

THE CONTINUING STORY OF THE DATE AND ORIGIN OF THE CODEX FLORENTINUS*

The most famous manuscript in legal history is no doubt the Florentine manuscript of the Digest. The only manuscript in the Biblioteca Medicea Laurenziana without a library number, common ancestor of all Medieval Digest manuscripts, tourist attraction for kings and emperors in the late Middle Ages and after,¹ and still a source of inspiration for legal historians.

However famous the manuscript may be, two most fundamental questions about it, concerning its date and origin, have never been answered satisfactorily. It cannot be the purpose of this article to do so: the questions have been more or less hotly debated for several centuries by eminent scholars like Agustín, Brenkman, Mommsen and Kantorowicz, to name just a few. The discussion is unlikely ever to stop, because all the arguments are only arguments of probability. Many facts related to the manuscript can be interpreted in several ways, to fit different theories. I can only hope to give my view of some of the arguments put forward by others, and perhaps add a few new arguments of my own.

Although of course the Florentine (which I shall indicate by its well-known siglum, F) was known to legal scholars long before that time, we may consider the discussion about its date and origin to have started with early legal humanism, at the beginning of the 16th century. As far as the age of the manuscript is concerned, Poliziano and Alciatus are the two extremes quoted by Brenkman, Poliziano thinking F *Justiniani aetate descriptum esse*, even going as far as stating that it was THE original manuscript of the Digest.² Alciatus, pointing out the many mistakes in F, thought it more likely that it had been written *multis post Justinianum saeculis*.³ As to the origin, most Humanists agreed that F was written by a Greek, which made it likely that it was written in Constantinople. I refer the reader to the second (*De aetate*) and third (*Ubi scriptus*) chapters of Brenkman's *Historia Pandectarum* for a summary of the discussion up to Brenkman's day.

In Mommsen's preface to his *editio maior* of the Digest, we find the same kind of arguments as Brenkman gave.⁴ For the origin, the most important argument for Mommsen still is that both scribes and correctors were Greeks, although he is careful to point out that this does not necessarily imply that F was written in

* I thank Prof. N. van der Wal, Dr. B.H. Stolte, Dr. D. Osler and Mr. J.H. Dondorp for their remarks which have been very helpful in writing this article.

1 S. Caprioli, 'Visite alla Pisana', *Le Pandette di Giustiniano. Storia e fortuna di un codice illustre*. Due giornate di studio, Firenze 23-24 giugno 1983. Florence 1986, 37-98

2 Poliziano, *Miscellanea*, cap. 41, quoted by Brenkman, *Historia Pandectarum seu fatum exemplaris Florentini*, Utrecht 1722, 5

3 Alciatus, *Disputationes* III, 12 ad fin. (quoted by Brenkman, *ibidem*).

4 Mommsen, *Praefatio*, xxxix-xxxx.

Constantinople. As far as the date is concerned, Mommsen considers it most likely that the manuscript was written some time after Justinian, because of the lacunas it contains. He makes it very clear that he does not think that the manuscript can be dated on palaeographical grounds.

Kantorowicz has tried hard to prove that the scribes who wrote F were of Latin nationality.⁵ Whatever he may say about that, he still has to concede that the correctors were Greeks. From the Latin origin he attributes to the scribes, he has no hesitation in declaring that F was written in Italy.⁶ About F's date, Kantorowicz mentions 542 as a *terminus post quem*, because the correctors used the Indices of Dorotheus and Stephanus, which had not been compiled before that year.⁷

The question of the date and origin of the Florentine was approached from an entirely different angle in 1961, when E.A. Lowe published an article about a number of Latin legal manuscripts.⁸ In this article, F is placed among a number of (mainly) legal manuscripts which have certain characteristics in common. All these Latin manuscripts appear to come from one scriptorium, which employs typically Greek practices. Lowe argues that Constantinople is the most likely place for such a scriptorium. Other possible candidates are Beirut, Alexandria and Carthage, but these are eliminated by Lowe.⁹ His conclusions concerning F's date and place of origin are: it was written reasonably early in the sixth century, in Constantinople. The date is based on palaeographical arguments; the conclusion concerning the

5 H.U. Kantorowicz, *Ueber die Entstehung der Digestenvulgata. Ergänzungen zu Mommsen*, Weimar 1910 (reprint with addenda of two articles that appeared in the *SZ* in 1909 and 1910), 4-10.

