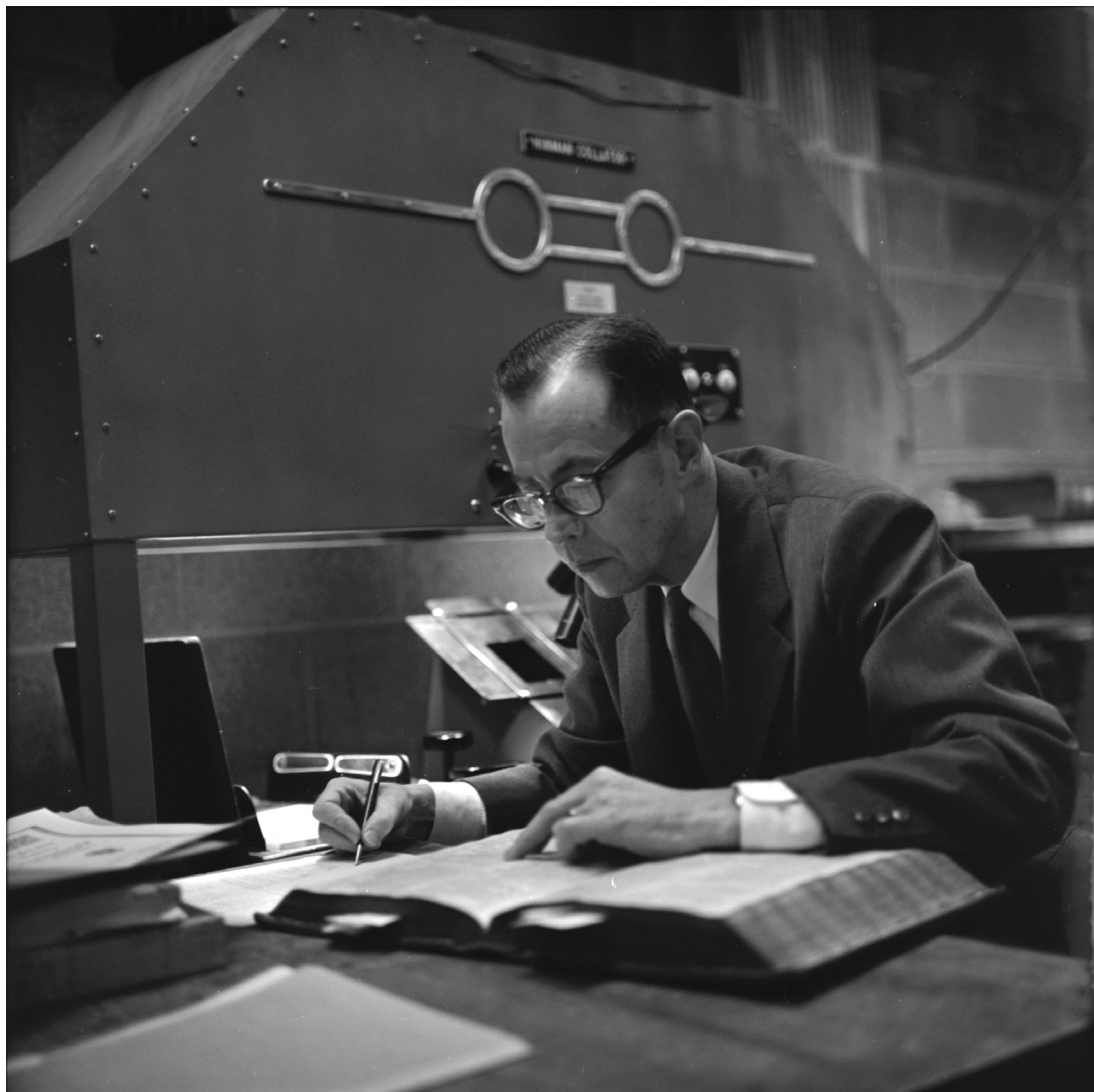


Script & Print

Bulletin of the Bibliographical Society
of Australia & New Zealand



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Front cover image: Charlton Hinman (1911–1977) at work at his desk next to the Hinman Collator in the Spencer Library at the University of Kansas, where he taught from 1960 until his retirement in 1976 (undated; image courtesy of University of Kansas Archives Photographs, <https://digital.lib.ku.edu/ku-uaphotos/27830>). Two other images from the same collection (27419 and 29912) show Hinman collating the opening of *Much Ado About Nothing* in an original copy of the Folio with a photocopy of the corresponding page in the quarto of 1600. That play happens to be the first one in which Hinman thought a page was typeset from case z.

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Some Thoughts on Analytical Bibliography in the Twenty-First-Century Academy

It is an honour to be able to publish the essay that dominates this issue of *Script & Print*. The cover image of Charlton Hinman collating text from the First Folio, seated beside the daunting optical collator he had designed, reminds us both of the intense dedication involved in his work and of the fact that he deserves to be remembered as a pioneer in the use of technology to advance literary scholarship.

Hinman published his magnum opus, *The Printing and Proof-Reading of the First Folio of Shakespeare* in 1963. Fifty-eight years later we are still learning even more about both Hinman's work and the First Folio itself. Or rather, we learn that we know less than we thought. As we approach the 400th anniversary of the First Folio in 2023, we can anticipate a renewed focus on the most renowned printed book in English.

I know some readers' eyes will glaze over at the tables and occasionally rather complicated signature references throughout this essay. To those readers, all I can say is that the summary here of the vast amount of data Pervez Rizvi has created is far more easily understood than the extensive data in Hinman's two volumes. As Rizvi acknowledges, Hinman's book astounded bibliographers and editors alike by documenting just how many states and variants he located in his collations of over fifty copies of Shakespeare's First Folio. Hinman's work is not a text to be trifled with and Rizvi's extensive analysis demonstrates appropriate respect and responds with appropriately detailed evidence.

We now have other options to collate books—either with mirrors to draw our visual attention to variations or with computer overlays—that have become increasingly accurate at identifying such areas of disagreement. On the other hand, almost none of the 59 Hinman collators known to have been constructed remain in operation, in part because they were somewhat complicated mechanically, but mostly because they took up a lot of room in libraries and were rarely used.¹

In his brilliant essay Rizvi demonstrates that we cannot rely on assumptions of regularity in the printing house, adding a second conclusive demonstration to Don McKenzie's work on the Cambridge University Press printing house practices. The field of bibliography thus simultaneously finds itself with a host of discredited past studies and opportunities for further work with collation tools that are certainly no harder to use than Hinman's own and should increasingly become much more efficient. Computers have enabled Rivzi to carry out statistical tests and data comparisons impossible for Hinman, who relied on file card indexes.

¹ Steven Escar Smith, "Armadillos of Invention: A Census of Mechanical Collators," *Studies in Bibliography* 55 (2002): 133–70.

So are we poised to launch a new era of analytical bibliography focused on typography and press practices or are we left in despair that no such analysis can be credibly conducted? Rizvi and McKenzie have alerted us to the assumptions which we must abandon, but might there be printing house records that would allow us to track the production process at the level of the individual pieces of type? Between Hinman and Rizvi's work, scholars have probably exhausted the typographical information to be derived from the First Folio, but few other books have been subjected to such meticulous scrutiny. One important point to emerge from Rizvi's work is that any data sets need to be sufficiently large to make such minor variations frequent enough to yield a pattern within the limits of a unit of printing (whether that unit be the page, the forme or the sheet). Double-column folios are probably the best sources of data for such work, though they are also the most exhausting to collate. The best studies of this sort would also incorporate a second set of data to act as a control to ensure that the patterns identified are specific to the author or work being studied. In the current academic climate I doubt many scholars would be able to commit to the time and intense attention such work demands. There is no question that we can learn more from these methods of close examination, as Peter Blayney has demonstrated in his study of the books printed by Nicholas Okes. But those able to devote their time and immense effort to such study remain few and far between. We should therefore be all the more grateful to those who achieve such dedication.

I urge you to devote the time to understand and appreciate the impressive analysis on display in this issue, and to recognise that bibliography is a demanding field, but one which, when pursued assiduously, continues to produce scholarship fundamental to all textual and literary study.

Typeset Attributions for the Shakespeare First Folio

PERVEZ RIZVI

The First Folio is the most important secular book in the English language. In *The Printing and Proof-Reading of the First Folio of Shakespeare* Charlton Hinman gave us an abundant amount of several new kinds of knowledge about it.¹ He proved beyond doubt that the Folio was set by formes, not seriatim. He collated more than fifty copies of the book, using a collating machine he invented, and found more than five hundred press variants.² He deduced the order in which the formes were typeset, which is not the order in which we find them in bound copies of the book. A brilliant instance of this was his reconstruction of the aborted and resumed printing of *Romeo and Juliet* and *Troilus and Cressida*, a problem which was known about from a few surviving copies but never before explained in bibliographical terms. He identified five different patterns of typesetting and designated them Compositors A to E. He deduced that two typesets, which he designated x and y, were in use throughout the printing of the book, and a third typeset, z, in use for a few quires in the Comedies.³ He made those deductions by an unprecedentedly deep analysis of the centre rules, running titles, ornaments, and spellings found in the Folio; and, above all, by the technique of type-recurrence analysis, which he invented.

In an earlier essay I considered the Folio compositor attributions.⁴ I concluded that the revisions to Hinman's attributions by scholars who came after him are unreliable and should be discounted. I questioned how much reliance we can have on Hinman's own compositor attributions. For that essay I took as my premise the correctness of the analysis by which he deduced the order in which the formes were typeset, and the correctness of the typeset attributions. Hinman had made his compositor attributions, based largely on counts of spellings, at the end of volume I and devoted almost all of volume II to the type-recurrence analysis which is the foundation of the typeset attributions. With hindsight, it is clear to me that I underestimated the extent to which the compositor attributions and typeset attributions are dependent on each other. Hinman referred to "case identification, that almost indispensable tool of compositor identification..."⁵ It might have been

¹ Charlton Hinman, *The Printing and Proof-Reading of the First Folio of Shakespeare*, 2 vols. (Oxford: Clarendon Press, 1963). References throughout this essay to volumes I and II are to this book.

² I, 230, 265, 280, and 323.

³ As Hinman explains at I, 108, the term 'case' means the four physical cases that held roman and italic types for what we now call uppercase and lowercase letters, as well as punctuation. Like him, I use the terms 'case' and 'typeset' interchangeably.

⁴ Pervez Rizvi, "The Use of Spellings for Compositor Attribution in the First Folio," *Papers of the Bibliographical Society of America* 110, no. 1 (2016): 1–54.

⁵ II, 23.

more exact for him to refer to them the other way around, since he repeatedly uses compositor attributions to justify the typecase attributions in volume II. Be that as it may, for this essay I have made the study of the typecase attributions that I should have made last time.

It is my purpose to show that Hinman's typecase attributions are, like the compositor attributions, doubtful, being affected in material but unquantifiable ways by two problems: the possibility of concurrent printing, which he understated, and his axiomatic position that Compositors A and B each had their own typecase, x and y respectively.⁶ By making new interpretations of the type-recurrence evidence, I shall show that different typecase attributions are available to us, which explain that evidence at least as satisfactorily, if not more so. In particular, I shall show that Hinman's belief in the presence of typecase z was not justified by the evidence.

As the premise to my work in this essay, I shall take the type-recurrence evidence Hinman gave us as correct.⁷ His description of the process by which he collected it testifies to the great care he took.⁸ The few samples I checked, in the Norton facsimile,⁹ were of course correct and, if the evidence is ever to be fully checked, the task will require a year or more of work in examining original copies of the Folio at the Folger library. I shall also take as correct Hinman's deductions of the order in which the formes were typeset.¹⁰ Those deductions were partly based on his analysis of skeleton formes.¹¹ To that extent they are now vulnerable to the cogent objections that D. F. McKenzie's classic essay "Printers of the Mind" made to the use of that evidence.¹² Nevertheless, they are confirmed by the extensive

⁶ Concurrent printing, also referred to as concurrent production, happens when a printer starts a new job before he has completed an earlier one. This means, for example, that sometimes his staff may work on two books at the same time; or they may alternate between the books in regular or irregular ways. Our reconstruction of the printing of one book may then not be accurate if we do not take into account evidence from the other book, something which is possible only if both have survived.

⁷ I have taken as definitive the listing of the evidence in Appendix B of Hinman's book. See my spreadsheet "Hinman-type-recurrences.xlsx" for a listing of that evidence in tabular form. Inevitably, there are a few discrepancies between Appendix B and the graphs in volume II. For example, on II, 322, he shows aaa5^v as having 7 types in common with each of bbb4^r and bbb2^r. In fact, they are not the same 7 types, only 6 being common to all three pages. There is a minor omission in Appendix B, where Hinman omits to tell us the column in which type I22 is found on P3^v. It is column a.

⁸ I, 66–69.

⁹ *The First Folio of Shakespeare: The Norton Facsimile*, 2nd edition, edited by Peter W. M. Blayney (New York: Norton, 1996).

¹⁰ My webpage www.shakespearetext.com/hinman.htm provides a graphical, downloadable view of the printing order, as well as a supplementary index which may help readers to locate Hinman's discussions of individual quires in volume II.

¹¹ I, 158–68.

¹² D. F. McKenzie, "Printers of the Mind: Some Notes on Bibliographical Theories and Printing-House Practices," *Studies in Bibliography* 22 (1969): 1–75. McKenzie showed that, at least for some books printed by Cambridge University Press in the late seventeenth century, deductions about the printing order of formes, made by grouping the skeleton formes as Hinman did, are contradicted

type-recurrence evidence, which is independent of skeleton formes. I shall take them as given, although the question of order may need to be revisited someday. I shall treat all compositor attributions as doubtful and take no account of them in my analysis.

Folio Facts

To provide context, it is useful to say something first about how many material objects we are dealing with when we talk about pages and types.

