Introduction

This book forms the first of two parts of an investigation into the textual problem of *King Lear*. The overall purpose of the study is to provide the foundation for a new editorial approach to the play, and the ultimate goal is a new edition. With the exception of this introduction, however, the present volume barely mentions the literary fabric of the play, but concentrates instead on the life and work of Nicholas Okes, the printer of the First Quarto. Other textual studies, particularly those claiming to be bibliographical, have devoted some attention to the printer or printers of whatever text was under investigation, but it has seldom been found necessary to pursue such matters as far as I have here pursued them. An explanation is therefore required.

The nature of the textual problem of *Lear*, and the history of the various attempts to solve it, will be examined more fully in the introduction to Volume II. In essence, however, the difficulty has been caused by the existence of two early editions which differ from each other in many ways. The first of these editions was a Quarto dated 1608, containing a text known as Q. Fifteen years later a second text, known as F, was printed among the Tragedies in the First Folio of 1623. Much of the play can be considered to be essentially the same in the two editions, but F omits scattered passages of varying length which total approximately 285 lines in Q. At the same time it adds a number of short passages totalling approximately 115 lines which are not found in Q, and makes hundreds of verbal changes of one kind or another. The overall effect of these alterations is considerable.

Could it be shown that one of the texts is uniformly authoritative and the other thoroughly corrupt, there would be few problems. It has long been held that the majority of the text which is effectively common to both editions is presented more authoritatively by F. But different investigators have found different numbers of Q readings to be superior to those substituted by F, and the lines preserved by Q alone are generally considered to possess some degree of authority, however much they may have been corrupted by the process of transmission. The difficulty is increased by the fact that although there are indubitable signs that a promptbook was used in preparing the F text, the later edition is evidently derived in part from Q itself. The usual explanation, which is not the only possibility but which will serve for discussion, is that F was printed from a copy of Q which had been copiously annotated by reference to a promptbook. Whatever the precise details, for the present the important and unassailable fact is that F is partly derived from Q.

It is one of the contentions of the present study that the problem has been obscured and further complicated by the methods and assumptions of the principal investigators, with the result that many supposedly unchallengeable pronouncements concerning *Lear* are far less soundly based than their almost unquestioned acceptance might suggest. That claim will be elaborated in the second part of the study, but a few essential points need to be made before the scope of the investigation as a whole can be explained. The first of these is the difference, and the confusion, between bibliography and textual criticism.

One definition of 'bibliography' has been generally agreed, namely that 'bibliography is the study of books as material objects'.[1] From this follow the consequences that 'to the bibliographer the literary contents of a book is irrelevant',[2] and that 'to confine the investigation of literature to what can be achieved by bibliographical means would be fantastic'.[3] In 1912, however, Greg proposed a new discipline which was to be called 'critical bibliography'. Far from excluding literary matters, 'critical bibliography is the science of the material transmission of literary texts'.[4] Later versions of the definition were to alter and to omit some of those words,[5] but we should expect to find a certain fluidity as the province of a new discipline was gradually defined. For in 1912:

> No attempt has yet been made to systematize critical bibliography, and I confess that I am by no means clear in my own mind what we shall finally agree to include under the name, always supposing that we recognize such a subject at all.[6]

As a general guide, however, 'Critical Bibliography is very nearly what is commonly meant by Textual Criticism.'[7]

The point was, and is, that there are three related areas of study. At one end is 'pure' bibliography, dealing exclusively with books as material objects – a form of archæology applied to books instead of sites, and often concerned to reconstruct the physical processes by which those books were manufactured. In the middle is the more objective form of textual criticism, which at one extreme can make use of bibliographical findings but which is itself strictly non-bibliographical. Literary texts – in whatever physical form – are its materials, and its concerns include the reconstruction of their history *as texts*. And at the other end is the 'metacritical' form of textual criticism, which includes the science or art of conjectural emendation. Unfortunately, the same name had been applied to both the second and third fields of activity. As Greg put it in 1932:

> Textual criticism involves two utterly different stages, phases, or processes: so different indeed