6 *Ibid.* 11. I think that Mommsen's argument concerning the scribes' nationality can be turned around here: a Latin origin does not necessarily mean that the scribe was working in his home country. In fact, is it not likely that there were scribes of Latin origin in Constantinople, making good money as experts in writing Latin?

7 *Ibid.* 13. I doubt the strength of this argument, because I fail to see why the similarity between a correction in F and a passage from one of the Indices can not be explained by assuming a common source. However, I did find a *terminus post quem* for the correction of book 24 of the Florentine in a recent article about the date of Dorotheus' Digest translation: F. Brandsma, 'An inappropriate use of Novels? On the date of the Digest translation of Dorotheus', *Novella Constitutio. Studies in Honour of Nicolaas van der Wal*. *SG IV* (1990) 51-55: Nov. 22 c. 8 (536) abolished slavery as an automatic punishment for a criminal sentenced to work in the mines. In order to bring the text of the Digest up to date, F's corrector expunged the clause *vel ancilla effecta* in D. 24,3,56; *ergo*: the corrector could not have worked earlier than 536.

8 E.A. Lowe, 'Greek symptoms in a Sixth-Century Manuscript of St. Augustine and in a Group of Latin Legal Manuscripts', *Didascalie. Studies in Honor of Anselm M. Albareda*, edited by Sesto Prete, New York 1961; reprinted in: E.A. Lowe, *Palaeographical papers 1907-1965*, Oxford 1972, 466-474.

9 *Ibid.* 472-473. Carthage had become too unimportant in the sixth century; Beirut was destroyed by an earthquake [which Lowe dates in 529, following Benzinger (RE III.1 col. 322). This date is certainly wrong: for Justinian, Beirut was still an important centre for legal studies in 533 (constitutio Omnem §7; 9; 10; Tanta/Δέδωκεν pr.), and the date is elsewhere put at 557 (Stein, *Histoire du Bas-Empire* II, Amsterdam 1968, 758 note 1)]. Alexandria, according to Lowe, is unlikely to have produced books for Italy; to this, we may add that Justinian explicitly prohibited his laws to be taught at Alexandria (constitutio Omnem §7).

place where it was written is based on probability, not on definite evidence.

In 1985, a joint article by Cavallo and Magistrale¹⁰ reassessed our knowledge of F's history in the light of Lowe's article. The authors point out the fundamental importance of Lowe's approach, which no longer treats F as an isolated case, but tries to establish links with other manuscripts. They proceed to examine Lowe's statements about the time of the development of the so-called B-R uncial, and conclude that this should not be dated as early as Lowe does, but rather to the middle of the sixth century. They also point out that it is very difficult to date legal uncial manuscripts solely on palaeographical grounds. Still, they consider the B-R uncial to be a phenomenon to be associated with, or even produced by Justinian's codification. Examining Lowe's hypothesis that Constantinople is the most logical place for a scriptorium that could have produced manuscripts with characteristics of the kind found in the group to which F belongs, they point out that this hypothesis is not unlikely, but that it is also possible that such characteristics would have spread to other scriptoria. They suppose the legal book-production in Justinian's time to have been spread over several centres in the empire. In the end, they prefer to believe in an Italian origin for F, suggesting Naples and Ravenna as possible birth-places.

In 1984, Stolte published an article containing a very significant contribution to the discussion about F's date and origin.¹¹ Anyone who looks up Mommsen's preface to the *editio maior* of the Digest can see that thirteen scribes worked on the manuscript.¹² Stolte discovered that the way the work was distributed among the scribes is somehow related to the curriculum in Justinian's law-schools as described in the constitution *Omnem*. The Digest was divided into seven *partes* (some of these were further sub-divided into smaller units), and the *partes* formed the basis for spreading the teaching of the Digest over the first four years of legal education.