As in my earlier work, I shall treat as a 'page' any physical page in that book on which there is a line to which Hinman assigned a Through Line Number (TLN) in the Norton facsimile. This excludes the preliminaries and the handful of blank pages between plays. It includes mixed pages, such as the last page of *The Tempest*, which contain both dialogue, to which he assigned TLNs, and dramatis personae, to which he did not. It excludes the only pages in the book that have only a dramatis personae, for 2 *Henry IV* and *Timon of Athens*. By this definition, the Folio has 883 pages.¹³

Hinman assigned almost all Folio pages to a single compositor each. However, he divided a few pages into what I shall call 'page units', to enable him to assign different units on a page to different compositors. In most such instances the page is divided into two units, one per Folio column. But, in one remarkable instance among others, he divided xgg2^v into four units, with column a forming one unit and column b split into three units, so that Compositors A and B could alternate the typesetting of them.¹⁴ In my view he succumbed to the temptation to overfit the data to his theory about the spelling preferences of the compositors. Be that as it may, it is not my purpose in this essay to challenge any of his compositor attributions. I have therefore adopted his division of the Folio pages into page units, although he did not use the latter term. Of the 883 pages, 863 consist of just one page unit. The remaining 20 pages are divided into 49 units, to allow columns or parts of columns to be differently attributed.¹⁵ This gives us a total of 912 page units in the Folio.

When Hinman presented his typecase attributions in volume II, once again he subdivided some pages to allow himself to say either that they were typeset from

by the press accounts, which have fortunately survived.

¹³ As can be easily seen in the Norton facsimile, the page numbers printed on the Folio pages contain many errors and cannot be relied on for any purpose.

¹⁴ Hinman's book is not the definitive source of his compositor attributions for xgg2^v because he changed his mind about them. See his introduction in the Norton facsimile, as reprinted in the 2nd edition, xviii.

¹⁵ The 20 pages can be most conveniently identified from the synopsis given at II, 514–18. For a few pages, such as P5^v, Hinman made his division in the middle of a Folio column. In such cases I have made a further division at the end of column a. This ensures that throughout the Folio each page unit in my work is wholly contained in one column.

more than one typecase, or that they were distributed into more than one typecase, or both. Inevitably, the division of a page for typecase attribution and the division for what we may call distribution attribution are not always the same. For example, for typesetting he divided P6^v into two, corresponding to columns a and b, with column a set from case y and column b set from case x.¹⁶ For distribution he again divided that page into two but in a different way, with column a and the top half of column b distributed into case y, and the bottom half of column b distributed into case z. He thus implicated all three typecases in the same page, in different ways for setting and distribution. The probability cannot be high that such an intricate reconstruction is really what happened under a London roof four hundred years ago. It is clearly an example of overfitting, which seems to occur most often in just those quires in the Comedies where he detected the presence of case z.

Hinman declared that he had used 615 distinctive types in his type-recurrence analysis and had counted 13,075 appearances of them in the Folio.¹⁷ As he acknowledges in the appendix in which he lists those appearances, some are doubtful, and he did not rely on them. As we should expect, he must have changed his mind about whether it was safe to use some types as evidence, and it should not trouble us that his totals are not entirely accurate. Having transcribed the evidence from his book into my laptop for analysis, I found 13,121 appearances by 619 distinctive types.¹⁸

I shall refer in this essay to ‘typecase attribution’ and I intend that term to cover both the attribution of the typecase from which a page unit was set and the one into which it was distributed, as the context requires. I have not created a separate division of pages into page units for typecase attribution, because such a division would neither contain nor be contained in the division of page units for compositor attribution purposes and would therefore be liable to confuse. Instead, I have subdivided the existing page units where necessary for typecase attribution. For example, for P6^v, as mentioned above, Hinman needed only to divide the page

¹⁶ Throughout this essay I have silently added a superscript ‘r’ to every signature of a recto page in quotations; for example, I have written A1^r whereas Hinman consistently writes A1.

¹⁷ I, 461.

¹⁸ A reader who consults Appendix B of Hinman’s book or my spreadsheet version of it may be helped by the following notes. The pages are listed in that Appendix in the order in which they are read, not the order in which they were typeset. Three short Folio pages are not listed at all because he found no distinctive types on them. These are χ gg8^r (the epilogue to *2 Henry IV*), χ 1^r (the prologue to *Troilus and Cressida*), and vv6^r (the last page of *Othello*). Two pages are listed, because they have distinctive types, but are not found in most copies of the Folio. These are *gg3^r and *gg3^v, respectively the last page of *Romeo and Juliet* and the first page of *Troilus* from their original aborted printing. Both pages were typeset again and reprinted, as Gg1^r and χ 1^v respectively, the reprints being found in most copies of the Folio and in their proper places in the Norton facsimile. The facsimile also shows us *gg3^r and *gg3^v, at pp. 916 and 918. Finally, three pages listed in Hinman’s Appendix B, *gg2^v (the penultimate page of *Romeo*), *gg4^r, and *gg4^v (the second and third pages of dialogue in *Troilus*), are also referred to in other parts of his book by the aliases gg2^v, χ 2^r and χ 2^v respectively.

into two units for compositor attribution, so that he could assign each column to a different compositor; but he decided that column b was distributed partly into case y and partly into case z. I have accommodated that complication by qualifying my mention of column b on that page with the lines from it that were distributed into each case.

It has been my aim that a reader should be able to check my work. I have provided several spreadsheets of data, with an accompanying guide, downloadable as a Zip file: www.shakespearetext.com/type-recurrence.zip. One spreadsheet documents Hinman’s attributions; where I revise a set of his attributions below, I provide a corresponding revised version of the spreadsheet. This essay is self-contained, and it is not essential for a reader to look at the spreadsheets. Nevertheless, each one is important for several reasons:

- It allows the reader to view the graphs in Hinman’s volume II in more detail, listing the types shared by any chosen set of pages, the typecases they are set from and distributed into, and any anomalies.
- It highlights any departure from his attributions.
- It provides a more detailed version of his Appendix B, showing the journey of each type through the Folio, the typecases it goes in and out of, and where it is anomalous.

Anomalies

Hinman discusses what he calls his “distribution facts” in detail in successive chapters of volume II of his book.¹⁹ That is, he tells us which typecase a page was set from and which one it was distributed into and by which compositor.²⁰ It is unfortunate that he used the term ‘facts’ to describe what should properly be called inferences. A compositor attribution is an inference. In the Comedies, it is often a weak inference, given that several type-A compositors—that is, Compositors A, C, and D—are supposed to have worked on that section. A typecase attribution is a stronger inference, being based on the presence of distinctive marks of ink on the pages of the Folio. But to say that Compositor A or B or any other compositor

¹⁹ For the explicit use of the term, see the index and II, 12. See my spreadsheet “Hinman-distribution-facts.xlsx” for the full list of these facts.

²⁰ I have noted two discrepancies on II, 399. That page tells us that Compositor A set “most” of K3^v, whereas I, 410–11 tells us that he set all of it. I have taken the latter to be correct. On the next line, II, 399 tells us that I5^v was distributed into case y, but Hinman must have meant case x, otherwise there would be several anomalies on the successor page K3^r, none of which he declares. I spotted one error by him only after my work was complete. At I, 453, he states that type st23 is found on “ff2a^v45”. This is a typographical error for “ff2^va45”, as his graph at II, 220, confirms; but it misled me into treating the type as being found on the recto rather than the verso page. As both pages were set from and distributed into the same typecase and have no anomalies, the error is of no consequence.

distributed any part of any page is nothing more than speculation. The distribution of a type into a typecase leaves behind no evidence whatever of the identity of the distributor. I regard this objection of mine as more than a quibble. When we state a distribution fact in the form Hinman invariably uses in volume II—for example, “Ax distributed i4^r and set i2^r”—we are associating a weak inference, the compositor attribution for the successor page, with an inference based on no evidence at all, the identity of the distributor of the predecessor page. Hinman makes hundreds of such statements in volume II, and I believe they have a subliminal effect, of weaving two separate physical acts, the distribution of one page and, at an unknown later time, the typesetting of another page, into one story.

At the end of each chapter of type-recurrence analysis in volume II, Hinman declares the number of anomalies found in the Folio pages discussed in that chapter. In principle an anomaly is when a type is distributed into one typecase but is next seen on a page set from a different typecase. However, Hinman makes many exceptions to this rule, to reduce the number of anomalies he counts, sometimes to nil. The most remarkable instance of this comes in quire R where, arriving at the end of his case z attributions, he avoids what would otherwise be some anomalies in the next quire by the *deus ex machina* of supposing, first, that Compositor C “liquidated” case z into case x and, second, that Compositor B, “knowing that he would for some time be working on the Folio alone”, liquidated the case “x-cum-z” into case y.²¹ It hardly needs pointing out that the evidence cannot allow us with any confidence to make such an intricate reconstruction, including an inference about a compositor’s knowledge of Jaggard’s future staffing plans. In another example, Hinman tells us: “Which types, if any, appear anomalously in the last four formes of quire V it is impossible to say.”²² His reason there was that he had inferred some concurrent printing of non-Folio material at around this time, and, as he recognized, that introduces considerable uncertainty in our reconstruction of which types were in which cases.

By my count, Hinman declared 167 anomalies for the whole Folio, a tiny proportion of the 13,000 or so type occurrences he had found. He disclaimed any intention to count all anomalies.²³ In making my own counts of anomalies, I have abstained from any special pleading about liquidation, concurrent printing, or anything else. I have used a purely mechanical definition of an anomaly: if a type had been distributed into one typecase but next appears on a page set from a different typecase then I have counted it as an anomaly on that page, even if we could excuse it in some way. That difference between us is why Hinman declares only 2 anomalies for quire V whereas by my definition there are 14. It should not be thought that by applying my mechanical definition I have inflated the total

²¹ II, 443.

²² II, 461.

²³ I, 129.

of Hinman’s anomalies to make my experimental changes to his reconstruction, given later in this essay, look better by comparison, because of course I have used the same definition to count the anomalies arising from my changes. In just a few cases my mechanical approach has the effect of decreasing the number of anomalies. For example, Hinman declares Y21 and iP21²⁴ to be anomalies on K5^v because, he tells us, it is a case z page whereas the types had been “long ago distributed into case y”.²⁵ However, that statement may either be an error or indicate a change of mind, because, although he had noted that the types last appeared in columns b of H2^v and G2^v respectively, he had not deduced which cases those columns had been distributed into. Accordingly, those types are not counted as anomalous on K5^v by my definition.

An anomalous type raises the possibility that we are wrong either in our typecase attribution for the page on which it is anomalous or in our attribution of the typecase it was distributed into after its previous appearance. If we find more than a handful of anomalies on one page then the possibility becomes almost a certainty. However, as Hinman correctly recognized, we should be wary of dismissing our attributions just because of a few anomalies. In a shop in which tiny pieces of metal are constantly in motion, it is inevitable that a few will be dropped on the floor, picked up later, and be put back into a different typecase than the one they came from. Hinman regularly considered a particular form of this phenomenon, the so-called ‘stripping accident’, but no doubt many other kinds of accidents also happened regularly. An anomaly is a cause for second thoughts, but not a show-stopper.

By my formal definition, the number of anomalies in Hinman’s typecase attributions for the whole Folio is 238, only slightly more than an average of one anomaly per sheet. This remarkably impressive number is important because I shall use it as a benchmark by which to judge the satisfactoriness of the experimental changes I suggest below.

Concurrent Printing

McKenzie alerted us to the danger of making nice calculations about the order and rate at which the typesetting and impression of the formes of a book took place, for two reasons, one historical and one commercial.²⁶ The historical reason was that modern scholars who make such calculations bring to the task our post-industrial

²⁴ To avoid technical problems when type-recurrence data is transmitted electronically by files, I have represented italic types as roman but prefixed by ‘i’. For example, iP21 means the type Hinman called P21. For the same reason I have replaced ¶ by ‘pilcrow-’ and χ by ‘chi-’ in the accompanying spreadsheets.

²⁵ II, 403.

²⁶ McKenzie, 10.

revolution notions of how people work. The commercial reason was concurrent printing, which is what I shall consider in this section.

Concurrent printing can completely disrupt our understanding of how the printing of a book proceeded, while sometimes leaving no trace of itself. To be fair, Hinman was fully aware of this possibility but, as I shall argue, he did not make enough allowance for the uncertainty it creates. Remarkably, we have an example of a concurrently printed document of which just one copy is known to exist and, moreover, that sole surviving copy was discovered and drawn to his attention while he was writing his book. This is the so-called Visitation Summons, a form printed by Jaggard around August 1623 and designed to be served on “Knights, Esquires, and Gentlemen,” with blank spaces left for their names to be filled in by hand, requiring them to present themselves at an appointed time with evidence of their arms and crests.²⁷ This is just the kind of ephemeral document that must have been often printed but rarely preserved after use. Jaggard was the City printer. He also had a monopoly of printing playbills, none of which have survived but many must have been printed in the eighteen months or so that it took to print the Folio. Any publication, whether a major book or a throwaway pamphlet, could have been printed concurrently with a Folio play, disrupting our type-recurrence analysis in unmeasurable ways. Hinman recognized the importance of the Visitation Summons for his type-recurrence analysis and succeeded in identifying the Folio quire with which it was concurrently printed.²⁸ As I am about to show, he did not recognize that concurrent printing might have been taking place even where he did not see a need to invoke it. He can hardly be blamed for that blind spot. His reconstruction of the printing makes excellent sense and there is a natural tendency for us all to think that if our hypothesis fits the evidence very well then it must be true. Part of my point in this essay is to challenge that thinking for the Folio by providing alternative reconstructions that make equal if not more sense.

