fo. 217r),¹² but I have not been able to make much sense of it.¹³ Even

⁹ She was also the previous owner of Aston Subedge (p. 178), and her name occurs in that context (*Privilegia*, fo. 214v) for the same reason that it occurs here.

¹⁰ And someone, by implication, was accused of having stolen it. This is the version of history taken for granted in what purports to be a charter of archbishop Anselm (no. 211) surviving in what purports to be the original (DRc/T48). That fact alone would prove it to be a late forgery (later than no. 512).

¹¹ Until the late thirteenth century, the bishop held all his lands from the archbishop, none directly from the king. In Domesday Book for Kent, as Brett (1996, 21) points out, the bishop of Rochester is given a numbered chapter to himself, and so seems to be treated as one of the king’s barons, like any other bishop; but that, in my opinion, is an error which crept in during the compilation process. This chapter should really have been another sub-chapter – like those for the archbishop’s knights and the archbishop’s monks – appended to chapter II, ‘Land of the archbishop of Canterbury’.

¹² The same hand, it seems, that rewrote part of the list of parish churches (fo. 220v).

¹³ The first name on the list is Gosfrid Talebot, answerable for one knight. There were two men of this name, father and son, whose descendants held Wickham and Islingham

so, it is clear that there are three points at least which I failed to understand. (i) The manor of Fawkham, which at first belonged to the monks (p. 179), ceased to belong to them because it was given to one of the bishop's military tenants, a man whose descendants took their surname from this place. In the list just mentioned, Godefrid de Facheham is responsible for finding two knights. (ii) The Gloucestershire manor of Aston Subedge was bought by bishop Gundulf from Willelm II (*Privilegia*, fos. 213v–214v). Contrary to what I said before (p. 178), the land was successfully acquired. We are told that at first it belonged to the monks, and that it was managed on their behalf by the monk who was in charge of Haddenham (fo. 214v); but sooner or later this manor too was given to one of the bishop's military tenants.¹⁴ In the same list, Tidbold de Eastuna is responsible for finding one knight. (iii) A charter of Henric I (no. 515) confirming a gift made to the church of Rochester by Gausfrid Talebot was also misinterpreted.¹⁵ The land described here as 'half of Little Wrotham' is sure to be the manor which was subsequently held from the bishop by knight's service, and which in later documents is more often referred to as Trottscliffe (not to be confused with the manor of the same name held by the bishop himself). I cannot identify this holding in the twelfth-century list,¹⁶ but later it was owned by a family named de Cressy (which died out in the 1260s). By Hasted's time, the place was called 'West-court, alias Wrotham-water'.

The charters of Willelm I relating to Rochester can now be consulted in Bates's (1998) edition.¹⁷ (i) His no. 226 is my no. 502. This seems certain to be earlier, not later, than the litigation of 1077×82 recorded in the Isleham memorandum (pp. 177–8).¹⁸ The writ is bilingual – the message is given in Latin first and then repeated in English – and that is probably a point in favour of an early date, not long after Lanfranc's arrival in

(in Strood and Frindsbury respectively) from the bishop; the younger Gosfrid died in 1140. (But his heirs are unlikely to have been able to claim the inheritance till after 1154, and a dead man's name might perhaps be retained on a list of this kind till someone else was firmly in possession.)

¹⁴ Though the charter relating to Aston (no. 511) is suspect for other reasons, the fact that it is absent from the monks' early thirteenth-century cartulary is not to be held against it. Perhaps the compiler chose to exclude this document, because it was no longer of any interest to the monks; perhaps he did not have the chance to include it, because it had been transferred to the bishop's archive; either way, its absence does not signify.

¹⁵ This is the elder Gosfrid Talebot (above, note 13), who died in 1130 or shortly before.

¹⁶ It may not have been held by knight's service at the time. In the list of parish churches, a chapel called *Cap' anfridi*, 'Ansfrid's chapel', is noted as dependent on Trottscliffe (*Privilegia*, fo. 220v). This seems to prove that Little Wrotham was given to the man of that name who started his known career as Gundulf's steward (fo. 188r) and who later served as sheriff of Kent.

¹⁷ Bates's no. 228 should be cancelled. This is just an excerpt from a (spurious) charter of Willelm II (no. 512).