It turns out that the division of the work on F is related to this division into *partes*: in all cases but one, a new *pars* is started by a new scribe. The same holds good for the smaller units within certain *partes*. Since we know that Justinian's system for the legal education was abandoned soon after 557,¹³ it would seem that the manuscript must have been written around that time at the latest. The link with legal education also means that Constantinople once again becomes the most likely candidate for having produced the manuscript, since legal education was only allowed to take place in Constantinople, Rome and Beirut. It is unlikely that a law course along Justinianic lines was ever set up in Rome, and Constantinople is more likely than Beirut.

10 G. Cavallo - F. Magistrale, 'Libri e scritture del diritto nell'età di Giustiniano', G.G. Archi (ed.), *Il mondo del diritto nell'epoca Giustiniana. Caratteri e problematiche*, Ravenna 1985, 43-58

11 B.H. Stolte jr., 'The partes of the Digest in the codex Florentinus', *SG I* (1984) 69-90

12 Mommsen, *Praefatio*, xxviii-xxx

13 H.J. Scheltema, *L'enseignement du droit des antécédents*, Leiden 1970, 9

In a reaction to Stolte's article, Osler suggested that Stolte's findings might imply that the copy text which F was copied from, rather than the manuscript itself, was related to Justinian's law course.¹⁴ This indeed is all we may conclude from the fact that the places where a new scribe started to work coincide with the *partes*-division: that may be nothing more than the copy text leaving its mark on the copy. But if we take a closer look at the overall division of the work on this particular manuscript, paying attention to the amount of work done by one scribe at a stretch, I think there may be more to be discovered. We must, of course, be very careful not to lapse into historic fiction; it is no use wanting to find out too precisely how the work on the Digest went. We shall never know. Still, there seems to be more information in the division of the work than has been extracted so far.

I have compared the amount of work done on F by the respective scribes, using Mommsen's table in his preface to the *editio maior* and the pages of the *editio stereotypa* of the Digest¹⁵ as the basis for my estimate of each scribe's contribution. It is interesting to see that scribe I wrote all of the first *pars*, and a few odd scraps elsewhere in the manuscript, 138 *stereotypa* pages in all. This is a unique situation, no other *pars* having been written by one single scribe.

The other *partes* have been sub-divided into pieces of varying length, written by different scribes. Stolte already noted that this sub-division is most easily explained for the *libri singulares* of *partes* 4 and 5. Within these *partes*, there were smaller units of mostly two or three books, bearing their own names, and these units are usually written by one scribe each. But the sub-division in *partes* 2 and 3 is not so easily explained; these two *partes* were both taught in their entirety. And the sub-division in *partes* 6 and 7 can certainly not be explained from Justinian's law course, because they were not taught to students at all.

Now if we look at the amount of work done by one scribe at a stretch, there seems to be a kind of pattern in most of the *partes*. The fourth *pars* was written by three scribes (VI, V and IIIb), the work being evenly divided: 35, 40 and 40 *stereotypa* pages, respectively. A similar situation is found in the fifth *pars*: divided into four pieces of 46, 43, 41 and 47 pages, with the last piece again divided,¹⁶ the first 23

14 D. Osler, 'Flashlight on the Florentine', *RJ* 3 (1984), 18

15 This is perhaps not very logical, since I should of course have taken the Florentine's own pages. But the new photographic reproduction of F is not on my bookshelf, whereas the *editio stereotypa* is; so I used the latter just as a matter of convenience. Most of the readers of this article may find themselves in a similar situation.

The result of my counting is (for the main body of the text, in the order of Mommsen's table on pp. xxviii-xxx of his *Praefatio*): scribe I - 74 *stereotypa* pages; II-13; IIIa-4; II-19; I-3; IIIa-49; IV-41; IIIb-7; V-39; IIIb-18; VI-35; V-40; IIIb-40; V-46; VII-43; IIIa-41; VIII-23; I-24; IX-49; X-23; II-74; I-4; II-19; VIII-34; IIIb-26; IX-113.