Let us observe first that typecase attributions are progressive: they cascade from one page to the next. To see this, suppose that two nearby pages share a set of distinctive types, typically a dozen or so in the Folio. If we say that both pages were set from case x then we are obliged to say that the earlier page must have been distributed into case x, so that its types could be picked for the setting of the later page. If we do not make that attribution, then we create an unacceptable number of anomalies. Conversely, suppose we say that the earlier page was set from case x but the later one from case y. That also forces our hand, for then we must say that the earlier page must have been distributed into case y, again to avoid an unacceptable number of anomalies. Alternatively, if we have already attributed the typecase that the earlier page was distributed into, then that forces our hand when we come to decide which case the later page was set from. These effects continue

²⁷ I, 24–25.

²⁸ II, 321.

as we progress through the next few pages, although they become less significant as the influence of type recurrences from other nearby pages comes into play.

Typecase attribution errors are also progressive: one error can cause several others in attributions for subsequent pages, until good luck leads us to make a compensating error and get back on the right path. This also means that typecase attributions are much less amenable to analysis in isolation than, say, compositor attributions. Compositor attribution for a page can be done by examining the spellings or psycho-mechanical evidence for that page alone. Indeed, when scholars after Hinman modified some of his compositor attributions, they were able to do so without having to revisit the attributions for all nearby pages. With typecase attributions we do not have that luxury. Because they are progressive, typecase attributions form a network. Distinctive types from one page may appear next on several different pages, while each of those pages may take their distinctive types from several other pages too. With more than 13,000 occurrences of more than 600 distinctive types in the book, Hinman’s distillation of the evidence into the distribution facts was a masterly achievement. Given this complex network, if we choose to change one of his typecase attributions then we must follow the trail of the distinctive types through the preceding and succeeding pages, reconsidering and if necessary changing their attributions in consequence of our first change. If we do not do this, the attributions may no longer cohere.

To apply these remarks to concurrent printing in theory first, before moving on to my experiment, suppose that we have made what we regard as a successful reconstruction of the printing of a part of the Folio. We have a chain of distribution facts that makes sense, with pages being set from one typecase, being distributed back into that typecase or another typecase after impression, and the types thus becoming available for setting on the next page. We therefore ‘know’ a set of distinctive types that are currently in case x and another set in case y. I shall call these Set 1 and Set 2 respectively. Typically, such a set might have a dozen or so types in it, these having been last seen on nearby predecessor pages. Suppose now that the concurrent printing of another document takes place. Cases x and y are used to set different pages of that document, and the types in Sets 1 and 2 get picked for those pages, since they are at the top of their respective boxes. When the case x page has been printed, the types need to be distributed. If at that point case x is in use, but case y is not, the compositor might distribute the page into case y. When the other page has been printed, it might be distributed into case x, to avoid one case becoming too full while another is running short. These pages are what Hinman calls ‘non-Folio matter’ and let us suppose that they have not survived. If cases x and y are now used to set one page each in the next Folio forme, then our typecase attributions will both be wrong. Since the Set 1 types were last known to be in case x, when they turn up together on the new Folio page, we will naturally diagnose it as a page set from case x; similarly,

we will diagnose the page on which the Set 2 types now appear as set from case y. What is worse, this error may then cascade through the next few Folio formes, because the types are no longer where we think they are, until some compensating event, such as more concurrent printing, puts us back on the right path. Any reader who browses through Hinman's reconstruction in volume II will see that this problem could have occurred almost anywhere, and we would be none the wiser. More specifically, consider the many places where he infers that a page was set from case x but distributed into case y, or vice versa. As I have just shown by my hypothetical example, those inferences could all be wrong, if concurrent printing was taking place. By my count he made such an inference for more than two hundred Folio pages. Even if only some of those inferences are wrong, their cascading effect will cause many others to be wrong too. We simply cannot calculate just how wrong Hinman's reconstruction might be because most of the evidence of concurrent printing has perished.

Consider another example, theoretical but realistic. Suppose we have made a reconstruction of part of the Folio as above, and we think we know a set of distinctive types currently in case x. A Folio page is set from that case and half the types in the set get picked for it. While that page is being printed, a page of non-Folio material is set from the same case, and the other half of the types get picked for that material. When the Folio page has been printed, its types are distributed back into case x. However, the types from the non-Folio material are distributed into case y, perhaps because case x was in use when that page came back from the press. If the next Folio forme is set, as usual, from both typecases then we will wrongly think that it was set just from case x, because that is where all the distinctive types were last known to be. We have thus wrongly diagnosed a forme as having been set from one case, when it was set from both. Scenarios like these are likely to be rarer, but it is quite possible that they did occur from time to time. We may even have an example. Hinman found that types from column a of bb1^v, a page in *Coriolanus*, reappeared immediately in the intercalary setting of *Romeo and Juliet* but types from column b did not reappear until later, in *Macbeth*. He resolved this minor oddity by supposing that column b was left undistributed for a while after column a was distributed.²⁹ As my example has just shown, it is also possible that the whole page was distributed at one time, as we should expect, and the column b types that 'went missing' for a while were being used for non-Folio printing.

There is one sense in which concurrent printing has been staring us in the face in the pages of the Folio itself, and we have just not regarded it as such. Consider *The Winter's Tale*, which was apparently printed late and out of sequence.³⁰ Unlike

²⁹ II, 193.

³⁰ Malone reported in the eighteenth century that the now-lost office book of Sir Henry Herbert, Master of the Revels, contained an entry for the relicensing of *The Winter's Tale* on 19 August 1623

almost every other play in the book, its sheets form a self-contained booklet of three quires. Moreover, they have a set of signatures, Aa to Cc, which are not part of any sequence of signatures found elsewhere in the book. Had its temporarily missing manuscript not been delivered to Jaggard in time, the Folio would have been printed and sold without it, just as some early copies were sold without *Troilus and Cressida*. Provided that, like *Troilus*, the printers omitted to list it on the contents page, no reader would have known that *The Winter's Tale* was missing. The play ends on a page numbered 303, but its omission would not have disrupted the page numbering because the next play in the book, *King John*, begins on a page numbered 1, the page numbering having been restarted for the Histories. The exclusion of *The Winter's Tale* would have left no trace on the rest of the book. As an experiment we may therefore treat that play as if it were a separate playbook being concurrently printed with the Folio. Indeed, if we think about the material objects—such as the paper, the types, and the skeletons—and the operations that the printers performed on those objects, then what actually happened is indistinguishable from what would have happened had *The Winter's Tale* really been a separate, concurrently printed book. The purpose of the experiment is to see how concurrent printing can disrupt our typecase attributions.

To do the experiment I took Hinman's set of 13,121 type recurrences and removed from it the 525 ones found in *The Winter's Tale*. For example, consider type r21, which is one of the most frequently seen distinctive types in the Folio, being found on 90 pages. It appears on page c6^v in *Richard II*, in the forme that according to Hinman was printed just before the printing of *The Winter's Tale* started. It then appears five times in *The Winter's Tale*, and is then next seen on page h3^v in *Henry V*. After removing the appearances in *The Winter's Tale* from the list, the type-recurrence evidence tells us that the type was seen in *Richard II* and then next seen in *Henry V*. The point to note first is that if *The Winter's Tale* really had been published as a separate playbook, then there would have been nothing in this reduced type-recurrence evidence that would have aroused suspicion, as it is only natural that a type found in *Richard II* would be found next in the history play that was printed next, *Henry V*.³¹ The same is true for the other types found in *The Winter's Tale*. Their absence would not cause us to suspect concurrent printing or any other disruptive activity. If anything, it would reduce the evidence

because the "allowed booke" was "missinge" (W. W. Greg, *The Shakespeare First Folio* (Oxford: Clarendon Press, 1955), 415). It is intriguing that this is the same play that was apparently not available to Jaggard when needed, obliging him to begin work on the Histories section before going back to finish the Comedies. According to Hinman, *The Winter's Tale* had been printed by the end of 1622 (II, 522). Perhaps its manuscript was noted as missing when it was needed for the Folio, was then found or reconstructed for Jaggard, but not relicensed until it was decided to perform the play.

³¹ According to Hinman's reconstruction the printing of the two *Henry IV* plays was started after *Henry V* had already been printed.

of disruption, because we would no longer have the puzzle of why the last comedy was not printed until after the histories were already in progress.

Taking away *The Winter's Tale* but leaving Hinman's typecase attributions for the other plays unchanged instantly creates a problem. The number of anomalies jumps from 238 to 274.³² That by itself might not arouse suspicion, because 274 only looks high by comparison with 238, but it is still a tiny proportion of the total type-recurrences. The evidence of the problem is visible when we look at where the biggest anomalies are. For page h3^v, which has the r21 type I mentioned above, the number of anomalies is now 4, whereas we had none before we removed *The Winter's Tale*. In Hinman's reconstruction, after its appearance on c6^v the type had been distributed into case x. It then appeared on page Aa3^r in *The Winter's Tale*, was distributed back into case x, then appeared on Aa5^v but that time it was distributed into case y. In its other three appearances in *The Winter's Tale* it was set from and distributed into case y. Therefore, its appearance on h3^v, which according to Hinman was set from case y, is not anomalous. But take away *The Winter's Tale* and we see that the type was distributed into case x after its appearance on c6^v and next found on h3^v, which was set from case y. That is how the anomaly arises and it is such instances that explain the rise in the total number of anomalies from 238 to 274.

The reader might think that I have just shown the opposite of what I set out to show. I tested the effect of concurrent printing by treating one self-contained set of quires as a separate playbook, and found that, without those quires, the typecase attributions look wrong and therefore enable us to suspect concurrent printing by inferring that some significant evidence has been lost. However, my point of course is that Hinman's typecase attributions are what they are because he made them for the Folio as it is. Had it really come down to us without *The Winter's Tale*, he might have made different attributions and avoided the anomalies that aroused suspicion in my experiment. Is it obvious that he would have been able to do that? Could it be that without the type-recurrences from that play, the evidence would have been intractable, and he would have found a suspicious number of anomalies in this part of the Folio, no matter what typecase attributions he tried? I can provide an immediate answer to this question.

I took the Folio without *The Winter's Tale* and I revised a handful of Hinman's attributions in the vicinity of that play.³³ My reattributions are given in Table 1.

³² See my spreadsheet "Folio-excluding-The-Winters-Tale-with-Hinman-attributions.xlsx".
³³ See my spreadsheet "Folio-excluding-The-Winters-Tale-with-new-attributions.xlsx".

Page Unit	Set From (Hinman)	Set From (My Change)	Distributed Into (Hinman)	Distributed Into (My Change)
c1 ^v			y	x
c1 ^r			y	x
h3 ^v	y	x		
h4 ^r (col. a)	y	x		
h3 ^r	y	x		
i3 ^v			y	x
i5 ^r	y	x		
k5 ^r (col. b)	y	x		

Table 1—My changes to Hinman's attributions near *The Winter's Tale*

Table 1 shows that for 8 pages or parts of pages I have changed the typecase from y to x, either for setting or distribution. Observe that now h3^v is set from case x and therefore the type r21 is no longer anomalous there. The total number of anomalies in this revised reconstruction is 245, compared to Hinman's 235 (excluding the 3 anomalies in *The Winter's Tale*), which is an insignificant increase.

If *The Winter's Tale* really had been published as a separate playbook, thereby qualifying as an example of a book concurrently printed with the Folio, would the typecase attributions I have made above for the reduced Folio have been made by Hinman? I believe the answer must be yes. The attributions I have made create only a few more anomalies than he found acceptable enough for his book. On the positive side, they have a simplifying effect. He had been forced to say that pages h4^r and k5^r had been set partly from case x and partly from y, to be able to make sense of the evidence. My attributions allow both pages to be set fully from case x, which is more plausible. There is nothing in this revised set of attributions which would have aroused any more suspicions than the attributions Hinman left us with.

The implication of the experiment I have just done is a serious one, for what I have shown is this. Suppose a document had been concurrently printed with the Folio. Suppose we did not possess any copies of it to include in our type-recurrence analysis, a gap in knowledge that I simulated by deliberately disregarding all evidence from *The Winter's Tale*. What I have shown is that we should still be able to interpret the evidence, without suspicion that it is incomplete, to satisfy ourselves that we have a set of attributions that make sense; that is, they are self-consistent to the extent of showing only a tolerable number of anomalies. In a jigsaw puzzle, even if just one piece is missing, we can detect its absence. Unfortunately, type-recurrence analysis is not like that.

The problem created by concurrent printing is analogous to one I discussed in my essay on Folio compositor attributions. Hinman had found that some of his compositor attributions were materially dependent on comparing Folio spellings with those in the copy text. I pointed out that, if so, then the game is up and we can stop doing compositor attribution, because for most plays in the Folio the copy text has not survived. I have now shown that concurrent printing can materially affect our typeset attributions, not just in theory but in practice for the Folio itself. This means that, since we do not know the extent of concurrent printing, an unknown number of books and other printed papers being lost, we can stop doing typeset attribution too. Nevertheless, for the other parts of this essay I shall disregard that limitation because I want to demonstrate that there are further problems with the typeset attributions.

It should be obvious that when concurrent printing takes place it disrupts not just the typeset attributions but also our deductions about the order in which the formes of each Folio quire were typeset. Consider Hinman's extended discussion in volume I of the method by which he deduces that order in volume II. He presents a "trial order" for quire o in which the inner forme o1^v–o6^r is posited as having been typeset before the corresponding outer forme.³⁴ He argues that this trial order must be wrong because, for example, that inner forme shares types with the preceding forme o2^r–o5^v, something which is not expected to happen when a book is being set by formes. However, in principle, concurrent printing makes any order possible. Here, suppose that after setting o2^r–o5^v the compositors had moved on to work on another book or pamphlet. While they were doing that, o2^r–o5^v could have been printed. It would then have been available for distribution before o1^v–o6^r was set, allowing the two formes to share types after all. I am not arguing that this is necessarily what happened. Hinman's reconstruction might be correct. I am pointing out the fatal flaw in his method for deducing the order of the formes. Be that as it may, it is my purpose in this essay to look at the typeset attributions on the premise that his order of the setting of the formes is correct; therefore, for present purposes I shall disregard that problem too. Lastly in this section, I want to note in passing that concurrent printing also invalidates any attempts to calculate how much type was in any typeset at one time. Hinman sometimes purports to make such calculations, as when he tells us that at one point in the printing of the Folio there were "seven and a half pages" of type left "standing"; that is, undistributed.³⁵ He based these calculations on the absence from some Folio pages of types which might have been expected to show up there, having been set in the immediately preceding pages. He inferred that the preceding pages must have been left undistributed. But if concurrent printing was taking place, it is also possible that the types do not turn up on the Folio pages

³⁴ I, 82.