¹⁸ The memorandum is also printed by Bates (1998, pp. 712–14).

1070. It was Lanfranc who benefited directly from this transaction: the king gave Freckenham to him. Then, some time later, Lanfranc gave it to Rochester; and presumably that is when the relevant writ passed into the archive there. Later again, the dispute over Isleham arose. The ‘extended’ version of this text (the version which gives the date as 1071) is my no. 505. This is a late forgery, existing only in Latin. It seems possible that the forger went to work on the original, erasing the English text to make space for the addition that he was intending to make to the end of the Latin one. But the textual evidence needs to be looked at more closely. (ii) The Penenden memorandum (p. 26), printed many times before, is printed once again by Bates (1998, pp. 319–21); he also prints a number of derivative versions (including two from Rochester), all of which, as evidence for what actually happened in the 1070s, seem perfectly worthless to me.

With regard to no. 504 – the charter allowing the gift of Dartford church made to the church of Rochester by Haimo the steward – I failed to think hard enough. It is true that the gift was made before 1086. In Domesday Book, at the end of the paragraph describing the king’s manor of Dartford, the following note occurs: ‘The bishop of Rochester holds the church of this manor; it is worth sixty shillings’ (fo. 2va).¹⁹ That is plain enough. But the gift cannot have been made by Haimo: it must have been made by a king – either by Willelm I or by one of his predecessors. No king can have approved a donation which Haimo had no right to make. Apparently the text has been tampered with.²⁰

2

The surviving catalogues of the monks’ library have all been printed and annotated thoroughly by Sharpe and Watson (1996).²¹ Building on previous work – by Ker, Waller, Richards and others – they aim to identify each book, if it survives; if it does not, they aim at least to match up the corresponding entries in different catalogues, and to identify the authors and works that the entries refer to.

¹⁹ Domesday Book goes on to say that ‘apart from this, there are three small churches there as well’. From the wording of this sentence, it would be doubtful whether the ‘three small churches’ (which I would take to be Sutton, Woolwich and Chislehurst) were also in the bishop’s possession. Not much later, however, the subordinate churches did all belong to Rochester (*Privilegia*, fo. 196v) – to the bishop or to the monks, depending on whose side of the story one listens to.

²⁰ It is possible that something may have been erased. Instead of saying ‘of the church which is in my manor of Dartford’, perhaps what this charter originally said was ‘of the church [of Woolwich?] which is in my manor of Dartford’. That Haimo may have had a claim on the ownership of Woolwich church is suggested by another entry in Domesday Book (fo. 14rb).

²¹ Four authors’ names appear on the title-page, but it appears that only two of them had anything to do with the Rochester catalogues.

One of the catalogues they edit (DRc/Z18, ed. cit. pp. 493–6) had not previously been printed as a separate text. It consists, in fact, of just two butterfly-shaped fragments,²² cut from a sheet of parchment which happened to have this catalogue written on it (on both sides). To the extent that they can be compared, this fragmentary catalogue is very similar to the one in *Privilegia* (ed. cit. pp. 471–92), so much so that it is doubtful which is the later of the two (ed. cit. p. 469). It differs in being a single sheet, headed with the word *Chirographum*. In other words, it is an inventory, written out in duplicate. One part, we may suppose, was given to the monk who would be serving as librarian, and the other part was kept by his superior, the precentor or perhaps the prior.²³ Hence, when the scribe who compiled *Privilegia* decided that he ought to include a catalogue of the monks' library (p. 29), all he had to do was to copy the current version of this chirograph.

A new catalogue was compiled in 1202 (ed. cit. pp. 499–526).²⁴ The first page of this was reproduced by Thomson (1969, pl. 88), and (as I ought to have noticed before) two of the entries appearing here were added by a recognizable hand – in fact by the hand of the ubiquitous Vespasian scribe (pp. 71–3). Now we have a facsimile of the second page (ed. cit. pl. 6), and the Vespasian scribe occurs there too.²⁵ Though Sharpe and Watson do not say so, this is the same scribe who (in a more careful manner) wrote the list of benefactors in the Vespasian register – the list from which they edit a selection of entries, those that relate to books and the care of books (ed. cit. pp. 529–31). In the 1202 catalogue, the marginal note which they print as a heading to one section of the text (ed. cit. p. 523) reads like a later comment, added by someone who wanted it to be known that the man who rearranged the library and wrote this part of the catalogue was the former precentor, Alexander. If that is what he meant, and if he was right, scribe β perhaps has a name. Alexander was a local man, belonging to a Rochester family: his ancestry is recorded (see below).²⁶

²² They owe their shape, and their survival as well, to the fact that they were used for making seal-bags.