[1] Greg, 'The present position of bibliography', p. 250; 'Bibliography – an apologia', pp. 114, 116, 121.
[2] Greg, 'What is bibliography?', p. 46.
[3] 'Bibliography – an apologia', p. 113.
[4] 'What is bibliography?', p. 48.
[5] 'The present position of bibliography', p. 250; 'Bibliography – an apologia', pp. 115 (twice), 121.
[6] 'What is bibliography?', p. 49.
[7] Summary of 'What is bibliography?' in *Transactions of the Bibliographical Society*, XII (1911–13), 10–12, p. 11.

that it is misleading to include them under a common name. ('Bibliography – an apologia', p. 128)

The obvious solution to any problems thereby arising, one would have thought, would be to enforce the distinction by finding a new name for the metacritical process. Greg's solution, however, was to coin the term 'critical bibliography' to describe the middle category – and then to omit 'critical', thus including the first and second of the three distinct disciplines under a no less misleading common name. He recognized one of several possible objections to the redefinition:

If some one were to say to me: Your definition is mere bluff; you insist that books are material objects and that bibliography is concerned only with their formal aspect and has nothing to do with the subject-matter; how can it then touch the problem of transmission, by which you mean textual criticism, a study essentially concerned with the subject-matter and not with the external form of books? – if any one were to argue thus he would be raising no merely captious objection, but would be penetrating at once into the very heart of the matter. (p. 121)

But Greg's reaction was to argue the merits of textual criticism, and to return to his position with the basic objection evaded and the bluff reasserted.

You will now understand why I hold that bibliography – or, if you will, critical bibliography, that essence of the subject that alone justifies its claim to rank as a serious science – is in fact the same as textual criticism. Now textual criticism is no new study – far from it – and it may be asked what is gained by seeking to give it a new name...I think that results of considerable importance to the study of literature are likely to result from the change of attitude implied by recognizing that Textual Criticism and Critical Bibliography are synonymous. (pp. 129–30)

There can be no doubt that Greg's textual work revolutionized textual criticism, and it is possible that the change of name may have contributed to the influence of his methods. But we are left with two other questions of no less importance than that which Greg declined to answer. How can Critical Bibliography, excluded from the province of Bibliography itself by the latter's oft-reasserted definition, be the *essence* of what excludes it? And if 'bibliography' can refer to either or both of two mutually exclusive subjects, how can its three meanings be distinguished? For Greg soon ceased to use the all-important qualifying 'critical', as in:

Two fundamental errors have been made, which are really distinct, though their results are not always easy to separate. The first has been the failure to distinguish between *critical* and metacritical *problems*, and the second to realize that the former *are essentially bibliographical*. (p. 130, my italics)

It could be claimed that a third fundamental error had just been committed.

Greg's new definition of 'bibliography', although it was possibly even more misleading than the earlier definition of 'textual criticism', became current. Later, therefore, it became necessary to redivide the subject into 'textual bibliography' (textual criticism) and 'analytical bibliography' (bibliography),[1] but those terms still fail to make the essential distinction very evident. On the assumption that I have no less right to choose and to state my own working definitions than has

[1] For example, Bowers, *Bibliography and Textual Criticism*, pp. 7–8, 24–30.

anyone who aspires to practise either subject, I shall hereafter use 'bibliography' as if it meant 'the study of books as material objects'; 'textual criticism' as if it meant 'the study of the transmission of literary texts'; and terms like 'emendation', 'restoration', and 'conjecture' to refer to various metacritical activities. I hope that this will not cause undue confusion.

One of the discussions which has exercised most influence on studies of the *Lear* problem is a lecture given by Greg in 1933, which was printed in *Neophilologus* in the same year under the title 'The function of Bibliography in Literary Criticism illustrated in a study of the text of *King Lear*'. When it first appeared in print, A. W. Pollard claimed in a letter to Greg:

> that for weight of argument, conciseness, and the pleasure with which it can be read the lecture is your masterpiece.

In 1959 F. P. Wilson referred to it as 'perhaps the finest statement of his views on the relations of bibliography and textual criticism',[1] and in 1966 J. C. Maxwell could still describe it as 'one of Greg's most notable writings'.[2] The title, however, is somewhat misleading, since what Greg meant by 'Bibliography' did not remain consistent throughout the lecture. The word meant, by turns, 'bibliography', 'textual criticism', or both: a fact which Greg did not make clear in his opening exposition.