³⁵ II, 200.

where he expected to find them because they were being used to set non-Folio matter which has not survived.

Hinman's Axiom and *Macbeth*

My essay on Folio compositor attributions was sceptical about the safety of compositor attributions in general. However, I made a concession that those attributions look convincing for *Macbeth*. Part of my reason was that in that play (as elsewhere) "there is a strong correlation between the [Compositor A] pages and case x, and between the [Compositor B] pages and case y".³⁶ I would not make that concession today. Greater familiarity with Hinman's analysis of the type-recurrence evidence, gained as part of the work for this essay, has convinced me that the compositor attributions and typeset attributions are dependent on each other to a far greater extent than I had realized, as I shall now explain.

In principle the compositor attributions and typeset attributions should be independent of each other. The former are based overwhelmingly on spellings and the latter overwhelmingly on typeset recurrences; or, rather, that is how it is in theory. There is no necessary connection between the two. A compositor is free to use his preferred spellings, if he has them, when using any typeset, because they all have stocks of the same types.³⁷ Conversely, we have no external evidence that printing houses allowed or expected compositors to behave territorially, in effect planting a flag on a typeset and treating it as theirs. Such a practice would hardly be consistent with the fact that most men in the trade were journeymen rather than apprentices or employees, nor with the irregular pre-industrial revolution patterns of work at that time that McKenzie reminded us of.³⁸ A typeset could be expected to have more than one regular user. We should therefore be reluctant to presume a correlation between the two kinds of attribution; that is, we should not presume that compositors habitually worked at just one typeset. At the very least, we should not regard it as axiomatic. Yet, I think it is fair to say that Hinman does just that. The first statement of this comes early in the book, when he writes: "Moreover, the same compositors normally worked at the same cases..."³⁹ As there is no external evidence for this claim, it could be validly made only from the internal evidence: that is if, having made compositor attributions from spellings and typeset attributions from type-recurrence, he had seen a correlation between the two. When he next addresses this question, he writes: "Certainly the combined

³⁶ Rizvi, 18.

³⁷ The instances when a compositor is forced to use a spelling because a shortage of type prevents him from using an alternative spelling can reasonably be supposed to be rare, because such a shortage is unlikely to have been tolerated for long before it was relieved by a distribution.

³⁸ At one time 75% of printers in London were journeymen. See Philip Gaskell, *A New Introduction to Bibliography* (Oxford: Clarendon Press, 1972), 176.

³⁹ I, 89.

evidence of spellings and types will provide us with a more powerful analytical tool than either of them alone.”⁴⁰ When he begins volume II, he is definitive: “It should be remarked finally here that the use of case x by Compositor A and of case y by Compositor B represents the consistent practice of these two compositors throughout the Folio.”⁴¹

The problem is that the supposed power of the combination that Hinman claimed would apply only if the two sets of attributions were independent of each other, one having been derived only from spellings and the other only from types. Unfortunately, as a browse through volume II shows, Hinman’s arguments from compositor attributions and typecase attributions are almost impossible to disentangle. He begins each section of his type-recurrence analysis by stating the typecase attributions he has made. He then provides a subsection called ‘Compositors and Cases’, in which he almost always explains how well the compositor and typecase attributions cohere with each other. What he appears to have done is to take as given the compositor attributions he stated at the end of volume I, and then looked to see how the type-recurrence evidence could be interpreted in such a way as to allow each compositor to work at the typecase that his axiom had assigned him to. He never considers if different typecase attributions might have explained the *type-recurrence* evidence more satisfactorily, independently of the compositor attributions. The reader might object that I am writing artificial, captious arguments here. Given the complexity of the evidence, a reader might think that we should be grateful that Hinman was able to arrive at just one interpretation that makes sense, and it is rather unfair to think that he had a choice of more than one. But my objection is real, not artificial. As I have shown for *The Winter’s Tale*, as I am about to show for *Macbeth*, and as I shall show much more extensively for case z later in this essay, the type-recurrence evidence can be interpreted in different, viable ways.

Table 2 shows Hinman’s attributions in quire mm, which contains only *Macbeth* pages, in the order in which the formes were typeset. The play starts near the end of the previous quire and finishes about halfway through the next quire, but I have confined attention to quire mm because I want to consider the places where he subdivided a page to allow it to be set from more than one typecase or distributed into more than one typecase.

⁴⁰ I, 121, emphasis in original.

⁴¹ II, 19.

Page	Column	Typecase Set From	Typecase Distributed Into	Hinman Compositor
mm3 ^v	a	x	y	A
mm3 ^v	b	x	x	A
mm4 ^r	a	y	x	B
mm4 ^r	b	y	y	A
mm3 ^r		x	x	A
mm4 ^v		x	y	A
mm2 ^v		x	x	A
mm5 ^r	a	x	y	A
mm5 ^r	b	y	y	B
mm2 ^r	a	y	y	B
mm2 ^r	b (top third)	y	y	B
mm2 ^r	b (rest of col.)	x	y	A
mm5 ^v		y	x	B
mm1 ^v		x	x	A
mm6 ^r		y	y	B
mm1 ^r		x	y	A
mm6 ^v		y	y	B

Table 2—Hinman’s attributions for *Macbeth*

We could guess from Table 2 that Hinman subdivided some pages so that he could have Compositor B set only from case y, and Compositor A set only from case x with just the exception of column b on mm4^r. He was interpreting the evidence to vindicate his axiom. We do not even need to guess because he tells us that he attributed the columns, and in one case just the top third of a column, between the compositors from the evidence of spellings, and he fitted the typecase attributions accordingly.⁴² This is one of several places in the Folio where he deduced that Compositor B was “called away,” to explain why a different compositor should take over from him in the middle of a page.⁴³ The attributions for page mm2^r look especially suspect, Hinman being forced to have one column set by two compositors from different typecases, with the consequent need to have its predecessor page mm3^v distributed into both typecases.

⁴² II, 192, 199.

⁴³ In one instance Hinman tells us that Compositors A and B “were absent ... and that both of them returned after being away for one working day” (II, 193).

Table 3 shows my own attributions, derived from the type-recurrence evidence and without taking the compositor attributions into account.⁴⁴ My changes to Hinman's attributions are shown in bold.

Page	Column	Typecase Set From	Typecase Distributed Into
mm3 ^v		x	x
mm4 ^r	a	y	x
mm4 ^r	b	y	y
mm3 ^r		x	x
mm4 ^v		x	y
mm2 ^v		x	x
mm5 ^r	a	x	x
mm5 ^r	b	y	x
mm2 ^r		x	y
mm5 ^v		y	x
mm1 ^v		x	x
mm6 ^r		x	y
mm1 ^r		x	y
mm6 ^v		y	y

Table 3—My changes to Hinman's attributions for quire mm (in bold)

The reader will see from Table 3 that my reconstruction effects a simplification. It is no longer necessary to split page mm2^r at all, let alone into three units, and therefore no longer necessary to split its predecessor mm3^v. In consequence, the successor page mm6^r is now set from case x. It might be possible to interpret the type-recurrence evidence to simplify even further; for example, by having mm4^r distributed into one typecase instead of two. But that would require extending the analysis back into the previous quire. I shall undertake a comprehensive exercise such as that later in this essay, when I deal with case z, but it is not necessary for my present purpose. The spellings evidence of course remains unchanged; therefore, we may, if we wish, keep Hinman's compositor attributions, provided we do not insist on his axiom.

It might seem that in making my changes I have simply gone through Hinman's list, arbitrarily changing some y attributions to x. Of course, there is more method to it than that. For each change I have made to the attribution for a page in quire mm, I have checked to see if that creates anomalies on any page that

⁴⁴ See my spreadsheet "Macbeth-with-new-attributions.xlsx".

shares a type with it, and where necessary I have made the consequential change. I found that, apart from the changes listed in Table 3, I needed to make only one change. Hinman had found that column a of ee1^v had been distributed into case y and column b into case x. Consistent with the changes that I made for mm2^r and mm6^r, which share types with it, I have found that the whole of ee1^v was distributed into case x, which is another simplification. Considering in turn the pages that share a type with ee1^v, I found that no other changes were needed. My handful of changes slot nicely into the rest of Hinman's reconstruction, subject to the evidence of anomalies.

My changes increase the number of anomalies from 238 to 240.⁴⁵ Further analysis might enable me to reduce that number by making more extensive changes. One of my anomalies might even be discountable: my changes make type h44 anomalous on v5^r, a page in *Henry VIII*. Hinman had already been forced to declare type H21 anomalous on that page, that type having been last seen on page nn6^v in *Hamlet*, the play printed just after *Macbeth*.⁴⁶ Because of the concurrent printing of some histories and tragedies, there may have been greater disruption to the type distributions among the cases than we have detected. Even if we avoid special pleading and insist on treating h44 as anomalous, the increase of 2 anomalies that my changes bring about is a small price to pay to reduce the implausibility of Hinman's reconstruction for *Macbeth*.

The point I hope I have illustrated is that the type-recurrence evidence is capable of more than one interpretation and the one Hinman chose was not always the most natural one, but it was almost always the one that allowed him to vindicate his axiom. The evidence for this is strong. By my count, for 72 pages set from case x he told us that they were then distributed into case y, and we can observe that the next page on which types from that page mostly appear is a Compositor B page. Similarly, for 47 pages set from case y he told us they were then distributed into case x, the next page being a Compositor A page. The numbers going the other way are only 22 and 9, confirming that it is the axiom which is usually driving the change.⁴⁷

Because of this, any supposed independence between the two kinds of attributions is illusory. To say so is not hyperbolic because, as I have explained earlier, typecase attributions are progressive. Thus, even though Hinman directly invokes compositor attributions to make typecase attributions for only a minority of pages, there is a cascading, indirect effect from every such invocation. The whole reconstruction is thereby infected, and we cannot say that the compositor

⁴⁵ Through my changes the following three page/type combinations are no longer anomalous: {ee6^v, T25}, {mm1^v, W43}, {mm2^r, G30}, while the following five have become anomalous: {mm2^r, W42}, {mm6^r, Y21}, {mm6^r, n32}, {mm6^v, Y25}, {v5^r, h44}.

⁴⁶ II, 228.

⁴⁷ These counts can be checked easily, although not quickly, by looking at the tables on the facing pages to the graphs in volume II.

attributions and typecase attributions are independent. The “correlation” I conceded in my earlier essay is not a true statistical correlation.

Case Z

Hinman’s reasons for inferring the existence of typecase z are given in his discussion of quire K.⁴⁸ They are surprisingly brief, given their significance. As his discussion of subsequent quires shows, that typecase’s use was then inferred in the setting of more than two dozen pages. The Folio has almost nine hundred pages, so that is a small proportion. Nevertheless, the printers needed only two typecases to allow the two pages in each forme to be set at the same time, and these had already been named as the typecases x and y that Hinman detected from the first play to the last. Therefore, we should have expected weighty reasons for the invocation of a third typecase for just a few quires, being mindful of the risk that every researcher runs, of overfitting the theory to the evidence, allowing the kind of quirks that often arise in real-life data to exercise a disproportionate influence. Because of its importance, I shall challenge each part of Hinman’s reasoning, in order.

The first reason is “that K2^v takes numerous distinctive types from hitherto undistributed I1^r ... but not a single type from any other quire-I page.... And none from any page earlier distributed into case y.” That statement is of course correct: of K2^v’s 12 distinctive types, 11 had last appeared on just one page, I1^r. However, Hinman might not have realized that the phenomenon is not uncommon. A typical Folio page has about four thousand types, excluding spaces, of which typically only 15 or so were found by him to be distinctive. With such a small sample it should not surprise us if every now and then all those types had also appeared together in a nearby page. That would have been very improbable if the types had been randomly chosen from the typecase, but we can be sure that the choice was far from random. When a type is distributed into a typecase it lands on top of the ones already in its box and is therefore disproportionately more likely to be picked for typesetting on the next page; indeed, that is a foundation of Hinman’s work. Browsing through his graphs, we can see that as many as 30 Folio pages take all their distinctive types from just one page each.⁴⁹ For example, all 21 of n6^v’s types are from one page, as are 13 of kk2^v’s 15 types (the other two first appear on that page). Ironically, K2^v is not among my list of such pages, because one of its types comes from a different page to the rest, a fact that Hinman disregarded because he could not identify the typecase used to set that page.

Hinman’s next reason is this: “None, above all, from I6^v – a page which had just been distributed into case x ...”. This is revealing, because it gives us an insight into the reason why he apparently expected K2^v to contain types last known to have been distributed into case x. Although he acknowledged that in quire K

⁴⁸ II, 400.

⁴⁹ A6^r A3^r A4^v A2^v A5^r B3^r B6^r B6^v E4^r E6^v G6^v I2^v L1^v m1^v n6^v o3^v o4^r d4^v d5^r e4^r xgg7^v q4^v kk2^v t2^v qq1^v rr5^r rr6^v Gg2^v hh5^v ¶¶¶1^r.

the “spelling evidence is less than ordinarily useful,”⁵⁰ it consists mostly of *doe-goe-here* spellings and he therefore felt compelled to assign K2^v to one of the type-A Compositors, all of whom were *doe-goe-here* men, unlike their colleague Compositor B, who was a *do-go-heere* man. Consistent with his axiom, this led Hinman to expect K2^v to be a case x page. Had it not been for that axiom, he should have had no reason to find it remarkable that K2^v has none of the types that had just been distributed into case x, for if two typecases are in use by two compositors, then it is only to be expected that one of them will not provide types to a page in the next forme to be set. But Hinman, having ruled out Compositor B as the man who set K2^v and thereby having discounted case y as the typecase, expected to find on that page types from the last page distributed into case x. When he did not find them, his belief was reinforced that a new typecase had appeared on the scene.

