

FREDSON
BOWERS

The
Bibliographical
Way

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*The
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by

Fredson Bowers

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THE UNIVERSITY OF KANSAS
THE ANNUAL PUBLIC LECTURES ON BOOKS AND
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First Lecture

An Informal Talk by Elmer Adler at the University of Kansas, April 17, 1953.

Second Lecture

Peter Murray Hill. *Two Augustan Booksellers: John Dunton and Edmund Curll.* Delivered at Lawrence, October 6, 1954.

Third Lecture

William B. Todd. *New Adventures Among Old Books: An Essay in Eighteenth Century Bibliography.* Delivered at Lawrence, December 9, 1955.

Fourth Lecture

Archer Taylor. *Catalogues of Rare Books: A Chapter in Bibliographical History.* Delivered at Lawrence, November 30, 1956.

Fifth Lecture

Jacob Zeitlin. *What Kind of a Business is This? Reminiscences of the Book Trade and Book Collectors.* Delivered at Lawrence, January 17, 1958.

Sixth Lecture

Fredson Bowers. *The Bibliographical Way.* Delivered at Lawrence, November 14, 1958.

A WELL-AGED SLICE OF AMERICANA goes something like this: "There's the right way of doing a job; there's the wrong way; and By God, gentlemen, there's the Navy way." I would suggest that there is also the bibliographical way.* This *way* is a method, a state of mind if you will; it is also one road to truth. I should not dare to assert that it is the only road, for manifestly it is no such thing. I should be content even to have the bibliographical way described as the low road to truth, and the way of literary criticism as the high road. Those of us who—fictively—plod along this somewhat dull and unadventurous low road may comfort ourselves, however, that without the distractions presented by the variety of scenery and the precipitous dangers of the path, we shall arrive safe in Scotland before our more intellectual colleagues.

Of course, I really do not believe in the terms of this over-elaborate metaphor, for which I apologize. The bibliographical way has its unexpected twists and turns, its ever-present dangers, and I may say its excitements. For myself, I find the discovery of demonstrable truth to be an exciting venture, worth all the drudgery that the bibliographical way normally entails. Too often, it seems, the uninitiate confuse the means with the end. No more serious error could be made. That bibliography depends upon

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ascertained fact and like the whale must strain some acres of plankton to build up a dinner does not mean that the results are dull and insipid because the method has superficially seemed to rely more on doggedness than on intellect. There are many mansions in the house of bibliography, many levels of truth. Some, I grant, are modest and inconspicuous in the extreme. The turrets of others may rise to gleam in the sunlight. I propose in this lecture to examine, with more brevity than they deserve, some of the means by which the bibliographical way leads to literary truth. My purpose is to explore various aspects of the bibliographical method and to try to analyze something that I can describe only as the bibliographical state of mind. Incidentally, and for illustration, I shall consider a few relations of its discipline to that of conventional criticism, but only for the sake of clarifying the peculiar methods of bibliography.

That bibliography concerns itself with books treated as tangible objects is a truism. But, truism or no, the baldness of this statement has led to various misconceptions. In my discussion I must pass over the Enumerative form. The general usefulness of arranging the titles of books systematically for certain purposes requires no words from me. What I am concerned with is Analytical Bibliography, or the examination of books as tangible objects in order to recover the details of the physical process of their manufacture. This general method starts with what may be called historical study: What can be gathered about the printing process from external evidence such as printers' manuals, contemporary references, craft records, and so on. Then by an independent study it endeavors to recover exact details about printing methods in general from the scientific analysis of the physical evidence of the books themselves. Next, it may endeavor to apply the knowledge thus gained in order to interpret—from specific evidence in any given book—the effect of the production process on the physical characteristics of the book as a whole, considered as part of an edition, and of any of its variant copies that comprise issues and states. Finally, this analysis of the effect of the

production process on the physical characteristics of a specific book may be developed in one of two ways, either as the foundation for descriptive bibliography, or as the foundation for textual bibliography, both of which serve as intermediaries between the book and the literary critic. In a nutshell this is the method, and the process, that I am concerned with today.

The determination of the physical, external appearance of a book in terms of the technical analysis of its production, which is the function of analytical bibliography applied to description, does not collide in any direct manner with other forms of analysis that go by the name of criticism. But the relation of analytical bibliography to textual bibliography, and thence—immediately—to textual criticism, is subject to misinterpretation, principally because analytical bibliography thereby invades a field that has customarily been preempted by some form of literary criticism. The examination of a book as a tangible object, consequently, applies to something more than the easily understood concept of its description as a dimensional object without relation to its contents. If we take it, in addition, that analytical bibliography is as much concerned with the contents as with the external form of a book, we may see its application to textual criticism. The peculiarity of the approach to textual bibliography, however, is this. The contents—the author's words—are not thought of primarily as symbols instantaneously to be resolved into meaningful concepts in the mind. Instead, at least at the start, the words and punctuation are thought of primarily as simple inked shapes, imprinted on paper from pieces of similarly shaped metal selected and arranged according to a system by some human agent, the compositor. This comprises another and perhaps less familiar notion of the book as a tangible object. Accordingly, the function of analytical bibliography is to treat these imprinted shapes, their selection and arrangement, without primary concern for their symbolic value as conceptual organisms but, instead, as impersonal and non-conceptual inked prints. Thus the general laws governing the selection and arrangement of the pieces of metal and

their transfer of tangible inked impressions to paper are the concern of bibliography. Yet these general laws must always be related by analysis to the peculiarities of the evidence at hand in the specific book, and indeed to any specific copy of the specific book.