> I have myself argued that essentially [bibliography and textual criticism] are the same. But that is generally thought to be a fanciful, and potentially dangerous, heresy of my own, and I have not come here to advocate or to defend it.... My thesis to-day is that critical bibliography, as I sketched it a moment ago, can throw a great deal of light on textual problems. (p. 244)

Greg had indeed sketched (but not defined) critical bibliography – under the name 'textual criticism' (pp. 242–3) – and textual criticism can undoubtedly throw light on textual problems. He had then stated clearly that bibliography – whose function in *literary* criticism he was to illustrate – 'is the study of books as tangible objects' and that 'it is not concerned with their contents in a literary sense' (p. 243). He subsequently turned (p. 250) to the task of examining the chosen text.

> I shall seek to illustrate my thesis by taking a particular case – the text of *King Lear* – and showing how at every step the problem, or rather the series of linked problems, involved depends for solution upon bibliographical principles and methods....The textual study of *Lear* involves five distinct though related problems: first, the number and order of the early quartos; second, the differences of reading that exist between the several copies of the earliest of these; third, the manuscript used by the printer of the first quarto; fourth, the copy used by the printer of the first folio; fifth the relation between the quarto and the folio texts, and the procedure a modern editor should adopt.

Greg showed first (pp. 250–1) that some years after the Cambridge editors had come to the belated textual conclusion that Q1 was earlier than Q2 their conclusion was confirmed by P. A. Daniel's bibliographical arguments, and later still by

[1] F. P. Wilson, 'Sir Walter Wilson Greg', *Proceedings of the British Academy*, XLV (1959), 307–34, p. 321. The quotation from Pollard follows Wilson's own comment.

[2] In Greg, *Collected Papers*, p. viii.

Greg's own bibliographical proof that Q2 was a falsely-dated reprint of 1619. He stated (pp. 251–2) that 'a trifling bibliographical detail' showed that there were still unanswered bibliographical problems connected with the Q1 variants, but that anyway 'their nature is tolerably evident, and that matters most for the text'. He suggested that:

> the general impression left by the Quarto is that the printer had before him copy that was entirely undivided metrically and altogether without punctuation. There is, indeed, some slight bibliographical evidence to this effect. (p. 253)

He did not, however, consider it necessary to state that 'slight bibliographical evidence', but relied instead on a comparative criticism of F and Q readings to show the nature of Q's manuscript copy (pp. 253–6). He then amplified the textual demonstration of Daniel and Chambers that F derived partly from Q (pp. 257–61), and stated that:

> We have now concluded our bibliographical discussion of the Quarto variants and the copy that must be supposed to lie behind the two textual authorities. (p. 261)

As illustrated, the functions of bibliography are these: to confirm pre-existing textual conclusions; to lend 'slight' support to a 'general impression'; and to indicate further bibliographical complexities in a problem to which the answer is nevertheless 'tolerably evident'. What Greg was really concerned to illustrate was a method of textual criticism, which method could lead to more reliable conclusions than could literary and metacritical speculation. Unfortunately, however, the method was not only misnamed, but also misapplied.

Concerning the source of the Folio text and the nature of the printer's copy, Greg claimed that:

> If the evidence already summarized be held to prove that the Quarto offers a reported text, then I think it follows inevitably, from the community of errors and other peculiarities in the two authorities, that the Quarto played some part in the bibliographical history of the Folio. (p. 257)

If 'bibliographical' includes 'textual' then the second part of the statement is undeniably and crucially true, but it is not dependent upon the condition suggested. If any Folio readings can be shown to derive from textual or bibliographical peculiarities of the Quarto, then it follows without qualification that the Quarto played a part in the textual history of the Folio. If the Quarto was merely used in the preparation of manuscript copy for the Folio the dependence is strictly textual, whereas if an annotated Quarto was itself used as copy the dependence may also be – in a somewhat limited sense – bibliographical. But the textual dependence holds if the Quarto was used *in any way*, and the authority of the Q text is absolutely unconnected with the matter. Daniel and Chambers, in fact, had already shown that F was partially dependent upon Q, and Greg added still more textual evidence to the demonstration (pp. 257–60).