Next, Hinman wrote: “In fact types last seen in pages distributed into case y do not appear in force again, after page K4^r, until Compositor B reappears too, in quire M.” As I shall show now, that is a circular argument. For comparison, I asked the equivalent question for the Histories section. That is, I looked at pages from the start of the Histories section and found the types last distributed into case y before page k4^r, and among those I found the subset that appear again after that page but before quire m. While we cannot expect such ratios to be constant across the whole Folio, they help at least to set our expectations. Returning to the Comedies, I then looked for types last distributed into case y before page K4^r which appear again after that page but before quire M. The results are shown in Table 4.

Page	Types last distributed into case y before page K4 ^r /k4 ^r	Types that appear again before quire M/m	Types that appear again before quire M/m in pages set from case y
K4 ^r	34 ⁵¹	13	0
k4 ^r	107 ⁵²	38	35

Table 4—Comparison of case y type movements in the Comedies and Histories

⁵⁰ I, 410.

⁵¹ A31 C24 D21 D23 F22 F23 G24 G25 H36 L23 M23 T31 W22 d22 d23 d53 e32 e33 h35 h36 h44 h59 k21 n26 n38 n48 o37 o38 o42 s28 sh26 st28 st33 u25.

⁵²)22)24 A23 A32 A34 B21 B25 B28 B31 B33 C22 C24 D22 D23 E22 F24 F26 G21 G23 G28 H22 H28 H33 I22 M25 N22 N29 O21 O22 O24 O26 P32 S22 S25 T26 T28 V22 V24 W21 W30 W31 W35 W41 W43 W44 W45 a21 a26 b21 c24 d23 d24 d33 d38 d54 e25 e28 e31 f27 fi21 fi21 g21 g22 g23 g24 g28 g29 h29 h35 h36 h37 h41 h44 iC21 k22 m21 m30 n24 n26 n33 n47 o23 o34 o35 o42 o47 o49 p24 p25 s22 s25 sh25 si22 st23 st24 st25 st29 t22 t25 u23 u34 v22 w29 w34 w35 y23 y24.

We can see from Table 4 the point Hinman was making. There is nothing significant about the fact that only 13 of the 34 types distributed into case y before K4^r appear again before quire M, because we see about the same proportion in the Histories. What he regards as significant is that none of those 13 types re-appear on pages set from case y, whereas in the Histories a high proportion do re-appear in pages set from case y. However, that is because he declined to infer the use of case y on any page after K4^r and before quire M. What he gives as a reason to invoke the new typecase z is in fact a consequence of the invocation, making his argument circular. Among the pages after K4^r and before quire M, he deduced 10 pages to have been set from case z. As I shall show presently, the type-recurrence evidence is entirely compatible with those pages having been set from case y and, had he made that deduction, the discrepancy he claimed would have disappeared because then there would have been several case-y pages after K4^r and before quire M.

Turning now to Hinman's two other reasons for his invocation of case z, we see that they were based on an observed shortage of types. He wrote:

Moreover, there are signs in K2^v that the case from which it was set was far from full, as if this case contained *only* the types just distributed into it from page I1^r. Nearly twenty times in K2^vb we find 'vv' substituted for 'w'; and at the bottom of the column, evidently because the supply of italic 'H' types had been exhausted, we four times find 'Bero.' instead of 'Hero.'⁵³

Again, this is correct, but a review of evidence from the whole Folio provides context that casts doubt on the significance Hinman placed on it. K2^v has 117 'w' characters.⁵⁴ Of the 883 pages in the Folio, only 13 have more 'w' characters than K2^v, the average and median counts for all pages in the book both being 79.⁵⁵ This means that the setting of K2^v was always going to create an unusually high demand for 'w' types. What made the position worse was that, according to Hinman's own reconstruction, while K2^v was being typeset, I5^r, K3^r, and K4^v had been typeset but not yet distributed. Those three pages had used 101, 88, and 104 'w' types respectively, of which numbers the first and third are much higher and the second slightly higher than the average. Some 'w' types may indeed have been lent from one typecase to another during the typesetting of these pages. Be that as it may, we can deduce that at the time that K2^v was set, the shop had an unusually high number of 'w' types at press or waiting to be distributed. Thus, there would likely have been a shortage of 'w' types available for use in setting K2^v, regardless of the typecase it was set from.

⁵³ II, 400 (emphasis in original).

⁵⁴ This count, and the ones in the next sentence, are slightly too high. My computerized text of the Folio does not distinguish between roman and italic types. Given the rarity of italic 'w' types, the overcounting does not affect the point I am making.

⁵⁵ F1^v F6^v K2^r P3^r P5^r R5^r T2^r Cc1^r xgg1^v p3^v q3^r q2^v qq4^v.

The evidence for Hero's speech prefixes is similarly illuminating. Before K2^v was typeset, five nearby pages had been set in which she speaks or is mentioned by name: I5^r, and the four pages in the K3–K4 sheet. To add to the demand, Verges is referred to as *Headborough* five times on K3^r, and there is even a mention of *Hercules* on K4^v. These pages had therefore used no fewer than 46 'H' types. According to Hinman's reconstruction, of these pages, four had not yet been distributed and the other, K3^v, had just been distributed into a different typecase, which was being used at the same time to set K2^v's forme-mate K5^r. This means that none of those 46 'H' types were available for K2^v and that is surely a more plausible explanation for the substitution of 'B' for 'H' four times at the bottom of that page than to suppose as Hinman did that the typecase had nothing but one pageful of types to start with. Had the typecase really contained so few types, the shortfall would surely have been spotted before the typesetting of K2^v began, and the problem solved, as it could easily have been, by distributing one or more pages from the K3–K4 formes.

We have seen that none of the reasons Hinman gave for inferring the presence of a new typecase are compelling on their own. I do not think they are compelling even when taken collectively. Each of them is based on a phenomenon which can be shown to have occurred elsewhere in the Folio, and each of them is the kind of thing that could be expected to happen routinely during the printing of that book. We should not regard their co-occurrence as something that requires the invocation of a new temporary typecase. Hinman found this combination of evidence in only the sixth of the Folio's thirty-six plays. Had he found it later, after greater familiarity with the type-recurrence data, and had it not been for the influence of his axiom on his thinking, he might not have been driven to the solution he gave us. That solution is disproportionate to the problem it solves; more importantly, as I shall show, we do not really have the problem that he thought we do.

Even if, as I have just argued, Hinman's reasons for invoking typecase z cannot survive a comparison with evidence from the rest of the book, that is not enough to dispose of his attributions of pages to case z. The possibility remains open that, even after dismissing the reasons he gave, it might still be necessary to invoke that new typecase because otherwise the type-recurrence evidence would be intractable. Tellingly, that was not a reason he gave but it would be a compelling one if it were true. The only way to find out is to try to change his attributions to eliminate case z and see what happens. Therefore, as an experiment I changed all Hinman's attributions to replace case z by x or y.⁵⁶ The results are given in Table 5.

⁵⁶ See my spreadsheet "Attributions-without-case-z.xlsx".

Page Unit	Col.	Lines	Set From (Hinman)	Set From (My Change)	Distributed Into (Hinman)	Distributed Into (My Change)	Compositor
I3 ^v			x		y		A
I4 ^r			x		x		A
I3 ^r			y		y		B
I4 ^v			x		x		A
I1 ^v			y		y		B
I6 ^r			x		x		A
I2 ^r	a		y		x		B
I2 ^r	b		y		y		B
I5 ^v			x		x		A
I1 ^r			y		z	y	B
I6 ^v			x		x		A
I2 ^v			y		x		B
I5 ^r -a			x		z	y	A
I5 ^r -b			y		z	y	B
K3 ^v			x		x		A
K4 ^r	b		y		z	y	B
K4 ^r	a		y		x		B
K3 ^r			x		x		A
K4 ^v			x		z	y	A
K2 ^v			z	y	z	y	D
K5 ^r			x		x		A
K2 ^r			x		x		C
K5 ^v			z	y	z	y	D
K1 ^v			x		x		C
K6 ^r			z	y	z	y	D
K1 ^r			x		x		C
K6 ^v			z	y	z	y	D
L3 ^v			x		x		C
L4 ^r			x		z	y	C
L3 ^r	a		x		x		C

Page Unit	Col.	Lines	Set From (Hinman)	Set From (My Change)	Distributed Into (Hinman)	Distributed Into (My Change)	Compositor
L3 ^r	b	11, 29	x		z	x	C
L3 ^r	b	44, 48, 52, 54	x		x		C
L4 ^v			z	y	z	y	D
L2 ^v			x		x		C
L5 ^r			z	y	z	y	D
L2 ^r			z	y	x		D
L5 ^v			z	y	y		D
L1 ^v			x		x		C
L6 ^r			z	y	y		D
L1 ^r			x		x		C
L6 ^v			z	y	y		D
M3 ^v			x		x		C
M4 ^r			y		y		B
M2 ^v			x		x		C
M5 ^r			y		y		B
M3 ^r			x		x		C
M4 ^v			y		y		B
M2 ^r	a		x		x		C
M2 ^r	b		x		z	y	C
M5 ^v	a		y		z	y	B
M5 ^v	b		y		x		B
M1 ^v	a		x		y		C
M1 ^v	b		x		x		C
M6 ^r	a		y		y		B
M6 ^r	b		y		z	y	B
M1 ^r	a		x		x		C
M1 ^r	b		x		z	x	C
M6 ^v	a	18, 21, 22, 25, 32	y		z	x	B

Page Unit	Col.	Lines	Set From (Hinman)	Set From (My Change)	Distributed Into (Hinman)	Distributed Into (My Change)	Compositor
M6 ^v	a	43, 46	y		y		B
M6 ^v	b		y		y		B
N3 ^v	a		x		x		C
N3 ^v	b		x		y		C
N4 ^r	a	1, 5, 24, 39	z	y	y		D
N4 ^r	a	53	z	y	z	x	D
N4 ^r	b		z	y	z	x	D
N3 ^r	a	5, 9, 13	x		z	x	C
N3 ^r	a	25, 29	x		y		C
N3 ^r	a	34, 46, 47, 48, 54, 63	x		x		C
N3 ^r	b		x		x		C
N4 ^v	a		z	y	z	x	D
N4 ^v	b		z	y	y		D
N2 ^v	a		y		x		B
N2 ^v	b		y		y		B
N5 ^r	a	1, 3, 5, 18, 19, 30, 26, 55	z	x	z	x	D
N5 ^r	b	2, 10, 17, 21	z	x	z	y	D
N5 ^r	b	58	z	x	y		D
N2 ^r			x		z	x	C
N5 ^v	a		x		z	y	C
N5 ^v	b		x		x		C
N1 ^v			y		y		B
N6 ^r	a		z	x	x		D
N6 ^r	b		z	x	y		D
N1 ^r			y		x		B

Page Unit	Col.	Lines	Set From (Hinman)	Set From (My Change)	Distributed Into (Hinman)	Distributed Into (My Change)	Compositor
N6 ^v			z	x	z	y	D?
O3 ^r			y		x		B
O4 ^v -a	a		y		x		B
O4 ^v -b	b	14	x		x		C
O4 ^v -b	b	31, 32, 33, 43, 58, 63	x		z	y	C
O2 ^v			x		x		C
O5 ^r	a		z	y	z	y	A
O5 ^r	b		z	y	x		A
O3 ^v			x		z	y	C
O4 ^r -a			y		x		B
O4 ^r -b			x		x		C
O2 ^r			x		x		C
O5 ^v			z	y	z	y	A
O1 ^v			x		x		C
O6 ^r			z	y	z	y	A
O1 ^r	a	5, 23, 26	x		z	y	C
O1 ^r	a	35, 40, 42, 43, 49, 54, 57, 60, 64	x		y		C
O1 ^r	b		x		x		C
O6 ^v	a		z	y	z	y	A
O6 ^v	b	8, 18, 27	z	y	z	y	A
O6 ^v	b	39, 49, 50, 55	z	y	x		A
P3 ^v			z	y	z	y	A
P4 ^r	a		x		y		C
P4 ^r	b	7, 15, 20	x		y		C

Page Unit	Col.	Lines	Set From (Hinman)	Set From (My Change)	Distributed Into (Hinman)	Distributed Into (My Change)	Compositor
P4 ^r	b	21, 23, 25, 28, 35, 41, 56, 59, 64	x		x		C
P3 ^r	a		z	y	y		A
P3 ^r	b		z	y	x		A
P4 ^v	a	26	x		y		C
P4 ^v	a	34, 39, 42, 62, 63	x		x		C
P4 ^v	b		x		x		C
P2 ^v	a		z	y	z	y	A
P2 ^v	b	12, 22, 31	z	y	y		A
P2 ^v	b	53, 59, 61	z	y	z	y	A
P5 ^r	a	3, 7, 24, 25, 26	x		y		C
P5 ^r	a	55, 62	x		x		C
P5 ^r	b		x		x		C
P2 ^r	a		z	y	z	y	A
P2 ^r	b		z	y	y		A
P5 ^v -a1	a	1, 3, 9, 17	x		y		C
P5 ^v -a2	a	52	y		x		B
P5 ^v -b	b		y		x		B
P1 ^v	a		x		z	y	C
P1 ^v	b		x		x		C
P6 ^r	a		y		y		B
P6 ^r	b		y		x		B
P1 ^r	a		z	y	x		D
P1 ^r	b		z	y	z	y	D

Page Unit	Col.	Lines	Set From (Hinman)	Set From (My Change)	Distributed Into (Hinman)	Distributed Into (My Change)	Compositor
P6 ^v -a	a		y		y		B
P6 ^v -b	b	11, 14, 17, 20	x		y		C
P6 ^v -b	b	29, 37, 45, 55, 64	x		z	y	C
Q3 ^v			x		x		C
Q4 ^r			y		z	y	B
Q3 ^r			x		x		C
Q4 ^v			z	y	x		D
Q2 ^v			x		x		C
Q5 ^r	a		z	y	y		D
Q5 ^r	b	24, 35, 36	z	y	y		D
Q5 ^r	b	50, 65	z	y	x		D
Q2 ^r			y		y		B
Q5 ^v -a			x		x		C
Q5 ^v -b			z	y	x		D
Q1 ^v			x		y		C
Q6 ^r			z	y	x		D
Q1 ^r	a		x		x		C
Q1 ^r	b	1, 4, 6, 9, 10, 13, 21, 22	x		x		C
Q1 ^r	b	27, 43, 53, 57, 65	x		y		C
Q6 ^v			y		y		B

Table 5—My attributions to eliminate case z

Table 5 lists the 150 page units between quires I and Q inclusive, in the order in which Hinman found the corresponding formes to have been typeset. These are all the quires in which he detected the presence of case z, either in typesetting

or distribution or both. For each page unit, I list the case that Hinman deduced it was set from and the case that he deduced it was distributed into. To make it easier to spot my changes, I have listed my attributions only where they differ from his; where I have left a cell in the table blank, my attribution should be assumed to be the same as his, which is given alongside. For completeness I have listed all pages in the quires, even the ones for which I have made no change to his attributions. I have also listed Hinman’s own compositor attribution for each page unit, although I took no account of them in making my typecase attributions. On principle I have not created new subdivisions of the pages, even if that might have enabled me to improve my reconstruction, regarding it as presenting too great a risk of overfitting. My attributions involve changes to Hinman’s attributions of the typecase that a page was set from or the one it was distributed into, or both. For each of 44 page units I have changed the typecase that some or all of it was set from, always to eliminate case z and replace it by x or y. For each of 43 page units I have changed the typecase that some or all its types were distributed into, always to eliminate case z. My reconstruction thereby eliminates case z completely. I now need to justify these changes.