16 That these two smaller pieces belong together is proven by the fact that on this occasion only, a new scribe starting a new book does not take a new quire, but starts on the verso-side of the leaf where his predecessor had finished the previous book on the recto-side. On the other 19 occasions where a new scribe starts a new book, he also starts with a new quire.

pages written by one scribe, the last 24 by another; scribes V, VII, IIIa, VIII and I worked on this *pars*, in that order. These *partes* show by far the most regular division of work.

Partes two and three seem to have been divided into roughly equal halves, but in the first half of the second *pars*, we see scribes II, IIIa, II again, then I, and finally IIIa takes over near the end of a quire (not at the beginning of a new one, and in mid-sentence!) to write the second half of the *pars*. In the third *pars*, scribes IV (41 *Stereotypa* pages) and V (39) start to write most of the respective halves, but these are then finished (the last 7 and 18 pages) by scribe IIIb.

The sixth and seventh *pars* are different from the other five at least in one respect: the seventh *pars* is not separated from the sixth by a change of scribe, because scribe VIII writes 34 pages towards the end of *pars* 6 which contain books 44 and 45, the latter being the first book of the seventh *pars*. Possibly, *partes* 6 and 7 are to be taken as a whole rather than as two separate *partes*. Apart from the logical unit (written by scribe IX) of books 37-38, already noted by Stolte,¹⁷ division of the work in these *partes* is very irregular.

What can we infer from all this? First, I think, a clear picture emerges of a scriptorium at work. There are a number of scribes who write one part of the manuscript here, another part there. Take scribe I: he writes all of the first *pars*, and we see him at work again in the second, fifth and sixth *partes*. The same holds good for most of the other ones: they are responsible for contributions in different parts of the manuscript. It even appears that on occasions one scribe took over from the other more or less in mid-sentence (I will not elaborate this point: everyone who looks up Mommsen's table can see for himself).

Secondly, it is clear that the division of the copy text (the exemplar) cannot have been by units as large as the *partes*; there must have been smaller units, mostly consisting of two or three successive Digest books, and (at least in *partes* 2-5) corresponding to some 40-50 *stereotypa* pages. This is a finding which only modifies Stolte's theory, but still may have some bearing on the textual history of the Digest.

It is interesting to observe that the way the work was divided among the scribes certainly does not point to any particular hurry in the copying process. Although we see quite a few cases of division into relatively small parts, several of these smaller parts were written by the same scribe, so they could not be written simultaneously. Also, there are some very long parts copied continuously by one scribe: the first *pars* (74 pages by scribe I), a long contribution (74 pages again) by scribe II in books 40 ff., and the last part of the seventh *pars* (113 pages by scribe IX).

I think we can draw some conclusions from this. In the first place, if the division of the work was not the most efficient one possible, this is an argument against F having been one of the first generations of Digest manuscripts produced

17 Stolte, *Partes*, 76-77

immediately after the publication of the Digest in 533. At that point in time, there must have been an urgent need to produce as many copies as quickly as possible, and if we look at the way the work on this manuscript was divided, I think it is clear that F does not fit into that context.

In the second place, the division of the work may tell us something about the origin of F. Two of the longest stretches of copying occur in the sixth and seventh *pars*, and this is, I think, compatible with F having been copied in the context of Justinian's law course. The work on the *partes* 2-5 was divided in such a way as to make quick and efficient copying possible. Apparently, no need was felt to achieve the same for *partes* 6 and 7. Note that in these *partes*, scribe IX is responsible for $49 + 113 = 162$ of their 341 pages, almost half the total. I am inclined to think that the scriptorium where F was written had established a practice of copying designed to meet the demands of students. *Partes* 2-5 were sub-divided so that copies could be produced quickly, and students were not forced to buy all of *pars* 4 or 5 at once in their second year. The last two *partes*, which did not form part of the law course, were treated by the scriptorium in a different way. This way of working seems to connect F, not only its copy text, to the law course.

