A Debate on "D"

To the Editor,

My attention has been drawn to the Dering manuscript of *Henry IV*, the subject of John Baker's article in the Spring 1996 *Elizabethan Review*. Your readers should be interested in the observations I made during a partial examination of the Folger facsimile and transcription, edited by Williams and Evans.

Baker says, "The case for D's originality is straightforward: it holds that bibliographic dependence [on earlier copy] cannot be based on the correspondence of accidentals" (16). This statement may be true only if the definition of 'accidentals' precludes the introduction of other abundant, powerful evidence.

1 Henry IV ran to five editions by 1613, and each quarto was a reprint of the preceding with no authoritative input. I don't believe Baker disputes this, and it is an easily established fact. Q1's features were repeated in Q2 sufficiently to prove that Q1 served as copy. Q2, in the usual nature of early printing, introduced changes of punctuation, corrections, errors, etc., which in their being carried over proved Q3's dependence on Q2, and so on down to Q5. Many features unique initially to one quarto were carried to subsequent editions so that derivative bibliographic evidence accumulated. In the case of D, the claim has been forwarded that the manuscript was based on Q5. Baker's argument (that D preceded Q1) would be invalidated if the evidence shows that D was based on Q5, *or that Q2-Q4 had any influence on the manuscript.*

Evidence is of two kinds: *Accidental*, meaning (for simplicity's sake) the kind of thing that can happen to texts by chance or for arbitrary reasons: punctuation changes, common printing errors, spelling, use of synonyms, etc.; and *substantive*, or those changes which would probably not be repeated by happenstance. Many textual alterations are substantive, but there are gray areas. For example, two compositors could independently spell a word the same way, but if the word is spelled oddly enough, it may be promoted from accidental to substantive.

Baker says the case for dependency of D on Q5 "argued that correspondence to bibliographic errata would show that D was a transcript and also suggested a date. Indeed it would, if it could be shown the correspondence wasn't accidental. As it turns out, D differs in thousands of 'accidentals,' including pagination, lineation, spellings and punctuation. Yet within this mare [?] of differences only three correspondences are cited: a missing pronoun, a misplaced apostrophe, and on f1r, 'the closeness of the punctuation' to Q5. The case was then claimed closed" (17). I will not take issue with Baker's arguments against cited evidence, given the inherent weakness of only three instances. I would note, however, that the number of *differing* accidentals means nothing.

For example, it is not at all significant that the pagination of a manuscript differs from a suspected printed source.

Despite my poor opinion of the scholarship of Hardin Craig, the early supporter of D as authorial, I was interested in Baker's enthusiastic argument. I turned to the Dering facsimile and transcription, armed with a photographic copy of Q1 and the Arden *1 H IV*, which collates the editions to a large extent. What became immediately apparent surprised me.

That part of the Dering manuscript corresponding to *1 H IV* is derived from Q5. There can be no doubt of this because the evidence is more than overwhelming. Therefore, Baker's hypothesis cannot be true; moreover, most of his supportive conclusions and their arguments must be wrong or misleading. After a glance at the D transcription, one must wonder how Craig and Baker could have neglected to perform a basic bibliographic investigation. I suspect that Baker was misled by the minimal evidence offered to prove that Q5 served as D's copy. I suspect also that so few instances of correspondence were cited because the aim was only to show Q5's influence as opposed to an earlier quarto, and no controversy was anticipated, for good reason.

Every page of the manuscript shows the influence of readings unique to Q5 or, because they are much more numerous, alterations introduced in Q2, Q3 or Q4 and carried on to Q5. In virtually none of these variant readings does D show agreement with Q1. In Act I there are about four dozen instances of Q5/D correspondence, a count and rating of which I deem unnecessary. Here are some examples from a less than exhaustive list (from the Arden edition which was based on Q1 and collated with the other early editions, and checked by me against Q1):

I.i. 49-51 *West.* This match'd with other did, my gracious lord, 49
For more uneven and unwelcome news 50
Came from the north, and thus it did import: 51

did] *Qq1,2*;	like *Qq3-5,*	other-like, *Dering.*
For] *Qq1-4*;	Far *Q5*	Far *D.*
import] *Qq1-4*;	report *Q5*	Report *D.*

One may argue that *far* for *for* is not significant, but the other two correspondences between D and Q5 are unlikely to be merely coincidental, especially when Q1 would have had to make an opposite swap in the first place if, as Baker believes, D represents the earlier text:

Report *D*;	import *Q1-4*;	report *Q5.*

An original reading in D, once lost by Q1, could hardly have been independently recovered by the compositors of Q2, Q3, Q4 or Q5. Yet that would have to be the sequence *in every instance*. Even then one must assume that the D readings are correct, when some are demonstrably corruptions of Q1. Obviously, the more natural sequence would have D follow Q5:

I.ii. 181 this same fat rogue] *Qq1-4*; this fat rogue *Q5, F, D.*

F was printed from Q5, but F obviously was not copy for D, because no significant F anomalies repeat in D. I drop most references to F, but it usually repeats the Q5 oddities the same as D, as one would expect.

I.iii. 25 As is delivered] *Qq1-4*; As he delivered *Q5, D.*

I.iii. 79-84 . . . the foolish Mortimer,
Who, on my soul, hath wilfully betrayed
The lives of those that he did lead to fight
Against that great magician, damn'd Glendower,
Whose daughter, as we hear, the Earl of March
Hath lately married:

80. on] *Qq1,2*; in *Qq3-5, D.*
82. That] *Qq1,2*; the *Qq3-5, D.*
83. the] *Q2-5, D*; that *Q1.*

In my soul is an error repeated in Q5 and D. The sophistications in lines 82 and 83 originated in different quartos, but were carried to D. They of course have no authority, even if accepted as correct by modern editors.

III.i. 24-26. Diseased nature oftentimes breaks forth
In strange eruptions, oft the teeming earth
Is with a kind of colic pinch'd and vex'd.

oft] *Q1-3*; of *Q4*; and, *Q5, F, D.*

In this case, the correct reading was lost in Q4, and the attempted correction in Q5 did not succeed. D is clearly at the end of the process, along with F. Why? Because Q5 followed Q4.

These examples are a tiny fraction of the total. There is no need to compound the effect by listing more. Some of the correspondences can perhaps be passed off singly as accidental (despite the unlikely sequence of coincidental recovery discussed above), but most can not, and many substantive alterations are almost by themselves proof against the independence of D.

D also reflects at numerous points a misunderstanding of its copy:

I.ii.96-98.
Prince. Where shall we take a purse tomorrow, Jack?
Fal. 'Zounds, where thou wilt, lad, I'll make one; an I do not,
call me villain and baffle me.

Here *an* means *if*, but D has 'and' in the form of an ampersand, thus spoiling the sense. There are other examples, of course. To go on would be to study the manuscript for its own sake, and not for any conceivable relevance to authority. Such study may be worthwhile. The copyist of the first page slavishly followed Q5 in punctuation and spelling, but the penman who took over did not hesitate to spell and punctuate to his own taste. This shows how scriveners and compositors may alter a text.

Baker has gone to a lot of work to "prove" his theory, but he must be wrong because D follows Q5, and therefore Q1. His resort to elaborate arguments can be instructive. Many contentions in articles of this nature cannot be proved either way, but in this case, because the underlying assumption is clearly incorrect, the supporting argument is false, no matter how strongly worded or believed. I noticed many overreaches in Baker's study, but as long as he had a chance to be correct, one could not complain. For example, he says the handwriting is from the sixteenth century, when common sense says an older, provincial scribe could well produce the work in 1623. In retrospect then, we can appreciate how one may plausibly follow a wrong lead to a wrong argument and conclusion.

Sincerely,
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John Baker responds:

In response to the bibliographic case made for D's dependence on Q5 by Gerald E. Downs, its easy to detect an air of partisan motivation. For example, Downs attacks Hardin Craig, alleging unspecified "poor scholarship," yet Craig's only sins regarding D are to have noticed its authorial nature and to have had his landmark articles on it misquoted by the proponents of dependence. The same scholars who "silently and without record" restored hundreds of readings in their typescript of D because they, like Downs, *believed* D to be a copy of Q5.