²³ Sharpe and Watson assume – I do not quite see why – that the chirograph was tripartite (ed. cit. p. 493).

²⁴ The date in the heading is *m^o cc^o ii^o*, misprinted here as *m^o ii^o* (ed. cit. p. 499).

²⁵ The additions he made on these two pages are entries 23, 49, 121–4. All are assigned by Sharpe and Watson (1996, p. 497) to the scribe whom they call γ . (The marginal addition to entry 49 is also his; by the way, he did know how to spell Deuteronomy. Entries 118–20 had been added previously by a different hand, much rougher than the main hand, β .)

²⁶ A few comments may be added relating to points of detail. (i) The name transcribed as *Heth'ame* (ed. cit. p. 501) is *Het'ha'me*, to be read as *Heterhamme*. (This is in one of the entries added by the Vespasian scribe.) The same man, 'Willelm de Eterhamme, monk', occurs in the list of benefactors (Vespasian, fo. 90r). His surname presumably derives from Otterham in Upchurch, spelt *Eterhamme* in the thirteenth century. (ii) 'Master

In their introduction, the editors include a discussion of those puzzling fourteenth-century inscriptions which were written into many of Rochester's books, and which, in some obscure sense, purport to associate a particular book with some named individual. Sharpe and Watson (1996, pp. 465–7) analyse these inscriptions in some detail, proving what Ker said flatly forty years ago – that 'often they cannot possibly be correct'. In so many cases they are certainly wrong that even those which could be right cannot be taken at face value. These inscriptions have caused all sorts of trouble in the past. After Sharpe and Watson's work, one may hope that they will never cause trouble again. But no doubt one should expect to be disappointed.

It is doubtful how far this evidence can be trusted even for the purpose of drawing up a list of Rochester monks – i.e. for the purpose to which it has been put by Greatrex (1997). For example, it was apparently believed, in the fourteenth century, that there had once been a prior by the name of Paul or Paulin (Greatrex 1997, p. 625). As Greatrex herself remarks, that is questionable. There was, we know, a monk named Paulin who served for some time as sacrist. He occurs, as sacrist, shortly after 1107 (p. 199) and shortly after 1108 (*Privilegia*, fo. 198r). He is also included, as Paulin the sacrist, in the list of benefactors (Vespasian, fo. 84r). Comparing that entry with another (fo. 85v), we can deduce that he outlived Ansfrid the sheriff (and Ansfrid's widow too); and Ansfrid remained alive (and in office) till after 1135. So it might be thought that Paulin should be added to the list of priors, where he would help to fill the gap between Ordwine and Brien (p. 241). If that were so, however, the list of benefactors would call him 'Paulin the prior': by calling him 'Paulin the sacrist', it implies that this was (as far as the compiler knew) the highest office that Paulin had ever occupied. Unlike the 'ex libris' inscriptions, this list is a generally reliable source, and I think we have to prefer it, even as negative evidence; but of course the compiler was human and may have erred.

For the twelfth century, most of the names recorded by Greatrex come from this list of benefactors, compiled in about 1220 (p. 74). The most obvious problem here is that these names are adrift in a chronological void. Now and then, with the help of other evidence, it may be possible to pin them down, as in Paulin's case. Sacrists were important members of the community, and, by the nature of their office, likely to qualify for

Hamo' (ed. cit. p. 505) was certainly a resident of Rochester (and so was his uncle Walter). This is proved by two charters of his, both relating to property in the city (Domitian, fos. 162v, 163r); one of them is datable 1182×6. In the list of benefactors he is called 'master Haimo Coc' (Vespasian, fo. 83v). (iii) 'Master Ernulf, prior' (ed. cit. p. 517), is presumably the prior of that name who (as I think I have proved) occurs in the 1170s (p. 241). If that is right, we discover two things about prior Ernulf which otherwise we should not know: he was a university graduate and an occasional poet.

inclusion in this list. Even so, it is doubtful whether we shall ever be able to piece together an organized list of twelfth-century sacrists.²⁷ There was a sacrist named Thalebot, for example, whose good deeds included the making of a bell to which he passed on his name (Vespasian, fo. 87r). Probably his term of office was later than Paulin's; probably it was earlier than Radulf de Ros's (p. 242); but that does not get us far.