Is it surprising that *pars* number one was copied at a stretch by one scribe? I do not think so: all the students had to have *pars* one, since they had to study it in their first year at the university. There must have been a more regular and reliable demand for it than for any other *pars*; not everyone who started to study law would get beyond the first year.

All in all, we seem to be looking at a scriptorium working, in the first place, for a university. If that is true, Constantinople is the obvious candidate for the origin of F.

There is one other very interesting point. So far, I have been considering only the main body of the Digest text, that is, without all the prefatory material. Now this main body of the text is more or less knitted together by the fact that several of the eleven scribes who worked on it wrote pieces in the beginning and in the middle, or in the middle and near the end. It is an interlocking whole. But what about the prefatory material? As it turns out, only part of it can be positively related to the main body of the text by the identity of its scribes. Three scribes (IIIb, XI and XII) have worked on it, and only one of them (IIIb), who wrote the constitution Δέδωκεν and the *Index auctorum*, has contributed to the main body of the text as well. Scribes XI and XII are cases in themselves for more than this reason. Their contributions are also considerably smaller than any of the other scribes': they wrote 5.5 and 6.5 *stereotypa* pages,¹⁸ respectively, whereas the next smallest contribution (by scribe VI)

18 Since the layout is so different in this part of the *editio stereotypa*, I calculated the length of these contributions by measuring the length of the text-columns in centimetres, and divided this by length of the text on the average *stereotypa* pages, which I calculated to be 36 cm (two 18 cm columns).

is equivalent to 35 pages. And there is another reason why at least the contribution by scribe XII, who wrote the Latin introductory constitutions, is a special one: his work is the only part of the entire manuscript that is not written in two columns to the page, but in lines running over the full width of the page.

So what do we have? It would seem that not all of the prefatory material can be linked to the rest of the manuscript in the same way. One may object that just the identity of the scribes is not a great deal to go on, and that is, of course, a completely correct observation. But there is also the layout of the Latin introductory constitutions, and if we suppose F to have consisted originally of constitution Δέδωκεν, the *Index auctorum*, and the main body of the text, there is a remark in the introductory constitutions which corresponds to that situation. Justinian says, in Tanta/Δέδωκεν §20, that he ordered a list of the sources from which the Digest was composed to be added to it: the *Index auctorum* (written by scribe IIIb in F). There is no reference in the introductory constitutions to the *Index titulorum* (written by scribe XI).

My suggestion is that the main part of F, consisting of the constitution Δέδωκεν, the *Index auctorum* and the main body of the text, was written by a group of eleven scribes belonging to one scriptorium. There is, of course, no evidence that the last two scribes, nrs. XI and XII, did not belong to the same scriptorium, or that the Latin prefatory material should not be considered part of an official Digest copy. Still, these two scribes and this material cannot be linked with the rest of the manuscript as tightly as the *Index auctorum* and the constitution Δέδωκεν. The part of the manuscript that is held together by the identity of the scribes would seem to have been produced for a Greek context. Once again, a scriptorium in Constantinople seems to fit this situation best: a scriptorium where Digest manuscripts as a rule were produced without the Latin prefatory material.

There is one question which the authors on this subject appear to have left unanswered. If we suppose that F was written in Constantinople (which is the most likely possibility) how did it make its way to Italy? It was present in Italy no later than the 12th century, and possibly as early as the 9th-10th.¹⁹ The question seems to be impossible to answer, because neither does the manuscript itself yield any information concerning its whereabouts in the first couple of centuries following its coming into existence, neither do we have any other sources about this period which tell us anything.

Perhaps, keeping in mind the distinction between the text itself (the Digest) and the bearer of the text (in this case, F) we can construct an argument of probability by rephrasing the question as follows: how did the Digest make its way to Italy? This question is related to the more general question: how did Justinian approach the distribution of his codification over his empire? As far as F is concerned, we cannot

19 Stolte, *Partes*, 81-82 and note 26.

find more than another probability, because what happened to the Digest is not necessarily the same as what happened to an individual Digest manuscript. Still, having a clear picture of the circumstances of the distribution of the Digest may help us in assessing the possible fate of an individual manuscript.