To determine the exact details of the mechanical process that produced the sequence of these inked shapes, and the selection and the order of each shape within an arrangement, therefore, is the primary end of textual bibliography. In other words, the heart of the technique consists in supplying a mechanical explanation for all phenomena whenever such an explanation can be arrived at on the available evidence. Since the transfer of inscribed symbols in an author's manuscript to the forms of impressed symbols in a book is a mechanical process, any explanation that can be made within the terms of the mechanical process for phenomena relating to these transferred symbols is to be preferred to explanations that ignore the process and seek some other terms of reference. This is the simple difference between a bibliographical and a critical explanation. However, such a statement does not go quite far enough. Bibliography has its own laws of evidence that must be observed, else its way is being violated.

It will sometimes happen that a critic will use a conjectural method proper enough for his own discipline, but improper for bibliography, within the terms of the mechanical printing process, and the results may too easily deceive the innocent. For instance, John Dover Wilson—who is a constant offender in this respect—makes certain statements about the text of *Hamlet* resting on a conjecture that the proofreader misconstrued the original compositorial typesetting and altered a word for the worse during the course of printing. In the known copies we have the misconstruction, he argues, but the lost original may be conjecturally restored. This sounds very 'bibliographical' since it appears to explain a phenomenon in terms of the mechanical process; but actually it is no such thing. Wilson is violating here a primary doctrine of bibliographical scholarship that conjectures based on our general

knowledge of printing as a process cannot be applied without specific evidence in the book itself from which inferences can be drawn in the light of general practice. Wilson's hypothesis is the product of pure guesswork, without a shred of evidence in its favor, and therefore it has no more validity because it is cast in language that refers to printing than if he had framed it in critical terms of value-judgment. It takes more than bibliographical language to achieve the bibliographical method. The bibliographical way is not the critical way with a vocabulary sea-change.

Analytical bibliographers must use as source studies the various contemporary accounts of the printing craft in different periods, but the evidence of the books themselves is the more detailed and valuable, at least in earlier times. An example that is perhaps not wholly typical—though certainly not unique—may be drawn from the process of proofreading. We know from historical accounts that can be checked from the evidence of the books, as McKerrow showed, that in Elizabethan days type was not set into long galleys, with the ensuing process of hand-pulled galley proofs, correction, page proofs, correction, and printing. Type was set directly into pages, and all proofreading was done at this advanced stage. Moreover, owing to the stiffness of the ink, proofs could not be pulled by hand but required the considerable pressure exerted by a press. The earliest English printer's manual, written by Joseph Moxon in the Restoration, describes the mechanical part of the proof-correction process as follows. When the compositor has prepared and locked up a forme (that is, the type-pages that print one side of a sheet of paper), he takes it to the pressman, who is obliged to stop printing, to remove the forme on the press, to substitute the forme to be proofed, with this forme to print a few sheets on one side that can be used as proofs, to remove the proofing-forme, substitute the forme he had been in process of working, and resume printing. After the proofs are read, the compositor will correct the type and in due course the forme is sent to the press. From this period we also have references to proofing presses, old presses no longer fit for active service but

good enough to use for pulling proofs. If one of these were present in a shop, the pressman need not be interrupted in his work in order to secure proofs from a forme just composed.

It must be obvious that if either of these methods were employed, the proof-correction of a book would be as concealed as it is in modern times, and any variants would come—as in modern times—from the emergency repair of errors. However, on the evidence of the books themselves Moxon's description appears to have been largely theoretical, for many—perhaps most—earlier seventeenth-century books exhibit variants within their sheets that are too numerous and too systematic to result from second thoughts alone. Instead, from the books themselves it becomes clear that a quite normal method of proofing was to start printing, to send an early sheet to the proofreader, but to continue printing with the uncorrected type until the marked proof was returned; whereupon the press was stopped, the type corrected, and printing then resumed with the corrected state of the type.

It is clear that such press-variants are of prime interest to an editor, who must embalm his choice from among them in a fixed text, and they are occasionally of import to a textual critic. Any process that alters the details of what the compositor set while looking at the author's manuscript must always come in question. When their existence in books was discovered, these press-variants were almost automatically equated with the several accounts we have preserved of authors attending the press to read their own proofs; and hence the corrections were at first assigned the highest authority. It is only after many years that soberer evaluation has prevailed and we have come to realize that though author correction is not unknown—as witness Ben Jonson's *Folio*, Herrick's *Hesperides*, and Dryden's *Indian Emperour*—the burden of proof is now on the critic who assigns such variants to the author. Positive evidence from the nature of the variants themselves in favor of authorship is now required, and negative inference based on external evidence will no longer serve. The change in attitude now means that an editor should view any alteration in press with

suspicion instead of trust, for it has become evident, on the face of it, that most press-variants represent the proofreader's own ideas, which may readily differ from those of the author; and that the proofreader seldom bothered to consult the author's manuscript when reading proof. Hence the authority of most such alterations is nil, and though some may correct compositors' mistakes, many succeed only in sophisticating the text and placing it at one further remove from the authority of the lost manuscript.

This new view of the variants has been arrived at by literary as well as by bibliographical evidence (as from posthumous books), and it may be useful therefore to contrast briefly the two methods as applied to the general problem. Literary judgments ought to derive from evidence, of course, and thus should ultimately refer back to a factual basis even though the major test for the interpretation of the fact is the intellectual satisfaction gained from the explanation that will lead to common acceptance. A literary critic, for instance, can read this dialogue in Dekker's *Match Me in London* (1631):

King. Whom hast thou poyson'd?

Doctor. The Queenes Father.

And when the proofreader alters 'Father' to 'Father in law' (a character that does not exist), such a critic is certainly justified in believing that the wrong meaning is sufficient factual evidence for a hypothesis that someone other than the author made this change. But suppose, as often happens, that the corrections in a forme appear to be minor and so relatively indifferent as to meaning that no evidence can be gathered whether the author or the proofreader was the agent. In such a case the fact of alteration is still present but the critic is helpless, for the evidence is insufficient to support a value-judgment that can be offered to the test of general opinion. Hence a discussion of the origin of the alterations can be only speculative, whether thinly disguised as an appeal to common experience or as some other proposition.