Then again, some of the monks who appear in this list were monks in only a superficial sense. They became monks only on the point of death, or in their old age. A man like Helias son of Hamo, who donated a number of properties in and around the city (Greatrex 1997, p. 601, from Vespasian, fo. 84v), was not intending to turn his back on the world until it suited him to do so. He was buying an insurance policy.²⁸ He paid the monks a premium, and the monks in return promised that they would take care of him, when the time came.²⁹ It is worth knowing that transactions of this sort occurred, and we should not assume that the motives at work were commercial through and through. But there is an important difference between a man like Helias on the one hand and a life-long member of the college on the other. In Helias's case, it was part of the deal for his son to be made a monk straight away, and the son, unlike the father, became a genuine monk. His name was Alexander, and he ended his life as precentor, admired and remembered later (see above) for his skill as a scribe.³⁰

From about 1220 onwards, the evidence improves in quantity and quality, and Greatrex's book will be invaluable to anyone working on the period after that. For the period covered by *Bishops and monks*, there is probably not very much that can be said. Some clues are drawn together by Brett (1996, p. 17); my own attempts to make sense of the data resulted in only the most banal of conclusions (p. 200).

The list of priors given in my book (pp. 241–2) is better than any other published list, but not as good as it might be. With regard to prior Reinald (occurring in 1155), it would have been helpful to note that a monk of the same name, presumably the same man, occurs as sacrist in 1143

²⁷ The same is true for the thirteenth century. McAleer (1999, p. 106) is right about this, but not right to say that 'there are no recorded dates' for Ricard de Waldene. There are some dates (Greatrex 1997, p. 645) – later than McAleer would want them to be.

²⁸ Which happens to survive in the original (DRc/T302). It probably dates from about 1150–60. Helias was dead before 1182×6, when one of the properties he had given to the monks was already out on lease: it was next-door to a property owned by master Hamo and his uncle Walter (Domitian, fo. 163r).

²⁹ Master Hamo Coc (above, note 26) took out a similar policy (Domitian, fo. 162v): the benefits were to be 'the same as those enjoyed now or ever by Thomas de Nhessindene or Willelm Turc'. It was possible even for a woman to make an arrangement of this kind (*Privilegia*, fos. 190v, 200v).

³⁰ Greatrex (1997, p. 589) has separate entries for Helias's son and the precentor, but there seems to be no reason not to make the identification.

(DRc/T191/1).³¹ As for the suggestion that Reinald occurs ‘possibly’ in 1160, this should have been relegated to a footnote, or perhaps omitted altogether.³² With regard to prior Willelm, the last of those listed in the book, one further item of evidence should be added. He occurs (as W—) in a document dated 29 May 1223 (Kingsford 1925, p. 82).³³ The dating of his departure from office can thus be sharpened up slightly, to May 1223 × May 1225. (I still think it likely that prior Willelm is the same person as the Vespasian scribe, who was still alive in April 1225 (p. 72), but have nothing new to say on that subject.)

3

With regard to the structural and stratigraphic evidence, some progress can be reported; but some new disagreements have emerged as well, which may not be soon or easily resolved. As far as the earliest (or supposedly earliest) structures on the site are concerned (p. 146), the only progress achieved has been progress in a backward direction. Perhaps it may soon become generally agreed that the ‘second Saxon church’ is a Roman building (McAleer 1999, pp. 12–13), just as Irvine supposed it to be, when he first discovered it, and that the ‘third Saxon church’ is a joke (McAleer 1999, pp. 14–16). To put it bluntly, the last hundred years have been a waste of time. We know nothing more now than was known in the 1890s; probably we know a little less, because I learnt long ago not to trust Livett’s probe as much as he did. The apsidal structure recorded by Livett – who was working, it should be remembered, under very difficult circumstances – is of doubtful date (except that it is certainly earlier than the 1080s), and puzzling in other ways too.³⁴ But the manner in which the foundations of this structure were (so to speak) embraced by those of the late eleventh-century church (fig. 15) is a point which ought to be stressed. It is suggested by McAleer (1999, p. 17) that a larger structure remains to be discovered somewhere further east. That may be right; but I do not see why we should specifically think of expecting to find it under

³¹ Again, Greatrex (1997, pp. 627–8) has separate entries for Rainald *secretarius* and prior Reginald, but we can probably take the risk of identifying them.