The point is more important than it may appear. The Quarto contains a text of (initially) unknown authority. The Folio contains a text which derives partly from Q and partly from another source of equally unknown authority, by reference to which numerous Quarto readings have been altered. If the alterations can be shown to be of high authority, they may be either authoritative corrections of a corrupt Q text or Shakespearean revisions of an earlier authoritative draft. We cannot tell which unless we know the authority of Q. If, on the other hand, the alterations are devoid of authority, they may be either deliberate sophistications of an authoritative Q text or conjectural emendations of a corrupt version. We cannot tell which unless we know the authority of Q. The first point which must be made is that no conclusion concerning the authority of the non-Quarto source of F can in any way predetermine the authority of Q. The second is that whatever is concluded about the non-Quarto source, no conclusion can be drawn about the Folio text *as a whole* without a prior and independent knowledge of the authority of its other source, namely the Quarto.

Greg claimed that in any attempt to decide on the physical nature of F's copy:

> The view we take will depend a good deal upon what opinion we have formed respecting the Quarto text. (p. 257)

This is untrue, for the authority of a text and the physical form in which it exists are not necessarily connected. But the view we take of the *text* of the Folio is entirely – not merely 'a good deal' – dependent on a conclusion concerning the Quarto. The alteration of Q was something which could not be done before Q existed. Our evidence that the alterations were in fact made is dated fifteen years later than Q. If they were made for the purpose of bringing Q back into conformity with an earlier and authoritative text from which it had been corruptly derived, then wherever the task was perfectly performed there is a sense in which at that point F represents an 'earlier' text. But that possibility is one of many, and in order to establish it we have first to study the Quarto. To presuppose that F is earlier will inevitably lead to the re-emergence of the presupposition in the guise of a conclusion, since it excludes all other possibilities. The differences between Q and F are not – most emphatically not – primary evidence for Q's origin. They show only what was done to Q at some unknown date after its publication. The only source of primary evidence for the origin of Q is Q itself. Logic therefore demands that the first step in any investigation into the relations between Q and F be an independent evaluation of the historically and genealogically earlier Quarto text. Greg's statements concerning that text, therefore, form the key part of his argument. Among the phrases we are offered are:

> persistent substitution of another (and generally inferior) reading for that of the Folio (p. 253)
>
> the expansion and dilution of the text, on the one hand by the introduction of exclamations, vocatives, and connective words generally, on the other hand by the use of looser and less close-knit phrasing (p. 253)
>
> a number of speeches assigned to the wrong character, and sometimes modified to suit (p. 254)
>
> inarticulate sounds and meaningless refrains indicated in the Folio, are in the Quarto either omitted or quite otherwise expressed (p. 254)
>
> it is surely the loss of a line and a half that has occasioned the [Quarto's] reconstruction (p. 256)

On each occasion the Folio is held up as the pre-existing arbiter of what Q should have contained. And the Folio did not exist until fifteen years after Q was printed, twelve or thirteen years after Shakespeare left London, and seven years after he died. Furthermore, to suggest that Q's supposed corruption is shown both by its 'introduction' of exclamations and connective words and by its 'omission' of inarticulate sounds and meaningless refrains might perhaps suggest special pleading. And open prejudice is evident at another point in Greg's argument:

When in the line:

That justly think'st, and hast most rightly said: [Folio, qq2^{v}b37]

the Quarto transposes the words 'justly' and 'rightly', the blunder is perhaps not beyond the range of original sin latent in a copyist. (p. 255)

Historically speaking, the Folio transposed the Quarto's words, and we cannot begin to suggest why it may have done so until we have decided on the nature of the text which it altered. Greg's tone shows that he has himself decided – but that decision does not appear to have been based on the evidence presented. Even were there a legitimate critical argument in favour of F (and Greg offers none), the 1608 version of the line can hardly be claimed as self-evidently inferior to the version in the 1623 Folio – a book, incidentally, in which 'iuſt' was set for 'right' in *Troilus and Cressida* I iii 164 and in *Richard II* I iii 55 (the latter case rhyming with 'fight').

What Greg offers as an assessment of the Q text is not in any sense a bibliographical argument. Nor is it an argument derived from a consideration of the material aspects of textual transmission, an essential historical fact of which Greg ignores. Nor is it really a *textual* argument as such, but the polemical defence of a presupposition founded on a purely metacritical judgement. The argument is, in fact, completely circular. Folio and Quarto readings are compared in that order to show the differences between them; the differences are assumed without stated foundation to be Q's alterations of F; and the fact that there *are* differences condemns Q as a corruption – a corruption, let it be stressed, of a late and heavily-altered reprint of itself.