Analytical bibliography may not always be able to solve a

specific problem either; but occasionally its peculiar technique will yield results that are demonstrable in a manner impossible for critical methods. Suppose, for instance, that an alteration in press occurs on page 1 of an early book, and another on page 2. The non-bibliographical critic might assume that any hypothesis he can make about the first variant will apply equally to the second, because of its proximity; and he might be tempted to appeal to similarity in his values-judgment as confirmation. On the contrary, the bibliographer's insistence on founding a case (at least at the start) only on some assured fact from tangible printing evidence—the bibliographical way—demonstrates not by opinion but by the requirements of the printing process that there is no necessary connection between the variants on pages 1 and 2. If the pages are part of a full sheet, they will have been physically separated in the printing and proofing process because page 1 will be in the outer forme (or the series of pages that locked up together as a unit printed one side of the paper) and page 2 was in the inner forme (or the series of pages that as a separate unit printed the other side of the paper at a quite different time). Separate printing is ordinarily the cause of separate proofreading. The same agent *might* have read proof on both pages, but there is no physical evidence resident in the printing process that he did. No necessary connection can be established between the two pages without further specific information.

Still treating these two pages and their variants with no regard for the derived meaning of their symbols but only as physical objects that have received markings in ink on one side and then on the other, the bibliographer may continue his examination. He knows that proofs can be pulled only on a press, and not by hand pressure. Moreover, to imprint one side of a sheet of paper the type-pages must be correctly arranged in relation to one another and firmly locked up in a frame, called a chase, a process that we loosely call imposition. To a critic seeking further physical information about the possible relationship of the variants on pages 1 and 2, the bibliographer can say this. If only one set of

materials, what is called a skeleton, were made up to impose the type-pages for all formes in a book, it is very clear that this skeleton cannot have been used to lock up one forme of a sheet for proofing until the other forme had altogether finished its printing, Moxon to the contrary. When a bibliographer from its running-titles identifies such a single skeleton, he can tell the critic that at least five or six hours separated the proofreading of page 1 from that of page 2. Thus the agent may have been the same but there is no physical requirement that he should be, and any assumption must rest exclusively on literary evidence. Nevertheless, if two different skeletons can be identified as imposing the formes of this sheet, the critic can be told with some positiveness that—under the conditions of normal printing—if there is a variant on page 1 no variants will ordinarily appear on page 2, and the type of page 2 will have been corrected by the same agent who proofread page 1.

On the other hand, if the pages were 2 and 3 in, say, a folio in 2's, and in some copies alterations were found in these pages, the bibliographer can guarantee the critic that there is a necessary physical connection between these two pages in the same forme, and that the odds are the alterations were ordered by the same agent in both pages. Even if in some copies changes were made on page 2 but not on page 3, and in other copies the page 3 alterations appeared beside those of page 2, the bibliographer could describe the process by which proof might be read and corrected one page at a time (as occasionally in the Shakespeare First Folio and in Dekker's *Magnificent Entertainment*) so that a provisional inference could be drawn that the proofreading of the two pages was actually continuous even though the correction in the type was performed in two parts. However, it would not be the bibliographical way to assume this continuous reading on general information about printing practice alone: general knowledge of procedures must always be used only to confirm the specific evidence of the book in question. Thus if the bibliographer collated twenty copies of the book and found no variants on either page 2

or 3 in five copies, alterations on page 2 but not on page 3 in one copy, and changes on page 3 added to those on page 2 in the remaining fourteen, he would be justified in the inference that very few copies of the singular alterations on page 2 were printed before the press was stopped once more to correct page 3. Hence it would be a reasonable hypothesis that the reading of the proof was continuous and performed by the same agent. On the other hand, if he found, say, five invariant copies in the original state, ten copies with the alterations only on page 2, and the remaining five with the added page 3 variants, the proportions would suggest by extrapolation that about one-half of the whole time required to print this forme had elapsed between the two stages of press-correction, or perhaps about three hours, and therefore the reading was very likely not continuous. In such a case the agent may have been the same, or a different person: the interpretation of the presswork can have any affirmative bearing on this problem only when continuous proofing is indicated.

This illustration could be carried forward into areas of some complexity. For instance, if two skeletons were utilized to print the sheets of the book, the situation would be quite different and would call for changed sets of time-schedules owing to the marked difference in the details of the printing process. In turn, such inferences would differ according as the bibliographer was able to analyze the evidence to see whether the book were printed with the compositor setting each page in order, or whether he used cast-off copy and skipped so that he set the diverse pages that make up one forme before returning to fill in the gaps with pages of the other forme. The discovery, made only recently, that this method of typesetting was commonly used in Elizabethan times has quite revolutionized the interpretation of bibliographical evidence, and we are still trying to find our bearings as a result.

In another situation that the bibliographer would recognize—but the critic would not—if the pages 1 and 2 with their variants were part of a half-sheet gathering, there might be as close a connection between them as between pages 2 and 3 in a full-sheet

forme; yet if the half-sheet type-pages were imposed according to an alternative method, there would be no connection at all. These technical issues illustrate how improper it is for critics to deal with evidence according to their literary standards until the absolutely basic facts have been determined by informed bibliographical analysis. How helpful, indeed how crucial, such technical investigation may be has been illustrated in the conclusions that have been drawn in the second volume of the Cambridge Dekker about the authority of the text of *The Magnificent Entertainment*. Here analytical bibliography furnished the textual critic with the necessary information for a series of reasonings that led to the view that Dekker himself had corrected the proof for a few specific pages and that the variants in the other pages were unauthoritative. Moreover, when a revised second edition was printed, the bibliographer could determine largely on mechanical evidence which revisions were Dekker's and which were not.