³² Looking at the evidence again, I think it very unlikely that ‘brother R. of Saint A. from R. the priest’ should be read as a mangled reference to Reinald prior of Rochester. The letter in question (printed by Barlow 1979, pp. 322–3) does not sound like the sort of letter which prior Reinald might have written. It sounds like the work of a humble monk, speaking for himself alone.

³³ This is also notable as (to my knowledge) the earliest occasion on which a prior of Rochester is known to have acted as a judge delegate.

³⁴ If Livett plotted it accurately (as presumably he did), the apse is not a ‘parabola’ (McAleer 1999, p. 170 n. 17), but something very close to a semi-ellipse. Some parts of this building were exposed again in 1998, when the road was resurfaced. Alan Ward has been kind enough to provide me with copies of this and other draft excavation reports; I thank him for that, and for his comments on a draft of the present article.

the early twelfth-century chapter-house.³⁵ There are many areas other than that where nothing is known of what lies beneath the surface.

Jumping forward in time, we seem to see another consensus emerging with regard to the date of the new presbytery. Hope's ideas on the subject varied over time (pp. 163–5), but the earliest date that he ever suggested was 'circa 1195'. Recent opinion has been pushing the date further back. Tatton-Brown was the first to suggest in print that an earlier date than Hope's was to be preferred. Independently, McAleer and I both arrived at similar conclusions, by rather different pathways. In my reading of the evidence (p. 165), the crucial event was the arrival of bishop Gilbert in 1185. McAleer (1999, p. 85), like Tatton-Brown, suggests that reconstruction was made necessary by a somewhat earlier event, the fire of 1179. That suggestion seems inadequate to me, because it does not explain why the church began to be reconstructed, instead of just being repaired.³⁶ We do not have any clear idea how much damage occurred. Perhaps the fire meant that something had to be done – but why do so much, rather than as little as possible? The writ of protection obtained by the monks from the king (no. 535) might be thought to convey an assurance that no unavoidable expense would be incurred. Someone who was shown this writ – the monks of Boxley, for instance – might agree that they ought to help pay for the church to be repaired (on the understanding that the monks of Rochester would do as much for them, if the roles were reversed); but they would not feel obliged to help pay for the church to be rebuilt.³⁷ I thus hold to the view that the arrival of a tough, reform-minded bishop was what made the difference. Like archbishop Lanfranc a hundred years earlier (p. 171), Gilbert started rebuilding because he chose to, not because he had to. Unlike Lanfranc, he failed to finish the job.³⁸

³⁵ If the analogy at work here is Cluny, it is surely too far-fetched.

³⁶ McAleer acknowledges this. The suggestion is, as he puts it, 'that the fire provided an "excuse" for the rebuilding of the east end' (McAleer 1999, p. 142). But one cannot say that and then stop. If the fire was only an excuse (or, to put it differently, an opportunity which might or might not have been taken), what was the real reason? The Canterbury fire of 1174 is a dubious analogy. There the opportunity was so opportune that it has even been suggested (absurdly) that the fire was started on purpose.

³⁷ One relevant source not cited in my book is a memorandum written by a Christ Church monk recounting the circumstances of bishop Walter's funeral and the events which followed ('Domesday Monachorum', fos. 7v–8r). This author is too busy cultivating his grievances to tell us anything about the architecture (he does not even tell us where exactly the bishop was buried), but he says nothing to suggest that the church was not in a fully functional condition at the time, i.e. in July 1182. Close to the grave there was a statue of Saint Nicholas, and the saint (because he was a bishop) had a pastoral staff in his hand. At one point in the proceedings, the archbishop was on the point of taking the saint's staff and burying it with bishop Walter (which implies that the statue was life-sized or nearly so). Either this statue had not been destroyed by the fire three years previously; or else it had already been replaced.