What do we know about the way Justinian decided to spread his new law-books over his empire? In 533, he was faced with two problems: he had to get copies of the Digest to all parts of his empire as quickly as possible, but he also had to make sure that the copying was done sufficiently accurately to make the texts reliable. Did he find a way to steer clear of the Scylla and Charybdis of slowness of copying and mistakes through excessive speed? I have set out my conclusions about this elsewhere;²⁰ I will give only a brief summary in this context.

Unlike Theodosius II a century before him, Justinian did not over-concentrate on the aspect of protecting the correctness of the text. Theodosius had given permission to copy his Code only to two specially-created government officials, the *constitutionarii*. This must have ensured a reasonably correct text, but it was just about the slowest way possible to achieve it. Justinian, in principle, allowed copying by anybody. That is why his introductory constitutions had to contain provisions about the way of copying. We find these provisions mainly in *Tanta/Δέδωκεν* §21-22: the prohibition of sigla and the prohibition of commentaries.²¹

To ensure that the books were quickly spread over the empire, it is likely that copies were sent to Athens and Beirut, to serve as examples for further copying there. This can be inferred from the combination of a passage from John Malalas' *Chronographia* (XVIII, 18) with one from the constitution *Summa* (§5). The last passage, admittedly, only refers to the distribution of the first edition of Justinian's Code, but the system looks a very logical one, and I see no reason why it should not have been used for the distribution of the Digest, Institutes and the second edition of the Code as well, with a possible fourth centre of distribution being added in Africa.²²

Were any Digest copies sent to Italy under this system? The fact that copies were sent at some stage is confirmed by the *Sanctio pragmatica Pro petitione Vigili*, which was issued in 554:

Iura insuper vel leges codicibus nostris insertas, quas iam sub edictali programme in Italiam dudum misimus, obtinere sancimus. Sed et eas, quas postea promulgavimus constitutiones, iubemus

20 T. Wallinga, *Tanta/Δέδωκεν. Two introductory constitutions to Justinian's Digest*, Groningen 1989 [thesis Utrecht 1989], 93-96

21 There is nothing in this context to support the idea of Cavallo and Magistrale (cf. note 11), that a uniform copying practice may have been imposed upon all scriptoria in the Empire.

22 The government officials responsible for the distribution and copying were the three praetorian prefects, of the East, of Illyricum and of Africa, together with the city prefect of Constantinople (*Tanta/Δέδωκεν* § 23). This seems to suggest that the system was modified for the Digest, in the sense of a fourth centre of distribution in Africa (Carthage?) being added to the other three.

sub edictali propositione vulgari, <et> ex eo tempore, quo sub edictali programme vulgatae fuerint, etiam per partes Italiae obtinere, ut una deo volente facta republica legum etiam nostrarum ubique prolatetur auctoritas. (Nov. Edd. Schoell/Kroll p. 800)

Although this source proves that Justinian did introduce his law-books in Italy, it does not state exactly when he did it. But it says that Justinian had already, some time ago,²³ sent his laws to Italy; we are left to determine when that happened.

We may be quite sure that the Digest was not introduced in Italy in 533. Not only did Justinian have no basis of power in Italy, there is also no trace in the introductory constitutions of such a thing having happened. There is a certain development: the first edition of the Code was probably sent to Athens and Beirut for further distribution, while copying was also being done in Constantinople itself. The Digest was possibly distributed from a centre in Africa as well: *Tanta/Δέδωκεν* § 23-24 orders the city prefect of Constantinople and three praetorian prefects (of Africa, Illyricum and the East: note the geographical spreading) to ensure the distribution of the new law-books. But nothing about Italy. Nothing about Italy either in the constitution *Cordi*, which accompanied the second edition of the Code in 534. It must be noted that Justinian seems to have sent a constitution to Italy in that very year: CJ 6,51 is addressed to the senate of Rome as well as to the senate of Constantinople, and we would have expected the same to apply to the constitution *Cordi*. But in fact, there is nothing about Rome in the latter constitution, and we are left to wonder whether the final words of the inscriptio of CJ 6,51 *et urbis Romae* are authentic; they have only come down to us in the *Summa Perusina*, not in the other manuscripts.²⁴