The Honest Whore is another Dekker play in which once the mechanical facts were determined and logically interpreted to relate to general printing procedures (a process not always so simple as it sounds), purely critical inference about Dekker's revisions was almost completely confined by bibliographical facts, and the lot of the editor was made surprisingly easy.

For example, when in 1933 Hazelton Spencer came to Dekker's *Honest Whore*, Part I, in his *Elizabethan Plays*, an anthology that for its day was most conscientiously edited (far in advance of conventional practice for anthologies at the present time, as a matter of fact), his use of the then newly discovered second edition was eclectic in the old-fashioned sense, since he had not solved the bibliographical problem of the exact printing relationship between the two editions, the retention of standing type from the first in the second edition, and the differing relationship of the variants in the standing type from those in the reset type. Thus he could not define the respective authority of each edition, page by page, and so could not explain why in some places the second

edition seemed inferior but in others patently superior. Failing here, he had no possible criterion other than personal, critical taste to guide him in the choice of readings from among the numerous variants, and it was inevitable that he should not be correct in all cases. (In truth, there is no indication in his introduction or notes that he was aware of the existence of any bibliographical problem in the relationship of the two texts.)

In comparison, present-day bibliographical techniques distinguish the use in this second edition of corrected standing type from the first, and of reset type; and then proceed to analyze the textual characteristics of the variants in each according to the bibliographical units of the different sheets printed in three different shops. The bibliographical explanation of the evidence suggests why in the work of only one shop the reset formes contain authoritative revisions and corrections, although the standing formes in all shops were authoritatively corrected. Thus the problem of authority of the several classes of variants is solved according to the mechanical evidence of the printing process. Consequently, editorial eclecticism in the choice of variants now from one and now from the other edition becomes quite automatic, completely demonstrable on physical grounds, and not subject in the least to differences of critical opinion.

It cannot be repeated too often that when the evidence of analytical bibliography is available, critical judgment must be limited by bibliographical probabilities and must never run contrary to bibliographical findings. Another simple illustration may be given. Suppose we have a sheet in an Elizabethan book that shows press-alterations in both formes, that is in the pages on each side of the unfolded sheet of paper that was the printing unit. The problem is—which are the original readings and which the altered in each set of pages. Often there will be little difficulty in deciding about the changes in one forme, for the printer usually took advantage of proofreading to correct literal errors such as a turned piece of type, transposed letters, obvious misspellings, and so on; and hence the direction of the changes will be clear. But

on other occasions the variants may be so indifferent as to give rise to legitimate doubt, especially when they are few in number and therefore not cumulative in their evidence. In such cases when the order of change in one forme can be determined, the order of the variants in the other forme can usually be demonstrated on incontrovertible mechanical evidence that has no need for critical conjecture. For instance, in Dekker's *Match Me in London* (1631), a quarto, there are thirteen press-variants in the inner forme of sheet G, and only two inconsequential punctuation changes in the outer forme. The literary world will not come to an end if the order of these latter cannot be proved; but any poor devil of an editor must know which is which before he can have a reasonable basis for deciding what to accept for his text.

The solution is quite elementary. Under most conditions when the press had finished printing one side of the pile of paper that comprised an edition-sheet, the pile was turned over so that the first sheets printed, say, with the inner forme would now be on the top of the pile and would become the first sheets, also, to be printed on the other side by the outer forme. When this happens, the earliest sheets will always represent both formes in their original, unaltered state, and an overlap will later occur in which the corrected state of the type of one forme will back the original state of the other, and then the printing will continue so that the corrected state of the laggard forme will appear in connection with the corrected state of the other. Among the thirteen variants of the inner forme in the *Match Me* sheet are four corrected literals that show beyond question which was the order of change. We may then observe that of the twenty-five collated copies only five show the original readings of the thirteen variants, and these five plus seventeen examples of the revised forme are backed by the uncorrected state of outer G whereas the three known copies of corrected outer G appear in connection with the revised pages of inner G. This is evidence that could be taken into a court of law to show that those readings in the outer forme of G that were printed on the same sheet with the original readings of the inner

forme represent the first examples of outer G to go through the press, and thus only the three copies with the altered readings contain the proof-corrections.

One more example may be taken from *Match Me*, since it is so rich in illustrations of the way in which analytical bibliography applied to textual problems can produce findings that limit very severely the areas in which criticism can operate independently. The first and the last gatherings of this play are half-sheets, and each gathering exhibits press-variants made at two different times. Analysis can demonstrate that these two widely separated parts of the book were actually printed together as one sheet before being cut apart for binding at front and back, and therefore that each of the two stages of proof-correction in one half-sheet can be associated with the corresponding stage in the other half-sheet as performed at the same time and hence presumably ordered by the same agent. Thus when the critic finds some suggestive evidence that Dekker himself made one of the sets of proof-corrections within the preface, in the first half-sheet, the bibliographer can guarantee the critic that the corresponding set of alterations in the final half-sheet should also be authorial. An editor need not consult his private judgment in such a case, for if he accepts one group of changes as Dekker's, he is bound to accept the other in toto as well, and he need not be concerned about the literary evidence. The bibliographical evidence is inexorable.

The point I want to make is this. Impersonal judgment is to be preferred to personal judgment. The mechanical interpretation of analytical bibliography based on physical fact is always to be preferred to the interpretation of the critical judgment from values. When bibliography and critical judgment clash, the critic must accept the bibliographical findings and somehow come to terms with them. Critical assumptions can never be so valid as strict bibliographical evidence. Indeed, this is not a question of degree: when a clash develops, strict bibliography must be right since step by step it rests on the impersonal interpretation of physical facts according to rigorous laws of evidence; and, corre-

spondingly, criticism must be wrong since its interpretation of evidence can rest only on opinion.