The marked difference between the Quarto and Folio texts and the general superiority of the latter are, indeed, commonplaces of criticism. (p. 257)

And commonplaces of (literary) criticism do not always form the most reliable foundations upon which to base supposedly textual arguments.

Before resorting to metacriticism, however, Greg had given an initial description of the Quarto which was almost unaffected by his opinion of the literary quality of the texts. The description concluded with the suggestion that:

the printer had before him copy that was entirely undivided metrically and altogether without punctuation. There is, indeed, some slight bibliographical evidence to this effect. (p. 253)

There is no such evidence. Two-thirds of the Quarto verse is divided in what editorial tradition has accepted from F as the correct form. Elsewhere there is *textual* evidence (as will be shown in Volume II) which suggests that Q's copy was mislined at certain points in particular ways. There is textual evidence that many passages may have been underpunctuated, or punctuated in misleading ways. And there is abundant bibliographical evidence throughout the Quarto which shows that the lineation and punctuation of the printed book were continually affected by a printing-house problem which had nothing whatever to do with the lineation, punctuation, legibility, or authority of the copy. As Greg observed:

Criticism [has], indeed, left the subject in a very unsatisfactory state. (p. 250)
I am indined [*sic*] to think...that there still remains a problem for investigation. (p. 262)

In partial defence of Greg's arguments it must be acknowledged that he was neither the first nor the last to apply circular logic to the *Lear* problem. Almost every investigator has presupposed that Q's text must derive from F's, and that most of the differences must therefore result from Q's failure to preserve the F readings. Even those who have not openly started from that position have implicitly accepted it before very long. The hypothesis may perhaps be true – it would be as unsound to reject that possibility as it has been to ignore all others – but while it remains a premiss its status as a conclusion is unsupportable. Any investigation must necessarily begin by allowing the Quarto evidence to speak for itself. The nature and scope of the present investigation have been dictated by that need.

The Quarto can be investigated both textually and bibliographically, and while it is not always easy to distinguish between the two approaches it is my contention that the distinction must not be forgotten. Readers of seventeenth-century books will frequently observe a combination of ink-marks in the shape of 'aud', and the presence of those marks on a page is a bibliographical fact. It is often permissible to make the strictly textual inference that the compositor intended to arrange three pieces of metal in such a way that the resultant marks would be 'and', but that is *not* a bibliographical fact. If the central mark is the same as that normally produced by a type which usually prints 'u' rather than 'n', then after making the textual inference we may make the subsequent bibliographical inference that the 'u' was in the 'n' box. In another example we may observe that the middle symbol is that which normally appears as 'n', but that on this occasion it is inverted. That is the bibliographical observation, and it remains a textual inference that the intended reading was 'and'. It is inferred after a consideration of the 'meaning' of adjacent sets of marks in addition to the characteristics of the marks in question, since in another set of textual circumstances the compositor's intention might really have been to set 'auid' or 'baud'. Yet again, if it is a bibliographical fact that the 'a' can be recognized as a mark made by an identifiable type, and that the same piece of metal elsewhere contributed to the shapes 'ſiaple', 'coapoſitorial', and 'aiſtake', it is from a combination of textual and bibliographical considerations that we conclude that the 'a' was in the wrong sort-box and that the reading of copy was probably 'mud'. It is not generally necessary to separate and to define the elements of a compound inference, but it may sometimes be important to recognize that they are in fact separable. The inference that 'aud' in print betrays 'and' in the printer's copy can be made from a photocopy of a typed transcript of a shorthand report of a telephone conversation about a facsimile of a printed page, and need not require any more direct observation of the original page as a material object. The fact that textual and bibliographical evidence can be considered more or less simultaneously does not mean that the two terms are synonymous.

There have been several studies of the *Lear* Quarto as a material object. Greg himself conducted a masterly examination of the press-variants, and drew attention to many of the typographical peculiarities of the book in addition to analysing the textual significance of the variants.[1] Both he and McKenzie have discussed the question of the compositors' measure,[2] and Bowers has offered an outline reconstruction of the book's printing history as indicated by the recurrence of identifiable running-titles.[3] With the exception of the variants, however, few of the physical features of the book have been adduced in support of textual arguments. The reason is not that their evidence is textually irrelevant, but that no context has been established in which their significance can be evaluated.