Then what may have been the occasion for introducing the law-books in Italy? A small excursus on Justinian's conquests in the West of the Mediterranean is in order. The throne of the Gothic kingdom had been left, in 526, to Athalaric, who was ten years old at the time; his mother Amalasuntha governed in his name. The Goths objected both to being ruled by a woman, and to her pro-Roman policy, so Amalasuntha looked to Justinian for support and started negotiations with him about giving up the kingdom in exchange for asylum. But when Athalaric died in 534, Amalasuntha assumed the title of queen, and proclaimed her cousin Theodahad king. Completely against her expectations, he sided with the Gothic opposition, deposed her and had her imprisoned, and soon after murdered, on an island in the Lake of Bolsena. Justinian promptly responded by protesting against this murder, and by sending two military expeditions, one to Dalmatia and one to Sicily.²⁵

23 The key word in the text is *dudum*. This may, but does not necessarily, mean "a long time ago" (cf. N. van der Wal, *Les commentaires grecs du Code de Justinien*, The Hague 1953 [thesis Groningen 1953], 11-12). It does not force us to assume any definite number of years.

24 Stein, *Bas-Empire* II, 340-341 appears to take it for granted that CJ 6,51 was actually sent to Rome. However, considering the political situation and the lack of evidence for other constitutions having been sent there, this seems unlikely, so I am inclined to see *et urbis Romae* as a later addition.

I shall not repeat the entire story of Justinian's reconquest of Italy. It will suffice to say that there were two different stages. From the beginning in 534, the war against the Goths brought a series of successes and defeats for Justinian, but he had definitely gained the upper hand in 539. Then trouble started on the Eastern frontier, where the Persian king Chosroes had had enough of the Eternal Peace, which forced him to sit and watch Justinian conquering the West. This new development in the East forced Justinian to bring the war in Italy to a quick conclusion. So in 540, the Ostrogoths surrendered to Justinian's commander Belisarius on reasonably favourable terms. Belisarius was recalled to Constantinople. Italy seemed to be conquered.

As one of the first administrative measures, John the Cappadocian's methods of taxation were introduced in Italy by the *discussor* Alexander, ominously nicknamed "Scissors". A *praefectus praetorio Italiae* had already been appointed by Justinian early in 537. There was no real successor to Belisarius as a military commander, and rigorous fiscal methods were introduced in a country which had suffered much from years of warfare. It is no surprise that the Ostrogoths soon resumed the war under their new king, Ildebad. Again, a depressing story of defeats and successes for the Byzantines followed: Chosroes' breaking of the Eternal Peace in the East prevented Justinian from concentrating his forces in the West. It was not until 552 that a strong army was sent to Italy under Narses. This army defeated the Goths; their king Totila was killed in the battle at Busta Gallorum. His successor Teias was defeated as well in the next year, and the Gothic army finally surrendered. The Franks saw their opportunity and became aggressive, but after they had been defeated at Capua in 554 there was peace in Italy at last.²⁶ In the same year, Justinian's *Sanctio pragmatica* was issued.²⁷

It has been suggested that the expression *sub edictali programme* in the *Sanctio pragmatica* refers to the year 535,²⁸ and that therefore the law-books had been sent to Italy as early as that. However, Archi has pointed out that we may not assume legislative activity in the direction of Italy at such an early stage: e.g. Justinian's Novel 69, which is from the year 538, and concerns all provincial iudices, does not mention Italy.²⁹ The conclusion must be that Justinian paid attention to administrative matters in Italy on two occasions: in 540 and in 554. Now when the *Sanctio pragmatica* states that in 554 the texts of Justinian's codification had been sent to Italy *dudum*, it is obvious that the texts must have been in Italy in 540. This,

25 A.H.M. Jones, *The Later Roman Empire*, Oxford 1973, 274 ff.

26 Jones, *Later Roman Empire*, 287 ff.

27 See G.G. Archi, 'Pragmatica sanctio pro petitione Vigilii', *Festschrift für Franz Wieacker zum 70. Geburtstag*, Göttingen 1978, 11-36

28 L. Zdekauer, 'Su l'origine del manoscritto Pisano delle Pandette Giustinianee e la sua fortune nel medio evo', *Studi Senesi* 6 (1889) 285-322 (295). Kantorowicz dismisses this article almost entirely: *Digestenvulgata*, 11 note 6.