Opinion that was largely based on false critical grounds decided that one of two editions in the same year of John Dryden's *Wild Gallant* (1669) was the first edition, since what seemed to be obvious errors in it were corrected in the other. But one single piece of quite impersonal evidence was sufficient to destroy the pseudo-logic of this reasoning and to show that the reverse was true. Whenever possible the bibliographical way shuns the interpretation of readings and tries to find evidence that does not depend upon assumed literary values. In this case such evidence was found when it was observed that in some copies of one edition in a particular word the letter 'l' inked fairly clearly but in other copies only its tip inked so that it closely resembled an apostrophe. And as a real apostrophe it appeared in the other edition. To reverse the order that mechanical evidence demonstrates, one would need to argue that a compositor, seeing an apostrophe in his copy, deliberately imitated it with an imperfect 'l'; or else that the whole concurrence was mere chance. A question of legal sanity might well arise about anyone disposed to uphold such a case. Nor do I need to cite the familiar story of the Pavier piracies of Shakespeare quartos and the fallacious literary judgment by which the Old Cambridge editors upheld the piracies of *King Lear* and *The Merchant of Venice* as the superior original texts, or thought that the bad quarto of *Richard III* was superior to the corrected Folio text. Too many other examples of the like exist for the point to need belaboring.

On the other hand, a note of caution must be struck. Just as the hypotheses of theoretical science are constantly being revised, so the findings of theoretical bibliography need revising from time to time as further information comes to hand. What a bibliographical fact is needs constant testing. In reference to some of the examples I have cited, let me say that the assumption of normality that is basic to bibliographical reasoning can be very dangerous if too little information has been recovered about the

variety of procedure possible in the printing process. Thus, pages of type are not always set in sequence, as we have usually imagined, and an assumption based on an incomplete general knowledge of printing practice that any specific book was typeset and machined in 'normal' order instead of by formes from cast-off copy may lead to quite false conclusions. Just so, there are known cases in which the pile of sheets printed on one side was not turned so that the printing of the other side was performed in exact sequence according to the original order of the sheets through the press, and these irregular examples can produce results quite different from those normally anticipated. All such cases come under the preliminary qualification I have tried to make that general hypotheses about printing practice must always be tested by the specific evidence of any book under examination. The bibliographical way is inductive, not deductive, when properly employed.

Moreover, there is a hidden danger difficult for laymen to detect, the ever-present danger of bad bibliography. This danger may come from insufficient general examination of evidence as well as from the misinterpretation of specific evidence. For example, in Macdonald's bibliography of Dryden, the most important edition of *The Indian Emperour* is not recorded because the bibliographer failed to compare enough copies to see that there were three editions in 1670, not just one or just two. Yet in this unrecorded edition Dryden first introduced a major revision of the text of the play. Misinterpretation of evidence (or rather the building of a hypothesis on a fact that was not truly evidential) is present in Macdonald's assignment of the second edition of *The Wild Gallant* as the first. Similarly, Sir Geoffrey Keynes chose precisely the wrong order for the first two editions of Sir Thomas Browne's *Religio Medici*.

These were serious errors because they purported to have been derived from bibliographical evidence and were affirmed by men who should have known their business. As a consequence, critics—whose judgment must always rest on the precise details

of a given text—were seriously misled for some years. The wrong order was assigned to *The Wild Gallant* editions by hinging the major case on the presumed bibliographical evidence of an ornamental initial letter. When the initial in one edition was seen not to fit the space so tightly as a slightly smaller initial in the other, and the editions were observed to be line-for-line duplicates, the theory was born that the apparently ill-fitting initial belonged in the second edition since the area of the surrounding type had been dictated by the first. This was an example of false bibliographical reasoning, for the assumed poor fit was actually a typographical convention, as should have been known. Two fallacies were present here. First, what was actually a neutral fact was used as the basis for reasoning that did not conform to bibliographical laws of evidence. Second, no attempt was made to confirm the suppositions by any further bibliographical examination, and Macdonald was content to rest his case on a very narrow band of evidence that played him false.

The Keynes error was the result of even faultier bibliographical method, and his erroneous conclusion was reached on evidence nobody would hang a dog on: the assumed order of alterations in a plate that was not even an integral part of the printing of the editions in question. In both cases it was bad enough to have a number of collectors and libraries bilked with second editions bought at fancy prices as the first; it was worse to have critics misled for years by incompetent bibliography into using corrupt texts as the basis for critical analysis. Both men were led astray because they were satisfied to do what little they could with the evidence of external features of a book and made no effort to develop the bibliographical evidence waiting in the text itself that would easily have settled the question. That neither investigated fully enough was almost certainly due to a feeling that after all both editions had been described so that a library or collector could acquire them. This attitude—which seems to have produced a certain jauntiness in Sir Geoffrey's later references to his error, as in his presidential address to the Bibliographical Society—is

the very reverse of scholarly. It reduces the importance of descriptive bibliography to catalogue proportions because it overlooks the fact that the critic is the ultimate consumer, and the purchaser (whether collector or library) is only the intermediary.

This is a point I cannot labor too strongly. Of course, the librarian must have the tools of his trade, and he requires some published basis, whenever possible, to guide him in his purchases and to identify (even if in an elementary fashion) what he has bought. Nevertheless, books are (or should be) bought not just for the sake of buying, but because they are to be put to critical use; and therefore it is the critic at whom the descriptive bibliographer must always aim.