There are two objective measures of the quality of any interpretation of the type-recurrence evidence. The first is the number of anomalies created by our interpretation. The second is the simplicity of the interpretation. We should be wary of any interpretation that tends towards over-intricacy, such as the subdividing of pages into small pieces and having them set from or distributed into different typecases, just to allow ourselves to explain the appearance of a handful of distinctive types. There is an obvious danger of overfitting every time we do that. I shall consider my reconstruction by reference to these measures.

As I have noted, the number of anomalies in Hinman’s attributions for the whole Folio is 238. My changes increase that number to 241. I have eliminated 10 of his anomalies while adding 13 of my own, giving a net increase of 3. That satisfies my requirement that any change we make to Hinman’s attributions must not cause the number of anomalies to rise too much. Table 6 gives a list of the anomalies. Where the source of an anomaly is stated in the table as Hinman, it means that the anomaly arose from his attribution but is removed by my change; conversely, where the source is given as myself, it means that there was no anomaly from Hinman’s attribution, but my change has introduced it.

Source of			Previous	Previous
Anomaly	Type	Page Unit	Typecase Set From	Typecase Distributed Into
Myself	y23	L6 ^r	y	L3 ^r
Myself	sh24	L6 ^v	y	L3 ^r

Hinman	iP21	M5 ^v	y	K5 ^v	z
Myself	d21	N5 ^r	x	L4 ^r	y
Myself	G21	N5 ^r	x	M6 ^r	y
Myself	st23	N5 ^r	x	L5 ^r	y
Hinman	f24	N5 ^v	x	M6 ^v	z
Myself	B21	N6 ^r	x	L4 ^v	y
Myself	Y26	N6 ^r	x	K4 ^v	y
Myself	i21	N6 ^v	x	N5 ^r	y
Hinman	N24	N6 ^v	z	M1 ^r	x
Hinman	r29	N6 ^v	z	M1 ^r	x
Myself	W42	N6 ^v	x	N5 ^r	y
Myself	h40	O5 ^r	y	N2 ^r	x
Myself	m30	O5 ^r	y	N2 ^r	x
Hinman	W30	O4 ^r -b	x	N2 ^r	z
Hinman	h36	O2 ^r	x	N3 ^r	z
Myself	u24	O5 ^v	y	N5 ^r	x
Hinman	S25	O1 ^v	x	N2 ^r	z
Myself	G21	O6 ^r	y	N5 ^r	x
Hinman	o23	Q6 ^r	z	P6 ^v -a	y
Hinman	sh22	R6 ^v	y	O1 ^r	z
Hinman	a26	S6 ^r	y	P1 ^r	z

Table 6—Differences between anomalies from Hinman’s and my attributions

The pages in Table 6 are listed in the order in which their corresponding formes were typeset. The values for the typecase set from and the typecase distributed into are from the relevant source of the attributions, Hinman or myself. For example, the third row in the table tells us that the anomaly is from Hinman: he told us that after being used in page K5^v, type iP21 was distributed into case z but was next seen in page M5^v which he attributed to case y, and that is the anomaly. I have said that I will not use special pleading to discount the anomalies that my reconstruction creates, and that is why I have declared my total to be 241. But I want to note the curiosity that a little special pleading could enable me to equal Hinman’s total of 238. The reader will observe from Table 6 that I have created 3 anomalies on page N5^r. When we look at the Folio page to see where these anomalous types are, we discover that they are on lines 1, 3 and 5 of column a, being the first three distinctive types on that page. They are anomalous because they come from predecessor pages that were distributed into

case y by my reconstruction, whereas N5^r is set from case x, and its forme-mate N2^v set from case y. However, suppose that the intention was that the compositor who set N2^v from case y would continue work on the forme and set N5^r from the same case. He might have set the first few lines of that page from case y, before being called away, leaving another compositor to set the rest of the page from case x. If that happened then the three types would not be anomalous, bringing my total down to equal Hinman's. I do not rely on this explanation and am content to acknowledge my slightly higher number of anomalies, but it is an interesting explanation and worth noting.

As the reader will see from the attributions in Table 5, my changes bring about a noticeable simplification to Hinman's reconstruction. I have not changed the number of typesets needed to set any page. Among the pages which he had divided into two or more page units, so that they could be distributed into different typesets, I have changed the attributions for 5 pages so that they no longer need to be divided: L3^r, M6^r, M1^r, P2^v, and P2^r. For another 3 pages, I have reduced the divisions so that they now have only two units each, one per Folio column, without the columns themselves being subdivided: N5^r, O1^r, and P6^v. Hinman had decided that from quire I to quire Q, 54 pages were wholly or partly distributed into a different typeset than the one they were set from. My changes reduce that number to 44: for 11 pages my reconstruction finds that they were distributed into the same typeset they were set from, while for just 1 page have I gone in the opposite direction. Hinman had found that N6^v had been set from and distributed into case z; having eliminated that case, I find that it was set from x but distributed into y. I have not increased the number of cases into which any page was distributed, but for 8 pages I have reduced it: L3^r, M1^r, M6^r, N3^r, O1^r, P2^r, P2^v, and P6^v. The most striking of these is N3^r, which Hinman somewhat improbably divided into 3 units for distribution, with column a being distributed into all three typesets. I first held that the whole page was distributed into case x, which I still regard as the most natural interpretation of the type-recurrence evidence here. However, two types from column a, st28 and y24, then became anomalous and, faced with a choice between complexity and anomaly, I reluctantly opted for the former by accepting Hinman's finding that just those two types were distributed into case y. It is not a satisfactory interpretation, although it seems to be the best one we have, but my suspicion remains that perhaps the evidence has been disrupted by some undetectable concurrent printing.

Because of Hinman's type-recurrence evidence we know that the Folio was set by formes, not seriatim. The major disadvantage of setting a folio seriatim is that seven pages must be typeset before the first forme can be sent for impression. Setting by formes removes that disadvantage and that is so even if only one compositor is setting the book. However, one further benefit of setting by formes is that two compositors can work in parallel on the two pages of a forme,

allowing it to be sent for impression more quickly. For that to be practical, the two compositors must work on different typesets. This means that when we know a book was set by formes, we must always keep in mind the possibility, perhaps even the likelihood, that each page in a forme was set from a different typeset (and therefore probably by a different compositor). This is a rebuttable presumption, and it must sometimes be rebutted. Indeed, considering the formes for which Hinman could identify the typeset that both pages were set from, he found that in no fewer than 171 formes the pages were set from the same case, in most instances by the same compositor.⁵⁷ Regardless of whether a forme was set from one case or more, any change to its typeset attributions should prompt us to reconsider its compositor attributions. If the number of typesets used to set a forme is increased, it suggests that either one of the compositors switched from one case to another while setting one of the pages, or one more compositor worked on the page than we had previously thought. Similarly, a reduction in the number of typesets might suggest a reduction in the number of compositors. My changes do not increase the number of cases used to set any forme. For 3 formes I have reduced the number of cases used for typesetting from 3 to 2: P1^r–P6^v, P2^r–P5^v, and Q2^r–Q5^v. In each of these formes, this means that case y is used to set the whole of one page but only part of the other and, according to Hinman's compositor attributions, by different compositors. This aroused my suspicion because it occurs nowhere in his reconstruction of any part of the Folio. However, I have shown that compositor attributions are not reliable and that applies particularly to attributions for parts of pages. Once we discount the compositor attributions, the suspicion is somewhat allayed, since Hinman's own reconstruction finds that 15 formes were set with the same typeset used for one whole page and part of the other.⁵⁸

The changes I have made to Hinman's reconstruction are all good simplifications, since our presumption should always be that a printer aims to keep each typeset full by distributing back into it after impression the types that were taken out of it for setting; and that he avoids complexity in the shop's working arrangements, preferring the simpler approach of setting a whole page from one case and distributing all of it into one case. Any other policy would require greater vigilance, to ensure that no typeset became too full or too depleted in general or ran short of some specific types. Moreover, we have incontrovertible evidence that Hinman's reconstruction for the Folio as a whole is largely supportive of that common-sense policy. A browse through volume II shows that types for something like three-quarters of pages were distributed into the same typeset they were set from. But, perhaps tellingly, the evidence is skewed. Table 7 shows for each typeset the

⁵⁷ The most convenient way to see the list of these formes is at II, 514–18.

⁵⁸ I2^v–I5^r O3^r–O4^v O3^v–O4^r h3^v–h4^r i1^r–i6^v k2^v–k5^r xgg2^v–xgg7^r ll2^v–ll5^r mm2^v–mm5^r mm2^r–mm5^v *gg3^r–*gg4^v oo1^r–oo6^v pp3^v–pp4^r rr1^v–rr6^r ¶¶3^r–¶¶4^v.

percentage of pages that Hinman felt compelled to subdivide into more than one unit for setting or distribution or both.

Typecase	No. of Pages	No. of Pages Subdivided	Percentage
x	325	38	12%
y	487	28	6%
z	28	11	39%

Table 7—Pages subdivided for setting or distribution, categorized by the typecases they were set from

In Table 7 the percentage for case z is noticeably higher, even after allowing for the much smaller number of pages set from it. Clearly, he had difficulty in arriving at a satisfactory interpretation of the evidence in these few quires in the Comedies—a difficulty he did not experience elsewhere in the book—and that forced him not just to invoke a new typecase but also to subdivide pages into sometimes improbably fine pieces, to be able to make sense of the type-recurrence evidence. There was probably more concurrent printing going on here than we will ever know. That of course diminishes the credibility not just of Hinman's reconstruction, but mine too.

Any simplification, such as the one I have made, brings with it the risk of an oversimplification that creates fresh problems. With that risk in mind, one further check needs to be done. Suppose a hypothetical forme α - β is distributed into just one typecase, rather than each page being distributed into a different case, which is more usual. Suppose then that types from both pages next appear in forme δ - ϵ . Since the types from both earlier pages were distributed into the same typecase, we should expect them to get mixed up with each other, so that each of pages δ and ϵ should get types from both predecessor pages, not just one of them. We might therefore think it odd if types from α turned up only on δ while types from β turned up only on ϵ . It might suggest that our distribution attribution for α or β was incorrect and perhaps the predecessor pages had been distributed into different cases after all, enabling their types to remain separate. However surprising it may seem, this phenomenon happened regularly in the Folio. Table 8 lists 33 formes in which, according to Hinman's own reconstruction, each page takes types from just one page of a forme which had been wholly distributed into the same typecase, while its forme-mate takes types from the other.