³⁸ As to those entries which relate to the re-roofing of the church (pp. 162–3), I can still

It is in the intervening period – say from about 1080 till about 1180 – that large disagreements prevail. Here opinions seem to be diverging, not coming together. In a way, this is not surprising: the evidence is sparse, and some of it is ambiguous. During this hundred-year interval, the structural history of the church can only in part be read from the fabric that is actually visible; in part it has to be read from a series of late nineteenth-century descriptions which, for one reason or another, do not inspire perfect confidence. Many crucial facts were recorded then – by Irvine (pp. 124–31), by Hope (pp. 131–3), by Livett (pp. 135–9) – which were only briefly revealed.³⁹ For us, the difficulty lies in deciding how far to rely on these descriptions – how much to believe, how much to take the liberty of doubting. It seems to me that these questions can mostly be answered, provided that we make the effort to read these descriptions as historical documents. Of course we all look forward to the time when further excavations will be carried out, in the right places and on a large enough scale to settle some of these issues;⁴⁰ but there is no excuse for sitting on our hands meanwhile. I have committed myself to some fairly precise predictions as to what will be found by a future excavator beneath the floor of the south choir aisle (fig. 28); in the same spirit, McAleer has ventured to predict what will be found beneath the floor of the crypt (McAleer 1999, fig. 9); and that is all to the good. Whoever is fortunate enough to be in charge of those excavations will need to know what questions there are to be answered, what possibilities there are to be considered. For the moment, our responsibility is the same as in any line of historical investigation: to make the best sense we can of the evidence available to us. There is no disgrace in getting things wrong, as long as one has made the effort.

To deal first with one incidental point, the stone used for the free-standing shafts in the crypt (pp. 126–7), and for the capitals and bases, used to be generally identified (with more or less assurance) as coming from the quarries at Barnack in Northamptonshire (a suggestion which seems to have originated with Irvine).⁴¹ It seems now to be settled that Barnack is out of the question, and that the likeliest source is the one first proposed by Tatton-Brown, the quarries at Marquise (near Boulogne).

see no way of making sense of them except on the basis indicated there – that the monks were repairing the old church while the bishop was building a new one.

³⁹ With regard to Robert Willis (p. 124), it should not have been overlooked that a full report of his lecture appeared in the *Gentleman's Magazine* for October 1863. (But neither Irvine nor Hope gives any indication of having read that report.) Willis is reported as saying firmly that of Gundulf's church there was almost nothing to be seen – nothing except the early portion of the crypt, and 'possibly' the north tower.

⁴⁰ 'We' here means archaeologists: the dean and chapter and the church's congregation may take a different view.

⁴¹ That some Reigate stone was used in the aisles of the crypt was first pointed out by Livett (p. 127).

This identification is accepted by Worssam (2001, p. 1); but it appears, from what he says elsewhere, that in his opinion a stone of this general type cannot be identified visually beyond a doubt. To identify the source with ‘complete certainty’, one needs ‘documentary evidence’ (Worssam 2001, p. 19). At Saint Augustine’s, it is a recorded fact that prefabricated components – bases, columns, capitals – were being imported from Marquise in the time of abbot Scotland. That information comes, of course, from Gocelin, who ought to be every archaeologist’s favourite hagiographer (p. 58).⁴² There is nothing to match it from Rochester.

As to the dating of the early crypt, I stand by what I said before (p. 127). On stylistic grounds, McAleer (1999, p. 37) feels confident that this crypt was part of the church built in about 1080.⁴³ On stratigraphic and other grounds, I think I have proved that it was not. The evidence of Irvine’s section (fig. 11) is conclusive, if it is to be trusted; and we do have some reason for trusting it, because it is borne out by the change of axis first detected by Hope (p. 132).⁴⁴ McAleer seems not to have grasped the significance of Irvine’s section (which he does not reproduce);⁴⁵ as to the change of axis, he seems to have been persuaded that this means nothing – nothing except that the nave was inaccurately laid out (McAleer 1999, p. 193 n. 51). There is some truth in that suggestion, but not nearly enough to explain all the facts – including the contraction of the first two bays (p. 158) – which need to be explained.⁴⁶

Separate from the question of its date, there is also disagreement between McAleer and me with regard to the shape of the missing portion of the crypt – the portion demolished in about 1190, when the new crypt was

⁴² Gocelin, *Miracula sancti Augustini*, ed. van Papenbroeck 1688, chapters 10, 21. From chapter 8 we learn that stone was being imported from Caen at around the same time – for the king’s palace at Westminster as well as for Saint Augustine’s.