Some of the things a critic wants to know may be summarized. He needs the information found in the systematic arrangement of books and their description in a bibliography to tell him what is the correct order of editions so that he can go to a primary edition and not base his evaluation on the inevitably corrupted text of a derived secondary edition. Moreover, when he holds an example of this primary edition in his hand, he wants to know whether it is complete in every respect, and whether different copies have physical variants such as cancels or substituted sections of text; in which case he wants the features of these to be identified and the order ascertained. He wants to know, in short, all the details of what is known technically as 'ideal copy.'

Since it is not always possible for the descriptive bibliographer to inform the critic about revisions in later editions unless they are announced in the book, the critic expects a full account of the editions after the first and a correct arrangement, with falsely dated editions, piracies, and so on, all identified and securely placed. Even after the death of the author and the end of very much possibility that fresh authority can enter the established text, the critic cannot be indifferent to the history of an author's work in later periods. The study of Restoration and of eighteenth-century adaptations of Shakespeare, for instance, is one of literary importance.

But if a critic is wise, he still wants to know more. First, he wants to know that the account of any book in a descriptive bibliography has been made up from the direct comparison of enough copies to assure him that the odds are small that a copy he holds in his hand will have textual variation from another of the same described state, issue, and edition—at least, no variation beyond presumptive press-correction. I can assure you from my experience with the collation of multiple copies of Restoration plays that this is no idle requirement. A somewhat surprising number of books will be found in which one or more sheets are printed from a completely different typesetting from that in most copies of the same edition. Almost always these represent remainder copies in which a few sheets have been reprinted, perhaps years after the original, to perfect the last sets of the sheets to be bound and sold. Always a present danger, moreover, is the sophistication in rebound volumes whereby owners insert sheets from a different reprint-edition in order to round out an imperfect copy. These are usually sold in good faith and bought in good faith, and some of this country's best libraries own such dangerous copies. (I am not referring to the Thomas Wise sophistications, either.) These generally distributed made-up copies are deadly traps for scholars, who do not realize that librarians for the most part are helpless to detect such variants and that even the fullest bibliographical descriptions may be insufficient to reveal the falsities. Of course, the more 'bibliographical' the description, the more it concentrates on the features of the book in which printing variation might produce identifiable anomalies; and the better chance there is for the detection of sophisticated copies from comparison with the bibliographical account. However, if a scholar is so fortunate as to hold in his hand a copy of an edition from a library recorded in the bibliography, he has an absolute right to demand that in no respect save press-variation should the text of this copy differ, without remark, from other copies of the same state, issue, and edition that are also recorded. Not many bibliographies are capable of meeting these demands, it must be confessed; nevertheless, it

is the bibliographical way. It is also the bibliographical way, if I may interpolate, for a library to emulate The University of Kansas in its ownership of a Hinman Collating Machine and thus to be able to check its duplicates to know precisely why it is keeping certain copies that in fact are not precisely identical.

But the deficiencies of descriptive bibliography when aimed only at the assumedly limited demands of the collector and librarian and not at the scholarly critic are too long a subject to expatiate on here. It is a false assumption that bibliography is indifferent to literary considerations. On the contrary, this method requires criticism constantly to refer the 'facts' on which its findings are based back to bibliography for validation. When Delmore Schwartz does not know how to identify a misprint, and thus interprets Yeats's poem "Among School Children" in two ways, depending upon whether one reads 'solider Aristotle' or 'soldier Aristotle,' he is making a mockery of criticism because he has not referred to the necessary bibliographical arbiter what wrongly seemed to him to be a critical problem. When F. O. Matthiesen goes into metaphysical ecstasies about the famed misprint 'soiled fish of the sea' in Melville's *White-Jacket*, he is betraying his own discipline by his ignorance of bibliography, just as surely as does Empson, who rests his whole critical theory about the point of T. S. Eliot's poem "Whispers of Immortality" upon two misprints in a late edition. The misconception that criticism and bibliography are not intimately connected goes back to that truism already cited, that bibliography deals with books as tangible objects. But since the shaped forms of inked impressions are tangible objects, and since they produce responses in a critic that flash the picture of 'solider' or 'soldier,' of 'coiled' or 'soiled,' the bibliographer is very much concerned with which is which. Unlike the critic he does not care from the point of view of the interpretation of the Yeats poem or of the detection of early metaphysical elements in Melville's style. His concern is much more narrow. He simply wants to know whether 'solider' or 'soldier,' whether 'coiled' or 'soiled,' is right; and he wishes the critic would consult

him when these questions arise, since he can very often report back with the correct answer.

So far I have tried to illustrate various cases in which bibliography by means of its particular methods can determine, analyze, and interpret facts in a manner that leaves no room for critical option. Such situations may concern large problems, such as the order of editions like the Pavier Quartos, the first edition of *The Wild Gallant*, or even the determination of the printer's copy for the Folio version of Shakespeare's *Richard III* as the third quarto or the sixth (although Mr. Walton, the New Zealand scholar who has recently attempted this, failed to develop the bibliographical method for discovering the really demonstrable truth about this interesting problem). Sometimes, however, the situations may be reduced to relatively small and narrow problems, like the determination of the authority of certain press-variants in *Match Me in London*, the Yeats reading 'solider' or 'soldier,' or the Shakespeare reading 'sullied' or 'solid' flesh.

Yet there still remains an important area in which bibliography competes with other methods to offer an explanation for phenomena that is not demonstrable by the ordinary tests for truth that bibliography can often meet. In these cases a plausible bibliographical—that is to say, a mechanical—explanation for an aberrancy must match itself against an explanation offered from conjectures based on literary or historical interpretation of the same phenomenon. It is of the utmost importance for critics to be able to distinguish the two cases—those in which bibliography can speak with absolute authority, and those in which it can offer only an alternative suggestion.