Motivation aside, I'm not surprised that Downs' bibliographic analysis turned up many points of similarity between D and Q5. Hemingway cited several dozen in 1936, similarities that Craig and I freely acknowledged. I again point out that bibliographic dependence, in a case such as this, cannot be based on the correspondence of accidentals, or even substantials. Nor can it be based on a "chicken and egg" argument which claims that D's readings are inferior to Q1's because Q1's must be correct since we are accustomed to them, as Downs suggests with the "import"/ "Report" readings. What the Folger editors wisely sought to do, in attempting to prove bibliographic dependence on Q5, was to restrict the correspondence between D and Q5 to variants that were *peculiar* to Q5. Only two can be cited. Even Downs concedes that case failed under my analysis, "I will not take issue with Baker's arguments against cited evidence." I should say, however, that my longer unpublished analysis considered and rejected the additional examples cited by Downs and, before him, by Hemingway. They hold with the status of accidentals when nearly every word in D is somehow different, i.e., in spelling or in capitalization, and more than a few are superior, as Halliwell-Phillips first noted with D's fine "shallow jesters and rash brain'd wits," rather than the confusing "rash bavin wits," which is found in quarto.

Take the difference between "far" and "for" which Downs cited. Isn't it more likely to be indicative of difficulties with a foul paper copy source than the transcription of a printed text? "Report" and "import" are similar looking words in script if one forgets to dot his "i." In Downs' example, that Q1-4 read "this same fat rogue," while Q5, D and F read "this fat rogue," is meaningless because it's likely that if D was the original *shorter* version of *H4*, then"same" entered the text *after* D was set aside, that is, during the play's expansion. So all extra words in Q1 represent revision. This is the whole point. As to why "same" is missing in Q5, it seems to have been due to the dovetailing of lines, or so other bibliographic authorities have suggested.

However, there is no argument for independence which will sway Downs. If one believes D to be dependent on Q5, then every similarity proves it, while every discrepancy is meaningless. Indeed Downs says as much when he asserts, "the number of *differing* accidentals means nothing." But is this true? Aren't upwards of fifty *thousand* differences meaningful? This was the problem with scholarship on this question when I took it up and will remain the problem until scholars study the manuscript from an objective framework. That framework tells us D looks more like an original manuscript than a copy of Q5. If—and only if—it is an earlier version, then all similarities between Q5's variants and D are accidental. The number of similarities in such a case is meaningless and both sides, as I wrote, hit an impasse on this point. The only way out of the deadlock is to back off, set aside the bibliographic argument and consider the broader picture.

Indeed, in an effort to widen his case into paleography, which is part of the broader picture, Downs asserts it is not "at all significant that the pagination of a manuscript differs from a suspected printed source." I'm not so sure, but the point made was that D's lines vary from sheet to sheet, to say nothing of their extreme variation from Q5's layout. This variation is significant since a copyist working from a printed source would be expected to have regularized his lines and to stay with whatever number of lines he discovered could fill his sheet properly. D's wild variations in the number of lines per page must then be judged a doubly significant fact if the scribe was being paid by the sheet, as has been claimed, because it would be foolish for him to increase the number of lines per sheet, as he has often done—for any increase would *reduce* his pay accordingly. In my monograph I cited examples which prove this, even from other manuscripts. Downs, apparently blinded by the categorical efficacy of his paradigm, simply missed the point.

In his second attempt to broaden the discussion, Downs is correct in noting that D often had difficulty resolving textual muddles, such as the one he cited in Falstaff's speech at I.ii. 96-98, "an I do not," where D writes "& I do not." Again, this is the main point. If D were a copy of Q5, D should have written "an", not "&", because Q5 had resolved its text and printed "an," whereas D had not. These sorts of difficulties indicate that D was a transcription of authorial papers, not the printed text. I cited many similar examples. Indeed, in this case, the ampersand "&" may indicate dictation, as does the freedom in spelling. Downs' example of the error in "Who, in [~on] my soul," is notable, but irresolvable. Did it arise as a copy error from foul papers, which was later caught and corrected, or was it a correct transcription, one that might prove dependence on Q5? How can one tell without a time machine or omniscience?

On this same issue, Downs attempts to cast doubt on the likely dates of the handwriting, noting "common sense says an older, provincial scribe could well produce the work in 1623." However, the point I made was that the scribe named by Dering in 1623 wasn't provincial and wrote in a lovely, quite remarkable Italic hand. Dering's own hand was a transitional hand, so the point stands that the hands of the manuscript are consistent with men born ca. 1565. Moreover, its total absence of transitional forms indicates a transcription date far more likely to be ca. 1592 than ca. 1623. Just consider the missing question marks. Q5 contains hundreds of *printed* question marks, but D doesn't evidence any. Why not? Even if Hand B was trained not to employ them, as these marks were just entering the language, an inadvertent mistake or two seems natural enough, but there aren't any. Why? As I noted in the monograph, Dawson and Yeandle, writing about transitional hands regarding Walter Bagot's letter of 1622, observed, "though essentially secretary, [it] exhibits a reduction in the exaggerated ascenders and descenders and so shows the writer to be a child of the seventeenth century." There are no such transitional elements in either of D's hands. Indeed, Dawson and Yeandle, writing about Hand B, conceded "the hand is...*pure* secretary." If Downs believes such a hand was writing in Kent in 1623, let him produce extended samples. I was not able to find it while visiting the Maidstone archives. Nor was Yeandle.