⁴³ The specific comparison he makes is with the sub-vault of the dormitory at Christ Church. Given that the dormitory dates from about 1080, he thinks it ‘stylistically impossible’ for the Rochester crypt to be later than that by forty years or more. (I would be satisfied with thirty years.) But one cannot be sure that one is comparing like with like unless one has some idea as to the function intended for this crypt; and I find only one passage in McAleer’s book (1999, 45) which addresses this question at all. As far I can see, the crypt had no primary function except to support the floor of the presbytery above (though no doubt it was put to some secondary use, such as storing lumber). Like the space under the stage of a theatre, neither the actors nor the audience were expected to see it.

⁴⁴ Among Irvine’s notes, there are some rough sketches which look very different from the finished drawing. Here and elsewhere, we have to exercise some charity: perhaps it was imprudent of Irvine to let these sketches survive, but we should not think of using them as evidence against him. Of course it took him some time to work out the spatial relationships between all the features represented in his section.

⁴⁵ One crucial feature, moreover, is mentioned only in an endnote (McAleer 1999, p. 198 n. 86).

⁴⁶ That the Gothic bays are shorter is a fact mentioned by McAleer (1999, p. 106) – but only to account for the odd spacing of the windows in the fifteenth-century clerestory.

built. The question here is simple: did Hope find what he says he found? Like McAleer, I am perturbed by the vagueness of the published report; Hope's plan is so idealized that it risks losing credibility. So what are we to make of it? Undeniably, Hope was a bad excavator; but the badness lay in the fact that he was interested only in masonry – foundations, walls, floors and other solid structures. Dirt for him was just dirt, something to be removed as quickly as possible. Of bad excavation, however, he was not without experience.⁴⁷ If Hope tells us for a fact that two walls are of one build, it seems to me that we can and must believe what he says (until there is proof to the contrary).

To come back to his excavations in the Rochester crypt (p. 131), it is clear enough that Hope was expecting to find an apsidal termination. (In fact, he was making a similar prediction to the one made by McAleer.) Before he started digging, he thought it would turn out that the wall reported previously by Ashpitel (p. 121), if it existed at all, was a sleeper-wall subtending an apse which Ashpitel had overlooked. Starting with that idea, he would not have dropped it until he was sure that it was wrong; and all he had to do, to falsify his own prediction, was to expose the outer face of these foundations at one or more crucial points. Instead of an apse, he discovered something else, the existence of which is itself incompatible with the existence of an apse – a small rectangular compartment projecting from the centre of the façade. If Hope tells us, as he does, that the foundations for this compartment were integral with the foundations for the façade – he had made sure of this, he stresses, by 'personal excavation' – do we believe it or not? One of these days we shall know (or somebody will) whether Hope understood the facts correctly.⁴⁸ Meanwhile it is hard to see how we can think of doubting his word, just to suit ourselves.

Because McAleer attaches no importance to the very facts which seem most significant to me – the stratigraphy recorded by Irvine, the change of axis detected by Hope – it is no surprise that his reconstruction of the late eleventh-century church differs greatly from mine. But in one respect they agree. Both reconstructions assume that the church had full-sized transepts and a full-sized central tower. McAleer, like me, is unconvinced by the structural evidence reported by Irvine (pp. 125–6) which seemed to him (and to Hope and Livett) to prove otherwise. As for the transepts, it should be recalled that there is some written evidence (unknown to Hope) which seems to prove that they resembled the transepts at Christ Church

⁴⁷ He had already carried out some extensive excavations at Saint Radegund's, for example (Hope 1882).