In some respects the more closely bibliography turns its application to specific and detailed textual matters, the more it is likely to enter the area of probability and to leave the area of demonstrable fact. I have some hopes for the future that this no-mans-land of probability will more and more be gathered into the realm of logical demonstration, and I see this happening, actually, in the newest and most advanced methods of applying compositor

analysis, a method sure to bring many dim areas now subject only to twilight conjecture out into the daylight of ascertained physical fact. But even when we have extended our future knowledge into many matters that are now very much a mystery, it will doubtless still remain true that superior possibility can be shown by bibliographical reasoning in some textual matters but not absolute demonstration. An interesting case in point has not been discussed in public before, and I am happy to be able to refer to it.

In Shakespeare's *Richard III* in Act IV, scene ii, some 18 lines present in the first and subsequent quartos are omitted in the Folio, which offers for the whole play a revised and much improved text. These lines are part of a scene in which Buckingham gags at Richard's proposal to slaughter the young princes, and—unconscious that he has sealed his own fate—returns to beg in vain for his promised earldom of Hereford. Although not necessarily Shakespeare's in every detail, since the only authority is the corrupt first quarto, the lines as a whole have not been questioned by modern critics, and the problem has become only that of accounting for their absence in the Folio.

The literary explanation—which in my opinion is pure romance—is summarized in the notes to the recent New Cambridge edition of the play (p. 159):

The deletion . . . of . . . some twenty lines, towards the end of 4.2 we may probably put down to the influence, direct or indirect, of the censor on the political side. The lines in question, which comprise the famous 'clock' passage at 4.2.96-114, are not, as Patrick has shown, a piece of actors' gag introduced into Q, as some conjecture, but have been cut out of the copy from which F. was printed, seeing that they refer to matter in two separate passages of Holinshed, while Richard's line (115)

Thou troublest me, I am not in the vein,

which appears in both F. and Q, is undeniably connected with l. 113,

I am not in the giving vein today.

which appears in Q1 only. Patrick was unable to explain the cut; but a year later W. J. Griffin and R. B. McKerrow pointed out that the deleted lines might well have been considered dangerous at the time when F. was

published, since the reference to the unlucky castle at Richmond (11. 100-4) would have sounded inauspiciously in the ears of King James who possessed a palace of that name, while a Buckingham whose demands upon his sovereign were repeated like strokes of a clock might seem to reflect upon James's favourite, who like 'his predecessor in the title was ambitious, grasping and importunate', and whose monopolies Parliament had actually attempted to curtail in 1621.

If we examine this reasoning carefully, we see that it is based on pure assumption without a single shred of evidence. Possibly, the audience of the time would equate the Jacobean Buckingham with the Yorkist, but we have no evidence that it ever did so or that such an equation was customary in history plays. Indeed, if history plays had been used in such a manner, scarcely a one might have escaped censure. That King James—who did not frequent the public theatre—would have objected to a reference to a castle because he had a palace of the same name is mere daydreaming. English monarchs owned the Tower of London, too, which comes in for few words of praise in the drama. If the literary critics had been able to show that the text as represented in the Folio was immediately derived from a playhouse prompt book that had been specifically cut for court performance, they might have had the shadow of a basis for conjecture, provided that similar cuts could also have been shown, but no such argument is possible. What we have, therefore, is the most insubstantial speculation applied with an air of specious confidence to try to explain a stubbornly inexplicable fact.

Here, instead, is a bibliographical explanation, and I think it legitimate to ask whether any thoughtful man would not prefer an answer such as this that has some evidence in its favor. The passage in question bridges the foot of one page and the head of the next in both Q3 and Q6, either one of which must have served as copy-text for the Folio. Thus it is not susceptible of any such rationalizing pseudo-bibliographical conjectures as compositorial eye-skip, a torn-off part of a leaf in the printer's copy, or some other elementary mechanical explanation. This would be applying gen-

eral printing procedures to a specific problem without the concurrence of evidence from the book itself. Baffled, the bibliographer might notice that in the Folio the lines come very close to the foot of the second column of signature s3^v (the equivalent of a page 6 in a Folio gathering), but this would at first sight appear to have little significance. Even when the bibliographer applied spelling tests and discovered that signature s3^v was set by Compositor A, and the next page s4 (the equivalent of page 7) by Compositor B, no immediate explanation would occur to him. It would not seem possible that the cut was somehow caused by the transfer of copy from A, finishing his stint, to B, just beginning his, because on the evidence Compositor A in his portion had resumed the scene after the gap and set nine lines of text to complete the column and the page. The change of compositors between two pages might have seemed significant if the break in the text had corresponded with the break between the pages; but it does not.

Actually, all the necessary evidence is present here, and the distinction between these two particular pages in the gathering set by different compositors is indeed a crucial one. What is lacking is an appeal to general printing practice to make the specific evidence of the book susceptible of interpretation in a rational manner. Without this information the matter would rest in troubled uncertainty, the scholar dissatisfied with the historians' fanciful reconstruction but unable to offer any positive alternative. However, Dr. Hinman has recently advanced our knowledge of Elizabethan printing in general, and of the precise printing of the Shakespeare First Folio in particular, in a quite sensational manner. We now know, thanks to his bibliographical discoveries, that the Folio was not typeset page after page in a regular order, with s4 (or page 7) succeeding s3^v (or page 6) in temporal sequence. Instead, the copy was estimated and marked off according as it was supposed to fill a Folio page, and on the basis of these paginal estimates the compositors typeset by formes; that is, the order of setting the pages in a gathering was not 1 through 12 in sequence but instead pages 6 and 7 first, then 5 and 8, then

4 and 9, 3 and 10, 2 and 11, and finally 1 and 12. This means that the amount of material assigned each page was strictly predetermined; and if the space assigned were insufficient, something had to give. This something would always be the text.