Bowers touched on the problem when he said of the sequence in which the *Lear* running-titles reappear:

> I have examined a number of Elizabethan plays but have not seen another like it.[4]

If *Lear* is unusual in this respect, then the reasons for its peculiarity may well be significant. The context which matters most, however, is not 'Elizabethan plays' – nor even Jacobean play-quartos (*Lear* is Elizabethan neither as a play nor as a book). Different printers may have used different methods, and while it would certainly be important if Okes's norms were found to differ from those of his contemporaries, the *Lear* Quarto itself can only be defined as 'typical' or 'unusual' in the context of Okes's norms. There have been studies of other Okes play-quartos of different dates, and it is possible that some of their findings may provide useful supporting evidence. But even the context 'Okes play-quartos' is insufficiently exact. At different dates and with different workmen, methods might vary considerably – and there is no guarantee that a feature found only rarely in play-quartos was not an absolutely standard feature of Okes's other books.

In the present volume, therefore, I have attempted to consider the *Lear* Quarto as a product of the Okes printing house, and have examined the other books of 1607–8 to provide the relevant context. But it has been necessary to extend the limits in order to study specific bibliographical problems for which the 1607–8 evidence is inadequate. The larger context of 1605–9 has provided some infor-

[1] Greg, *Variants*, *passim*.

[2] Greg, '*King Lear* – mislineation and stenography', *The Library*, 4, XVII (1936–7), 172–83, pp. 181–2; McKenzie, 'Indenting the stick', *passim*.

[3] Bowers, 'An examination', pp. 30–8.

[4] *Ibid.* p. 25, n. 2.

mation concerning the way in which Okes's methods differed from those of his 1605–6 predecessors, and the way in which he appears to have increased his productivity in 1609. I have therefore outlined the history of the printing house before 1607. For the study of press-correction it was necessary to seek out surviving proofsheets, and since such evidence does not survive in quantity the examples reproduced and discussed range in date from 1606 to 1636. Printers' copy is even less easy to find, and the discussion of Okes's use of copy deals perforce with a titlepage layout of 1611 and a book (or rather, half a book) of 1613.

The main subject of the complete study, however, is not Nicholas Okes's printing house but the First Quarto of *King Lear*. As Greg put it in 1912:

> I freely confess that my own interest in bibliography is by no means purely bibliographical. It is literary. I stumbled into bibliography by accident. Finding it impossible to obtain the information I required about a certain class of literature, I set to work to collect it. It was the results of bibliography that I wanted.[1]

The controlling factor throughout this investigation has been the need for information which can be applied – however obliquely – to the study of *Lear*. In presenting that information I have not usually explained in advance the way in which it will be used in Volume II. The relevance of the printing history of *Lear* itself, and of the discussions of press-correction and printers' copy, will presumably be obvious. What may be less immediately apparent – especially to those whose main interest is not so much in the results themselves as in their textual application – is that the discussion of printing methods and the reconstruction of Okes's work in 1607–8 form an essential background to several subsequent arguments. Some of Okes's books resemble *Lear* in one or more material ways, some differ in those respects but not in others, and some differ in nearly every way. If three or four books which share a particular feature were printed consecutively there is a certain range of possible explanations. But if those books were printed at irregular intervals, a different set of possibilities may need to be considered. It proves to be of some importance that *Lear* was the first play that Okes ever printed.[2] It is likewise significant that while *Lear*, *Belman 1*, and *Belman 2* were printed on the same kind of paper, several intervening books including *Cobler* were not. And if such points themselves are important, it is equally important to present the data and to show how the facts were established and the inferences and conclusions drawn.

The pertinence of the biographical sketch of Okes himself may seem even less apparent, and indeed it is difficult to demonstrate. But it will be shown that when Okes took over the printing house there was an immediate change in the nature of the books which the house produced. Okes moved the business to new premises, purchased new materials, established a new clientele, and appears to have

[1] Greg, 'What is bibliography?', p. 47.

[2] His name appeared on the titlepage of Gwinne's *Vertumnus* in 1607, but he printed only the preliminaries. The text of the play itself was printed by George Eld.