⁴⁸ Some limited excavations in the crypt were carried out in 1994–5, when new drainage was being installed; but the trenches were all too far to the east to settle this question for us. (This information comes from Alan Ward's interim report.)

in being galleried (p. 148).⁴⁹ As for the central tower, the written evidence is, at a pinch, open to more than one interpretation. It all depends what tower is meant by those thirteenth-century sources which speak of the 'greater tower', *maior turris*. McAleer and I both take the view (the obvious view, one might say) that this *maior turris* is the central tower. It is stated, for instance, that prior Reinald 'made two bells and put them in the greater tower';⁵⁰ and this can be taken to mean that the central tower existed and had bells hung in it no later than 1180 (on the assumption that Reinald made these bells before he ceased to be prior), and possibly no later than about 1150 (on the suspicion that Reinald made them while he was sacrist, before becoming prior).⁵¹ Correspondingly, the earliest written evidence which can be taken to refer to the north tower is a casual mention of the 'little tower', *parva turris*, in a list of anniversaries which dates from about 1230 (p. 84). On the other hand, Tatton-Brown seems still to think that the *maior turris* is the north tower. If that is indeed what he thinks, he will have some explaining to do.

The north tower has become the subject of a debate between McAleer and Tatton-Brown into which I have no wish to intervene. In my view, it is not clear that we can be sure of anything yet except that the north tower existed before about 1190 (because of the asymmetry it caused in the design of the north-east transept) and had bells hung in it by about 1230.⁵² But the suggestion by McAleer (1998, pp. 158–9; 1999, pp. 24–5) that the north tower may have been built as early as about 1070 seems to me, for a number of reasons, as near to being incredible as makes no difference. In my understanding, the eleventh-century church was laid out parallel with the Roman wall, at a distance from it dictated by the intended length of the eastern range of the cloister (fig. 21), and the tower, later, was built alongside the church. In McAleer's view, the tower was there first and dictated the siting of the church. Sooner or later, no doubt, exploration in the space between the church and the tower will prove which structure is the earlier; it will surprise me greatly if McAleer's view does not turn out to be wrong.⁵³

Before quitting the subject, I need to make one brief excursion into the

⁴⁹ McAleer (1999, p. 77, for example) cites this evidence, but apparently does not interpret the word *testudo* in the same way that I do.

⁵⁰ The entry comes from the list of benefactors compiled in the 1220s (pp. 73–6). It seems to need saying strongly that this is not contemporary evidence.

⁵¹ A monk named Reinald, presumably the same, occurs as sacrist in 1143 (see above); Reinald occurs as prior in 1155. The next known prior was in office before 1180.

⁵² There is one item of written evidence (never previously cited, as far as I am aware) which seems to prove that the belfry on top of the north tower was being rebuilt in the 1250s (*Calendar of Close Rolls 1251–3*, 364). The same date may perhaps apply to the massive buttresses added at the north-east corner.

⁵³ A small trench was cut through this space in 1999. As far as I understand it at the time of writing (July 2002), the stratigraphic evidence was not quite conclusive.

fourteenth century. On Hope's interpretation, the central tower was not built until this time, its completion being due to bishop Hamo de Hethe (1319–52). Under the year 1344 (not 1343, an error which began with Wharton), it is recorded that the bishop 'caused the new bell-tower (*campanile nouum*) . . . to be raised higher with stonework and woodwork, and caused it to be covered with lead' (fo. 90v). That statement comes from a historical text (the only surviving copy of which is BL Cotton Faustina B v) describing the events of bishop Hamo's time in office. I had thought that I was the first person to suggest (p. 6) that this text might be Hamo's autobiography, but in fact, as he has just recently discovered, the credit for that suggestion belongs to Wigan (1983).⁵⁴ Wigan was also the first to prove, in the same paper, that Hamo did not die in office (as had always been supposed), but that he resigned and remained alive for several years.⁵⁵

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⁵⁴ This is the Rev. Bernard John Wigan (1918–1994); an obituary (by D. A. H. Cleggett) appears in *Archæologia Cantiana*, cxiv (1995), 484–5. I am grateful to Mrs Mary Griffin for letting me have a copy of Wigan's paper.

⁵⁵ ((The original paper included one more section – a discussion of the west portal and the sculpture inserted into it. An expanded version of that section was published in *Friends of Rochester Cathedral – Report 2002/2003*, pp. 9–14; so I do not reproduce it here.))

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