In this particular case if Compositor A had set both pages s3^v and s4 (pages 6 and 7), page 7 would have come after page 6, and adjustments might have been made if the text assigned for page 6 had been too great for the space. But since, as it happened, Compositor A started to set page 6 at approximately the same time that Compositor B started to set page 7, if towards the end of the second column on page 6 Compositor A had found that he had more lines in his copy than he had space to set them in, he would have been helpless to make adjustments because he positively had to join the text at the bottom of his second column to the text of B's heading the first column on page 7 that had been set several hours previously. Something would have to give, and it would be the text. Hence if the amount of text assigned Compositor A for signature s3^v had been miscalculated, cuts would need to be made as he neared the end of the second column and recognized his predicament. From the literary point of view this 'clock passage' is most important, but from the point of view of the play's action it can be omitted without interfering with the course of the narrative. Hence, if as I speculate the passage was excised for such mechanical reasons, the omission was selected with some intelligence.

We have this situation, then. The excised lines cannot be explained as a theatrical cut: they comprise the most extensive omission in the play and there are no other certain signs of cutting in the Folio text. The theory that they were omitted because of censorship difficulties rests on a very flimsy line of argument quite without evidence of any validity; in fact, without any evidence at all. If, instead, we seek a mechanical cause, we have the suspicious circumstance that this self-contained passage occurs near the end of a page that had to link with the beginning of the following page set several hours previously by a different com-

positor. We have the general knowledge that copy could be cast off and printed by formes. We have the specific knowledge that this method of printing was the invariable method used to print the First Folio. If the same compositor had set both pages, some doubt might arise whether—despite the cast-off copy—he would not have tried so to adjust his future pages that the extra lines could have been absorbed. But, instead, it can be proved that the very circumstances obtained that would make this absorption impossible: the division of the copy at pages 6 and 7 between Compositors A and B. It is obvious that what we lack here is the specific evidence from the book itself on this page, although our information about the printing of this page is more detailed than usual. No power on earth, therefore, can demonstrate that the agent who cast off the Folio copy here had miscalculated to the extent of eighteen lines. Nevertheless, if the success of an explanation may be judged by whether it does or does not satisfy, I should be content to rest the case on this bibliographical possibility rather than on the quite speculative historical if's and and's about the hypothetical reactions of a Jacobean audience and of King James. This is only to say that a plausible mechanical hypothesis in matters involving the transmission of a text ought usually to be preferred to a plausible (or, in this case, implausible) literary or historical conjecture.

This is a situation that we are likely to find occurring more and more as analytical bibliography extends its methods into the problems of textual criticism. In some cases the new techniques, when properly evaluated, will produce results attainable by no other form of scholarship. Several generations of critics have haggled on inconclusive evidence over whether, for instance, the Folio *Hamlet* was set from an independent manuscript or from an annotated copy of the Second Quarto. Until this problem is satisfactorily answered, the very foundation for a scholarly edition of the play is lacking. I have some definite expectations that the new bibliographical technique of applying compositor analysis to transmission problems will eventually lead to the

solution and thus enable editors to tackle the numerous individual textual problems on the basis of a sound hypothesis about the exact relationship of the two substantive texts, Quarto and Folio.

Textual criticism dealing with the transmission of texts has heretofore used only readings as evidence, but these are subject to value judgments. Much of the confusion that has previously attended the discussion of the source of the Folio *Richard III* text has come from the exclusive use of readings as evidence. Thus one finds that at IV.iv.536 the first five editions of *Richard III* read

Is colder tidings, yet they must be told,

but the sixth edition reads

Is colder news, yet they must be told,

and the Folio agrees with Q6 in the substitution of *news* for *tidings* and reads

Is colder news, but yet they must be told.

If readings alone must be evaluated, the critic is faced with estimating the odds whether (1) this is a quite fortuitous similar corruption made independently by two compositors, (2) whether the similarity of error means that the Folio was directly dependent upon Q6 at this point, or (3) whether no connection exists between the two editions, but the Folio alteration was the result of an authoritative annotation made by the scribe fixing up Q3 for the printer and the earlier Q6 similar reading was merely a chance compositorial corruption that happened to correspond to the true reading. Whatever conclusion he comes to here will be controverted by just the same sort of evidence from other readings in these texts pointing in the opposite direction. But once the bibliographical way is established in textual studies and the analysis of compositorial characteristics of spelling, capitalization, punctuation completed for the specific workmen who set the Shakespeare texts, it will be a blessed relief to escape from value judgments that carry little conviction in such matters and to arrive at

the truth by impersonal evidence of a mechanical nature that does not lean so much weight on the unpredictable human element, as at present.

The conclusion we may come to, then, is a hopeful one. The bibliographical way is not only a technical manner of dealing with complex material. It is a logical method of analysis that inevitably prefers physical facts to immaterial speculations, but it is quite prepared to combine with any other form of criticism in whatever proportion is needed to solve the problem at hand. If a value judgment is the last brick needed to support the arch, the bibliographer is I daresay prepared to make, or to accept, this value judgment provided it fits into the one place left vacant by bibliographical logic. Once analytical bibliography began to extend its usefulness beyond the confines of the library and to take an active part in the affairs of academic scholarship, especially in this country, it broadened and developed its methods and vastly extended the material on which it could properly operate. This increased influence is not to be measured alone in terms of the positive results that have accrued in new fields by the application of bibliographical analysis to material not previously thought to be subject to such examination. The influence has been, and will continue to be, more widespread. Once a scholar has tasted the intellectual pleasures of a bibliographical solution to a given problem, some part of bibliography's rigorous treatment of evidence is inclined to rub off on him. Under the pressure for novelty, much academic literary criticism is growing increasingly slipshod in its logical method, especially when it comes to deal with the interpretation of evidence. It may be that the discipline that will return us to an intellectually bracing critical vigor will come from the extension of the kind of reasoning mind that is now sweeping clean the bibliographical way.