With the exception of these forays, Downs, armed with the conviction of a tautology, would avoid all the significant paleographic, literary and proof-mark oddities of D which show its independence from any Quarto by asserting that *only* bibliographic *correspondence* is meaningful. Since bibliographic divergence is, according to Downs, meaningless, how can one win?

Yet the broader view notes how odd it is that D has consistent problems in choosing between letters that are similar when written but grossly different when printed, such as in its persistent confusion of *good, God* and *gold* and words like "on"/"in," "import"/"report" and "for"/"far." Isn't it also odd how D displays, throughout, problems with words and phrases which were clearly resolved in Quarto? Words such as "Francis" and "fire-eyed maid?" Isn't it remarkable that D itself evidences a version of the play which is *much* shorter than what is found in D, i.e., a layer where the final scene was once near f31 and is now 48 pages (24 sheets) away from it? Isn't it odd that D evidences no summaries or bridge lines and that its missing material is simply missing, even though the sense of the text remains intact? Isn't it peculiar that D's various styles indicate that long periods of time elapsed between bouts of transcriptions? Isn't it curious that several sheets of D show that Hand A and B worked together on a sheet. And that Hand B came to the *end* of his materials in the middle of what, in Q5, was a continuous speech and waited for Hand A to supply him with the materials for the verso, as I cited concerning f6r?

Downs, in asserting that Hand A "slavishly followed Q5 in punctuation and spelling [on f1r]," missed the important point about the conjectured "similarities" between Q5 and D's f1r, a point which trivialized the bibliographic argument. First, I pointed out that there are as many differences in punctuation between the two as similarities. Second, I noted that a bibliographic argument simply cannot be made on this issue, since Q1 is essentially the same as Q5 in these matters, i.e., spelling and punctuation. Thus, any similarity between D and Q cannot be assigned to Q5 and may be due to the fact that something like D's f1 stood as copy for Q1, (the chicken and egg problem). Downs' difficulty in understanding this is not unexpected, since he is baffled by what should be easily understood logical paradoxes, ambiguities due to bibliographic limitations. These paradigm problems are similar to those that plagued proponents of Ptolemaic astronomy in defending against Copernican thought.

One should also notice his pejorative "slavishly followed." Indeed, "slavishly" following the spelling and punctuation of Q5 is what would have been expected of a scribe and would have helped proved D's dependence on Q5.

However, take the reverse example. D could be a copy of Q5 even if it were *completely* different than Q5. We might imagine a dyslexic scribe who reversed all the letters in each word, confused the lineation and otherwise failed to copy his text, or an inventive one who wrote another play, line for line. This means proponents of independence cannot simply point to the differences in the text and say, "see, these prove D isn't a transcript." If my essay had a key point it was this: because of its many differences from a printed text, D's status cannot be proved bibliographically. So far, all other evidence indicates that D *preceded* any printed text. Moreover, evidence of transcription from Q5 should have been abundant, but wasn't, so the lack of corroborating evidence is a very significant indication that D was anterior, not posterior, to published versions of the play.

Obviously, if D reflected the style of Q5, the argument would be more difficult. But consider that D might have been pieced together to fill the hole in the First Folio. To do so required collating a copy of Q5 with the original unified source, which existed only as a foul paper. We are told something like this furnished the copy for F's *Othello.* That would place D *inside* the authorial stream even though it might mean the play was transcribed in 1622/3, and, thus, make it significant to modern scholars (even though it relied, in parts, upon Q5). So again, I caution reliance on bibliographic correspondences in a case such as this, where, by any objective tally, there are far more disagreements than correspondences. In conclusion, the correspondence of accidentals cited by Downs is meaningless in such a sea (mare) of differences.

Lest Downs feel that I did not consider his case thoroughly, I would invite him and others to participate in a public debate on this question.