Forme	Type Recurrences
K3 ^r -K4 ^v	H5 ^v ->K3 ^r [1], H2 ^r ->K4 ^v [2]
S2 ^v -S5 ^r	R5 ^r ->S2 ^v [1], R2 ^v ->S5 ^r [1]

Forme	Type Recurrences
T3 ^v -T4 ^r	R6 ^r ->T3 ^v [1], R1 ^v ->T4 ^r [1]
X3 ^r -X4 ^v	V1 ^r ->X3 ^r [4], V6 ^v ->X4 ^v [7]
b1 ^v -b6 ^r	b4 ^r ->b1 ^v [3], b3 ^v ->b6 ^r [2]
b1 ^r -b6 ^v	b2 ^v ->b1 ^r [1], b5 ^r ->b6 ^v [1]
Y2 ^r -Y5 ^v	Y3 ^v ->Y2 ^r [1], b1 ^r ->Y2 ^r [9], Y4 ^r ->Y5 ^v [4], b6 ^v ->Y5 ^v [3]
Z3 ^r -Z4 ^v	Y4 ^v ->Z3 ^r [5], Y3 ^r ->Z4 ^v [2]
Z2 ^r -Z5 ^v	Y4 ^v ->Z2 ^r [1], Y3 ^r ->Z5 ^v [1]
h1 ^v -h6 ^r	h3 ^v ->h1 ^v [2], h4 ^r ->h6 ^r [1]
i2 ^r -i5 ^v	h2 ^r ->i2 ^r [1], h5 ^v ->i5 ^v [1]
k2 ^r -k5 ^v	i1 ^r ->k2 ^r [2], i6 ^v ->k5 ^v [1]
l2 ^v -l5 ^r	k6 ^v ->l2 ^v [10], k1 ^r ->l5 ^r [1]
Gg2 ^v -Gg5 ^r	Gg4 ^r ->Gg2 ^v [7], Gg3 ^v ->Gg5 ^r [20]
Gg1 ^v -Gg6 ^r	Gg5 ^r ->Gg1 ^v [14], Gg2 ^v ->Gg6 ^r [6]
ss1 ^r -ss6 ^v	ss3 ^r ->ss1 ^r [4], ss4 ^v ->ss6 ^v [11]
tt1 ^v -tt6 ^r	tt3 ^r ->tt1 ^v [10], tt4 ^v ->tt6 ^r [1]
tt1 ^r -tt6 ^v	tt2 ^v ->tt1 ^r [8], tt5 ^r ->tt6 ^v [10]
vv3 ^r -vv4 ^v	tt2 ^r ->vv3 ^r [6], tt5 ^v ->vv4 ^v [1]
vv2 ^v -vv5 ^r	tt1 ^r ->vv2 ^v [5], tt6 ^v ->vv5 ^r [10]
vv2 ^r -vv5 ^v	tt2 ^r ->vv2 ^r [3], tt5 ^v ->vv5 ^v [1]
xx3 ^v -xx4 ^r	hh2 ^r ->xx3 ^v [1], hh5 ^v ->xx4 ^r [1]
xx3 ^r -xx4 ^v	vv5 ^v ->xx3 ^r [10], vv2 ^r ->xx4 ^v [10]
xx2 ^v -xx5 ^r	vv5 ^v ->xx2 ^v [3], vv6 ^v ->xx2 ^v [7], vv1 ^r ->xx5 ^r [5], vv2 ^r ->xx5 ^r [2]
xx1 ^v -xx6 ^r	xx3 ^r ->xx1 ^v [2], xx4 ^v ->xx6 ^r [7]
xx1 ^r -xx6 ^v	xx2 ^v ->xx1 ^r [2], xx5 ^r ->xx6 ^v [7]
aaa3 ^r -aaa4 ^v	zz1 ^v ->aaa3 ^r [8], zz6 ^r ->aaa4 ^v [2]
bbb3 ^v -bbb4 ^r	aaa2 ^r ->bbb3 ^v [5], aaa5 ^v ->bbb4 ^r [7]
bbb2 ^v -bbb5 ^r	aaa1 ^r ->bbb2 ^v [3], aaa6 ^v ->bbb5 ^r [7]
bbb2 ^r -bbb5 ^v	bbb4 ^r ->bbb2 ^r [8], bbb3 ^v ->bbb5 ^v [5]
bbb1 ^v -bbb6 ^r	bbb3 ^v ->bbb1 ^v [2], bbb4 ^r ->bbb6 ^r [1]
¶¶2 ^r -¶¶5 ^v	¶¶3 ^v ->¶¶2 ^r [5], ¶¶4 ^r ->¶¶5 ^v [2]
¶¶1 ^r -¶¶6 ^v	¶¶1 ^r ->¶¶1 ^r [1], ¶¶6 ^v ->¶¶6 ^v [1]

Table 8 – Pages in a forme each taking types only from one page of the predecessor forme

In some rows in Table 8, a forme qualifies for inclusion in the list only because of one type. But there are more suspicious examples, such as xx3^r–xx4^v, where xx3^r received 10 types from vv5^v and none from vv2^r, whereas for xx4^v it was the reverse, even though both predecessor pages had been distributed into the same case. A possible explanation, which saves us from having to reject Hinman's distribution attributions for vv2^r–vv5^v, is this. Even when two pages are distributed into the same typecase before it is used for the setting of another forme, one page must be distributed before the other, not at the same time. If two compositors tried to distribute into the same typecase at the same time, their arms would constantly clash with each other's. Therefore, with serial rather than parallel distribution of the two pages, it is possible for the second page's types to cover the ones distributed from the first page, allowing them to be picked for the setting of the next page, thereby exposing the first page's types and allowing them in turn to be picked for the next page. Despite the availability of this explanation, I would have been concerned if my changes had added to the number of formes where this phenomenon is observed. Fortunately, they do not.⁵⁹

We can only speculate why the attributions I have made did not commend themselves to Hinman. It is unlikely that a mere increase of 3 in the number of anomalies—some or all of which he might have been able to discount in the way he discounted many others—was what dissuaded him from giving the reconstruction I have now given. We can see a more likely reason if we look in Table 5 at the instances where I have changed the case used to typeset a page unit from z to y. I have made 38 such attributions, all for page units whose typesetting Hinman had attributed to Compositors A or D. He had never attributed any part of a page typeset from case y to Compositor D and had attributed just column b of mm4^r in *Macbeth* to Compositor A from case y, as we saw earlier. If he had adopted the typecase attributions I have now made, it would have undermined his axiom, and that may have been a strong influence on his thinking.

I hope that the evidence above is enough to make good my point that the advantage of vindicating his axiom that Hinman gained by his invocation of case z for these few quires in the Comedies is comfortably more than counterbalanced by the advantages of my reconstruction. It eliminates the need to invoke a new typecase for these quires; increases the number of anomalies by an insignificant amount, if at all; reduces the number of pages it is necessary to split into more than one unit for setting or distribution; and reduces the number of instances where we must suppose that a page was distributed into a different typecase than the one it was set from. Furthermore, it does not require the *deus ex machina* by which

⁵⁹ My first successful attempt to change Hinman's attributions to eliminate case z resulted in a pleasing reduction of 7 in the number of anomalies, down to 231, but I rejected it because it created 8 new instances of this phenomenon, which seemed uncomfortably high. The interested reader will find that rejected attempt in my spreadsheet "Attributions-without-case-z-rejected-attempt.xlsx".

Hinman "liquidated" case z at the end of quire R, because in my reconstruction the transition between quires R and S is seamless. That is enough to show that the invocation of case z is uncalled for, it being a complicated solution when a simpler and better one is available.

Conclusion

In looking back at what Hinman achieved from his study of the Folio it would not be extravagant to think of Pope's famous lines:

Nature and Nature's laws lay hid in night,
God said, "Let Newton be!" and all was light.

Almost everything we know, or think we know, about the printing of the Folio comes from Hinman. Even someone who browses only casually through his book can hardly fail to be awed by it. The more-than-casual acquaintance with it that I have gained has only increased my respect for his scholarship.

Hinman made his reconstruction of the printing from two premises, although he did not state them as such. The first was that concurrent printing was either not extensive enough to place that reconstruction at risk of significant inaccuracy, or that the account he took of it when the evidence of it has survived was enough to mitigate the risk. I have shown by my experiment with *The Winter's Tale* that the premise is not a safe one to adopt, by proving that even artificially reduced evidence can be interpreted to yield a self-consistent set of attributions. The second premise was that the two main (supposed) compositors, especially Compositor B, each had his preferred typecase, and after impression the typecase into which each page was distributed was chosen with regard primarily to that preference. I have shown that if we do not insist on this premise, for which there is no external evidence, then we can obtain an equally self-consistent set of typecase attributions; moreover, those attributions do not require the invocation of a new typecase just for a few quires.

It is possible that some, perhaps many, of Hinman's compositor attributions are correct. We should expect a folio set by formes to be set by two compositors. Therefore, even a random set of attributions between two compositors will sometimes be correct, and Hinman's attributions were better than random. The problem is that, bearing in mind the failure of the copy texts to survive for most plays, and the ubiquity of journeymen in the printing houses of the time, we will never know which compositor attributions are correct and which are incorrect. At first glance the situation might appear to be better for typecase attributions. Jaggard might have employed many journeymen in the months it took to print the Folio, but he is unlikely to have liquidated and restocked his typecases often. The appearance of some distinctive types from the start to the end of the book suggests

a continuity of operation, at least as far as the equipment in the shop is concerned.⁶⁰ We might think that this provides a stability that we cannot rely on with compositor attributions. Once we discount case z, we have the expected scenario of a folio being set in formes from two typesets, which we may as well call x and y, and we might allow ourselves to think that Hinman's typeset attributions are probably correct in many cases. However, given the gap in our knowledge of concurrent printing, the insoluble problem is that we cannot know what the correct ones are.

I have shown *separately* that the compositor attributions and typeset attributions are unreliable. It may be tempting to think that we can, after all, accept Hinman's appeal to the combined power of these attributions, since they are mutually supportive, and thereby salvage them both. However, I do not think that option is safe either, because each kind of attribution is suspect *even if* we accept the other as sound. I showed that more than half of Hinman's compositor attributions were suspect even on the day he published them, because he made them without knowledge of the spellings in the copy texts; spellings that, in plays for which they are known, he regarded as determinative. That problem would be so even if all the typeset attributions were correct. Conversely, as I have now shown, the typeset attributions are suspect because of the possibility of concurrent printing, even if all the compositor attributions are correct. What Hinman gave us is a persuasive story, almost a human story. We hear of the compositors moving around the shop, of being called away, of getting involved in stripping accidents, even of borrowing types from each other.⁶¹ Like any good story, it has the ring of truth, even though we see from his discussions in volume II the disturbingly high number of times he is forced into special pleading to be able to sustain his narrative. We should not allow ourselves to forget that all we have are black marks of ink on yellowing paper. The rest is inference and, as I have shown here, the mere internal consistency of a set of inferences is not a guarantor of their correctness.

Finally, if the type-recurrence evidence can be satisfactorily interpreted for typeset attribution in more than one way, the possibility arises that the same evidence might be able to support an alternative reconstruction of the order in which the plays were printed. To a large extent the order Hinman gave us is the natural and expected one; for example, all but one of the fourteen comedies were printed in the order deducible from their signatures alone. However, that expected order breaks down early in the Histories section and then again in the Tragedies section when the supposed Compositor E appears on the scene and what Hinman called the intercalary sequence of formes is printed. The evidence for Hinman's order comes from skeleton formes and type-recurrence. That it

⁶⁰ For example, B33 appears in 25 out of the 36 Folio plays. It is found three times in *The Tempest*, the first play; four times in *Cymbeline*, the last play; and in the reset first page of dialogue in *Troilus and Cressida*, which is on the last leaf of the Folio to be printed.

⁶¹ Among many examples, see I, 128; II, 116 and 165.

might be undermined by a different interpretation of that evidence has always been a possibility in theory.⁶² My experiments in this essay suggest that it might be a possibility in practice too. The question is now open.

Appendix—Prequel to Case Z

Hinman's compositor attributions were revised by several scholars in the twenty years or so after the publication of his great book, but with his typeset attributions he almost had the last word. That is no doubt partly due to the differing values that scholars place on compositor and typeset attributions. Compositor attribution is believed by some to be of value to editors in deciding how readily to emend the Folio text in their editions.⁶³ In my view this belief is misguided. Whatever confidence we think we have in our estimates of the kind and quantity of error that compositors were prone to, every word and every punctuation mark in the Folio must be considered by the editor on its merits, and I do not think probabilities are of much use in that. The point was well made by A. E. Housman in relation to scribal rather than compositorial error.⁶⁴ Be that as it may, no one has claimed such significance for typeset attributions, which have been regarded either as knowledge for its own sake, or as aids to compositor attribution. It was in the latter cause that Paul Werstine produced what I believe is the only post-Hinman work of scholarship on typeset attributions.⁶⁵

Werstine's essay shows even more clearly than Hinman's book the co-dependence of compositor attribution and typeset attribution. It is essentially a prequel to Hinman's analysis for case z. He not only accepts Hinman's attributions to case z; he also extends them backwards by finding that case being used in quires G, H, and I. Indeed, he claims that case z had been in use since the start of the printing of the Folio.⁶⁶ He revises some compositor attributions in those quires, using his case z attributions to derive support for them. I have already published my reasons for thinking that—whatever the extent to which Hinman's compositor attributions were correct—the revisions to them by his successors have no credibility. In so far as Werstine bases his arguments on the post-Hinman compositor attributions, they should now be considered suspect for that reason alone. Nevertheless, I shall do my best to separate the parts of his arguments that are based on type-recurrence. I shall

⁶² For example, it is somewhat suspicious that among the handful of three-sheet quires found by Hinman to have been typeset in an irregular order, three (I, M, O) are associated with case z.

⁶³ For example: "The editorial usefulness of compositor study has been demonstrated more effectively in Folio *Lear* than in any other play" (Stanley Wells and Gary Taylor, *William Shakespeare: A Textual Companion* (Oxford: Clarendon Press, 1987), 529).

⁶⁴ A. E. Housman, "The Application of Thought to Textual Criticism," *Proceedings of the Classical Association* 18 (1921): 67–84.

⁶⁵ Paul Werstine, "Cases and Compositors in the Shakespeare First Folio Comedies," *Studies in Bibliography* 35 (1982): 206–34.

⁶⁶ Werstine, 233.

try to show that those arguments for the presence of case z in the earlier quires are not compelling. In doing so I shall ignore my own changes to Hinman's typecase attributions, as they are clearly incompatible with Werstine's. If we adopted both my changes and Werstine's then their combined effect would be to have case z disappear suddenly towards the end of quire I, leaving behind some unwanted anomalies. To be fair to Werstine's arguments we must start from the premise he started from, that Hinman's attributions to case z in quires K and beyond are correct.

In my view Werstine's arguments—despite the meticulously collected evidence he presents—have two fundamental weaknesses. The first is a tendency to overfit the data to his theory, in the way that Hinman did, by subdividing the pages too intricately. I have noted how unlikely it is that a reconstruction is correct that claims to be able to say not just which column but which lines of a page a compositor distributed into which case. The second weakness is that he, like Hinman, did not make enough allowance for the possibility of concurrent printing, and the havoc it plays with nice calculations about setting and distribution. I shall consider a representative example of each weakness from Werstine's essay.