29 Archi, *Pragmatica sanctio*, 20, and cf. note 25 above.

however, does not exclude the possibility of more texts having been taken across in 554 when Vigilius (who had left Rome in 545, and had been in Constantinople since 547³⁰) returned.

If that is so, then it would be a very straightforward hypothesis to assume that F was taken to Italy on one of these occasions. It fits in well with the fact that the manuscript was in Italy no later than at the end of the 9th century.³¹ This hypothesis has a considerable advantage over the one that considers F to have been copied in Italy. The latter hypothesis involves one more step in the process than the former. Copying in Italy entails supposing the existence of a scriptorium capable of producing a manuscript as good as F. This is unlikely, though not impossible. But there is a more serious objection. If F was copied in Italy, what can we say about the copy text that must have been taken to Italy from Constantinople? It must have been divided in such a way as to produce the division of the work among the scribes as it can be seen in F. Can we imagine such a copy text?

Yet another objection is: why should scribes in Italy have divided their work the way F's scribes did, with relatively short shares in the work in those *partes* that were taught during the law course, to enable efficient copying, but a different approach to the two *partes* which were not taught to students? This way of working makes no sense outside the context of Justinian's law course: if the scribes had worked in Italy, one would expect the work on F to have been shared more evenly among them. In Italy in 554, the urge to establish a law-school according to the principles laid down in the constitution *Omnem* cannot have been very great; the system was in decline already. No such school could have been established in the years when war between the Byzantines and Goths was still going on.

Was F the only manuscript that went to Italy? This is most unlikely. There is no sign of it having been used as a copy text, divided into (groups of) quires; it is more like an "official" copy, possibly intended for being on show rather than for actual use.³² Also, if F would have been the only copy, how are we to explain the variant readings in the Bolognese Digest tradition that cannot have come from F? There may have been more copies; a copy text consisting of separate quires may have been sent to Italy in addition to F. Another possibility is that (parts of) Digest manuscripts were acquired in Constantinople much later: does this perhaps explain the remark by Odofredus, that the Pisans had the Digest (or F?) directly from Constantinople?³³

The final conclusion must be that Constantinople, once again, turns out to be the most likely birth-place of the Florentine. A closer look at the way the scribes divided

30 Stein, *Bas-empire* II, 578 note 2; 640 ff.

31 Stolte, *Partes*, 82 note 25.

32 Stolte, *Partes*, 88

33 Spagnesi, *Le Pandette di Giustiniano*, 41 nr. 27

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their work appears to overcome Osler's objections to Stolte's article, and this means that F itself, not just its copy text, is related to Justinian's law course. We may assume the latter to have been in operation only at Constantinople and Beirut. Against the background of Justinian's reconquest of Italy, it seems likely that a manuscript like F came to Italy from Constantinople rather than from Beirut. And as to the date: the years 540 and 554 seem to be the most likely occasions. Given the *terminus post quem* of 536 (cf. note 7), we may fix the date of the manuscript within the 536-554 period. Of course, it is possible that the manuscript made its journey on a later occasion, but it is difficult to imagine another occasion as obvious as these two.

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POSTSCRIPT

After finishing my article, I was given the opportunity to look at Mr. Wilson's contribution to this volume of the *Subseciva*. He seems to have pulled the foundations away from under my hypothesis about F having been taken to Italy in 540 or 554: if F was in Constantinople in the 9th century, it probably never left the East in the previous period. The years 540 and 554 remain the most obvious occasions in the 6th century to take Digest manuscripts to Italy, and I do not doubt that some manuscripts were in fact sent there, but F does not seem to have been one of them. However, the rest of the article, about the date and origin of F remains unaffected.

T.W.