When introducing his type-recurrence analysis of quire H, Werstine writes: "In evaluating the possibility that each of the three quire-H compositors used a different case, we must entertain the second possibility that, not just Compositors B and D, but all three compositors may have shared distribution of the same pages and columns."⁶⁷ He states his first example thus:

Column G5^rb provides types to Compositor D's columns H3^rb and H4^rb as well as to the disputed page H3^v ... but none to any page or column accepted by all scholars as set by Compositor C. Lower column G5^ra provides a type to each of Compositor C's page H2^v and column H4^ra ... but none to any page or column set by Compositor D and none to H3^v. However, upper column G5^ra does provide types to page H3^v.... Had all of page G5^r been distributed into a single case from which both Compositors C and D worked, we might expect that at least one or two of the nine identifiable types in quire H drawn from column G5^rb and upper column G5^ra would recur in pages or columns set by Compositor C. None does recur. Yet the presence of types from lower G5^ra in Compositor C's work confirms and extends Hinman's observation that the compositors of quire H sometimes shared distribution of the same columns from wrought-off pages.⁶⁸

A minor but nevertheless important point here is that Werstine is using the misleading language of 'distribution facts' that Hinman invented for talking about type-recurrence evidence. Again, we see some leading words: "Column G5^rb provides types to ... columns H3^rb and H4^rb...." This is the wording we might use if there had been a direct transfer of types from column b of one page to the

⁶⁷ Werstine, 209.

⁶⁸ Werstine, 209.

b columns of the other pages. Indeed, that claim is key to Werstine's argument. But the words misrepresent the material acts. When a type is distributed from any column of a page, it falls into a typecase. The typecase is not divided into columns a and b. At that moment, the link between the type and the column it came from is broken. Neither the typecase nor the compositor who uses it next for typesetting has any knowledge of the column that any type in the case came from. Werstine's argument that what he observed is significant must be based on the unstated assumption that types from each column of a page are about equally likely to appear on a successor page; without that assumption, there would be nothing remarkable in seeing only or mostly types from one predecessor column on the successor page. Not only is this assumption not plausible in theory, but Hinman explicitly contradicted it. He told us that the 'a' column of a verso page and the 'b' column of a recto page—that is, the two outer columns on the forme—were usually distributed before the two inner columns, because that is more convenient for the distributor.⁶⁹ This in turn makes it quite likely that clusters of types from either column a or column b will turn up together on the successor page, because they have fallen into the typecase on top of the ones from the column that was distributed first. The most that could be said for Werstine's argument is that, if dozens of distinctive types from a page are distributed into a typecase but only those from one column appear on a successor page, then we might be able to argue from probability theory that only one column from the predecessor page had been distributed into that case. But, given that we are dealing with perhaps only a dozen distinctive types from a page, and they are not distributed randomly but from one column before the other, the argument from probability that Werstine implicitly makes is in my view too weak. Despite that, the words that he, following Hinman, uses are liable to create the impression of a purposed transfer of types from one column to another, without the mediation of the typecase that, as a matter of physical fact, breaks the link between them.

Notwithstanding my objections above, let us look more closely at the evidence that Werstine provides. After the distribution of G5^r, forme H3^v–H4^r was, as we should expect, the first forme in quire H to be typeset. It is unremarkable then that 7 types from G5^r turn up in that forme, spread between both pages. The most natural explanation is that both pages were set from the same typecase, something that happened hundreds of times in the printing of the Folio. Of those 7 types, 5 are on one page, and 2 are on the other. There is nothing remarkable about that either. Even if we were to expect the 7 types to be equally spread between the two pages, which is itself doubtful, the best that could happen is that 4 types would be on one page and 3 on the other. That the actual split is 5–2 rather than 4–3 is not in the least significant from a statistical point of view. The next forme to be typeset is of course H4^v–H3^r. We find 4 types from G5^r on this forme. Again, that

⁶⁹ I, 104.

is not remarkable, since all it tells us that the forme was probably typeset from the same case as the previous forme. As the setting of that previous forme will have used up many of the types that lay at the top of the boxes of types in the case, we expect to find fewer types here from G5^r than in that previous forme, and we do. By the same logic, we expect to find even fewer G5^r types on the next forme to be set, H2^v–H5^r, and indeed we find just 1. Thus far, we see that eschewing the leading words applied to doubtful compositor attributions that Werstine used, we have seen nothing in the type-recurrence evidence to justify the conclusion he reached, “that the compositors of quire H sometimes shared distribution of the same columns”. The final piece of evidence, that appeared to clinch the argument for him, is the observation that all 4 types from G5^r which are found on the H4^v–H3^r forme are found in just one column of just one page, column b on H3^r. Now, that types from a predecessor page do not appear on both pages of a successor forme is not remarkable either, as I have already shown in Table 8 that it happened regularly.⁷⁰ Therefore, the point comes down to this: is it significant, as Werstine thought, that all 4 types from G5^r that are found on H3^r are found in the same column? In my view, neither theory nor evidence support the view that it is significant. Even if the 4 types had been randomly placed on the page, the probability that all 4 would end up in the same column is one in sixteen, which is low but not usually considered low enough to be statistically significant. Be that as it may, evidence from the Folio itself shows us that it is not significant.

As a test I picked the first ten pages in the Comedies on which we find exactly 4 types from a predecessor page. I did the same for the Histories and the Tragedies, giving me a reasonable sample of thirty pages, on each of which there are exactly 4 types from a predecessor page (as well as types from other pages of course). This method of selection by me avoids any suggestion of cherry-picking. For each page I checked how many of the 4 types are in column a, and how many in column b. For no fewer than 9 of the 30 pages, all 4 types are in the same column.⁷¹ The types a compositor uses to set a page are neither randomly picked from all the types in the case, nor are they randomly placed on the page, since setting proceeds in order from column a to column b. Therefore, there is no theoretical basis for regarding the appearance of all 4 types in the same column as unusual enough to be significant, and the evidence I have collected confirms that it happens often. Hinman had not made the deductions from this evidence that Werstine made, and I think he was right not to. Werstine’s argument appears to me to be an example of confirmation bias. He made an intricate reconstruction, dividing the

⁷⁰ For that table I applied an even more restrictive condition than Werstine is arguing from here, since I required *both* pages in a forme to take types only from one page each of the predecessor forme.

⁷¹ In this list the page is followed in brackets by the predecessor page, followed by the list of the 4 types they share: A4^r (A1^v): A31 d57 n26 u23; D6^v (D5^v): C24 u28 w31 y24; a6^v (a5^v): o49 st23 u26 u34; h2^v (h4^r–b): M21 N22 d27 e28; h5^v (h4^v): B26 D23 c25 f29; aa1^v (aa3^v): H29 W31 W42 o40; cc4^r (cc2^r): D23 F26 i23 s21; cc3^r (bb4^v): F24 M22 T26 h27; kk4^r (cc4^r): F28 O23 m26 n30.

distribution and typesetting of just a handful of pages between three cases and three compositors, and interpreted unremarkable evidence as confirmation of that.

What I considered above is the first of eight examples that Werstine gives for his conclusions about quire H. In my view, neither alone nor in combination can they justify the conclusions he draws, all being subject to the objections I have made. I shall move on to the second weakness that vitiates his arguments. This is his neglect of the possibility of concurrent printing, as when he claims that “There is also strong evidence in quire H of simultaneous composition of different formes.”⁷² The first two of his three examples are of the same kind as I have discussed already. My interest here is the third example:

Centre rule evidence also indicates simultaneous composition of successive formes. Centre rule *D1* was freed through distribution of page G1^r in preparation for setting forme H2^v:5. Yet centre rule *D1* was used to impose a page in the preceding forme, H3.⁷³

But simultaneous composition is not the only explanation of this evidence. When G1^r was distributed, centre rule *D1* became free. Werstine, following Hinman, supposed that this happened just before the typesetting of the H2^v–H5^r forme. Suppose instead that G1^r was distributed a little earlier, before the setting of the H4^v–H3^r forme, and that some non-Folio material was then required to be worked on. In this scenario of concurrent printing, the typecase into which G1^r was distributed could have been used to set that non-Folio material. If that material was not a folio in two columns, it would not need centre rule *D1*, which could therefore be used in the concurrent setting of Folio forme H4^v–H3^r. If the non-Folio material was printed promptly and its types distributed back into the typecase they had come from, they would be available to appear where they indeed did, in forme H2^v–H5^r. I do not suggest that this is a certain explanation, only that, given what we think we know about the normality of concurrent printing, it is more plausible than to suppose that three men at three different typecases were simultaneously at work on two different formes of the Folio.

Werstine’s essay is a painstaking work of bibliographical analysis. Had it not been for the level of detail in which I have looked at Hinman’s book, that essay would have claimed a much deeper engagement and I intend no disrespect to Werstine in dealing with it so briskly here. I hope I have been able to indicate that his arguments for extending case z backwards into quires G and H are not compelling, requiring an even more intricate division of the setting and distribution duties than Hinman arrived at.

London

⁷² Werstine, 213.

⁷³ Werstine, 214.

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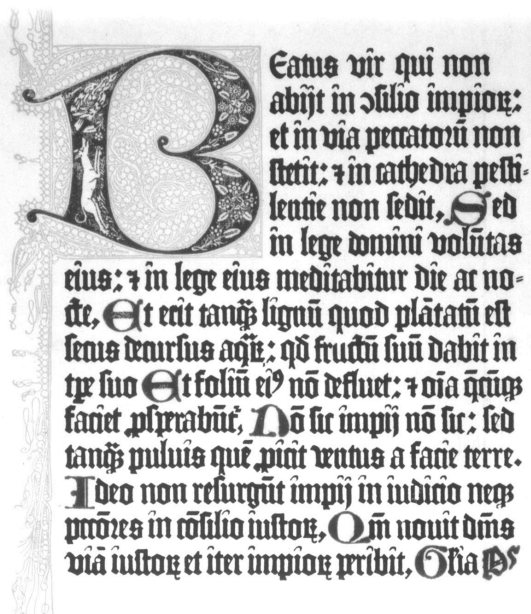
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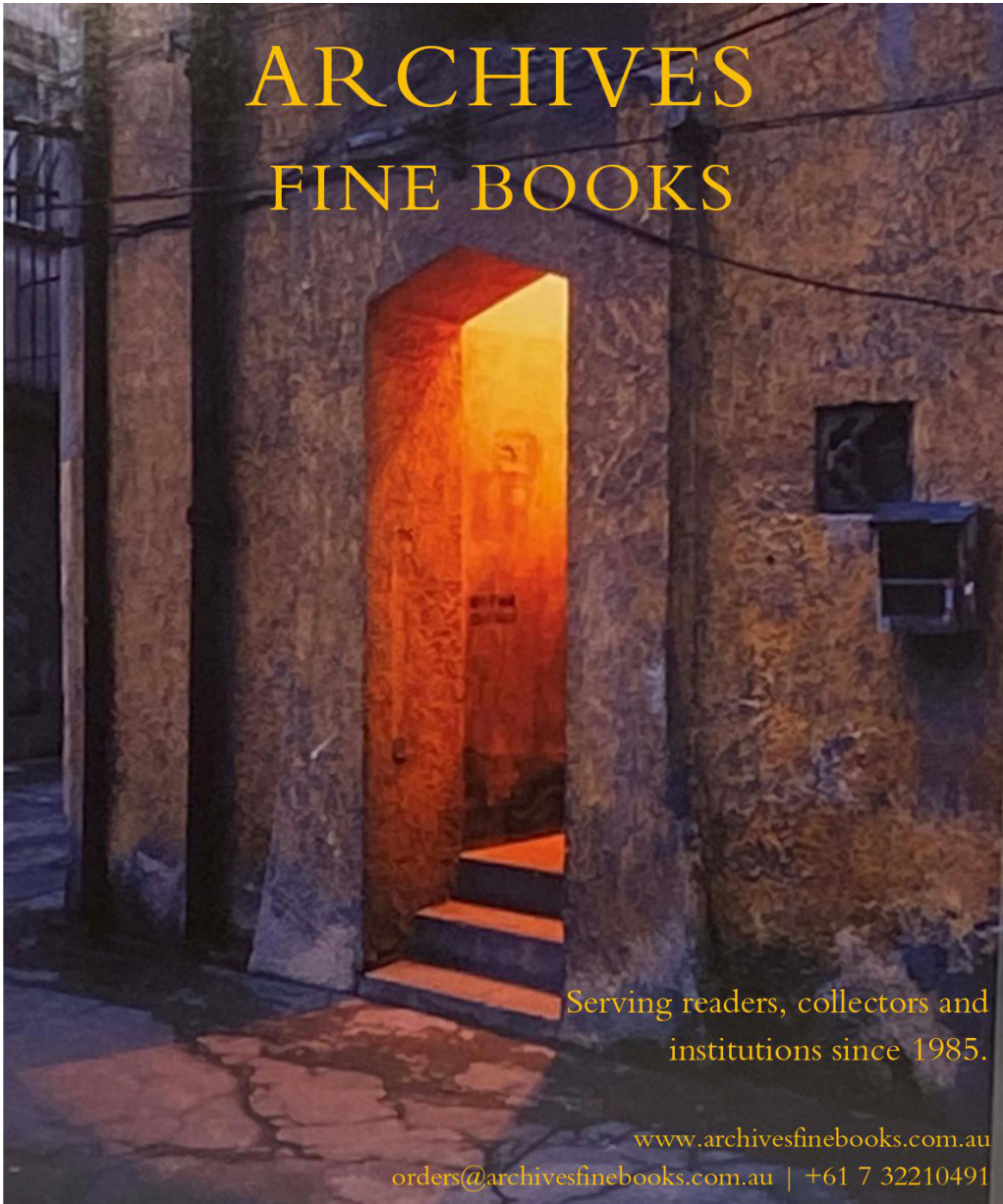


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