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# The Elizabethan Review

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# What's in a Name?

## Shakespeare, Shake-scene and the Clayton Loan

Diana Price

Over the years, biographers of William Shakespeare have occasionally disagreed on which historical records to accept or reject. For example, Shakespeare's engagement one day to Anne Whately of Temple Grafton and his marriage bond on the next naming Anne Hathaway of Shottery prompted Sidney Lee (1925; 31) to reject the Whateley-Shakespeare license:

The husband of Anne Whateley cannot reasonably be identified with the poet. He was doubtless another of the numerous William Shakespeares who abounded in the diocese of Worcester. E.K. Chambers (Facts and Problems, 1:18) accepted the Whateley-Shakespeare license; he thought it more likely that a blundering clerk had entered the wrong information.

How does the biographer decide what evidence to accept and what evidence to reject? The question is not exclusive to Shakespeare. Scholars continue to disagree as to whether the "John Fletcher of London" admitted to Cambridge in 1591 refers to the dramatist, who would have been 11 or 12 years old at the time. Alexander Dyce (1:8) leaned toward accepting the record on various grounds, noting that "in those days students were admitted into the universities at a very early age." Chambers (Stage, 3:314) rejected the record because the date of 1591 seemed too early to him. In this case, the words "of London" failed to clarify the matter, since the year of Fletcher's move to London is otherwise not known. Nevertheless, here was a record of a "John Fletcher" — complete with first and last name — in a place where a biographer might reasonably have expected to find one and which had been provisionally accepted by a respected nineteenth century scholar, yet Chambers could not be sure that it referred to the same John Fletcher because there was no corroborating information.

Such standards of proof are occasionally suspended by Shakespearean biographers and replaced with something that looks suspiciously like personal bias. Some name-only records are accepted into the biography while others are usually rejected, and the criteria varies. A case in point is the Clayton loan.

In Will Shakspeare and the Dyer's Hand, Alden Brooks (xii-xv) used the Shakespeare-Clayton loan to show that even so venerated a Shakespearean biographer as Chambers has been guilty of bias. The record in question is dated in 1600 and refers to the recovery of a £7 loan made in 1592. Chambers (Facts

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*This article has been adapted from Diana Price's Shakespeare: An Unauthorized Biography.*

and Problems, 2:52) provided the following account:

in 1600 Shakespeare brought an action against John Clayton and recovered a debt of £7. . . . I agree with Lee 321 that there is no ground for identifying the Willelmus Shackspere of this with the dramatist. The debt was acknowledged in Cheapside on 22 May 1592. No local description is given by which the habitation of the plaintiff can be fixed. The defendant was of Willington in Bedfordshire.

Brooks argued that Chambers misled his readers into thinking that he was "following" Lee, whose revised 1925 edition reads:

Shakespeare inherited his father's love of litigation, and stood rigorously by his rights in all his business relations. In March 1600 'William Shackspere' sued John Clayton 'Yeoman,' of Wellington in Bedfordshire, in the Court of Queen's Bench, for the repayment of a debt of 7l. . . . judgment was given for the plaintiff with 20s. costs. There is nothing to identify John Clayton's creditor with the dramatist, nor is it easy to explain why he should have lent money to a Bedfordshire yeoman. It is beyond question however that at Stratford Shakespeare, like many of his fellow-townsmen was a frequent suitor in the local court of record.

Lee went on to recite a number of Shakespeare's litigious activities comparable to, if not specifically corroborative of the Clayton transaction, suggesting that he did not disqualify the loan out of hand. Brooks could have taken his argument one step further; in fact, Lee (1898; 206) initially accepted the Clayton loan and evidently did not entirely change his mind.

It is the basis for acceptance or rejection that is at issue here. The Clayton loan record names "Willelmus Shackspere" as the plaintiff but contains no age, occupation, spouse, hometown, or signature to further identify him. Nevertheless, Lee initially accepted the record as another example of Shakespeare's "love of litigation," while Chambers rejected it for lack of corroborating information. Yet Chambers accepted the 1613 payment to Shakespeare for the Earl of Rutland's impresa, an account that includes no corroborating information either; the impresa record does not even list the payee's first name:

To Mr. Shakspeare in gold about my Lorde's impreso, xliiiij<sup>s</sup>; to Richard Burbage for paynting and making yt, in gold xliiiij<sup>s</sup>.

In this case, the presence of Richard Burbage's name clinched the identification of "Mr. Shakspeare" for Chambers (Facts and Problems, 2:153), who rejected Mrs. Stopes's suggestion that John Shakspeare, the royal bit-maker might be the payee. For Chambers, the juxtaposition of the names Shakespeare and Burbage was apparently sufficient as corroborating evidence, even though the record itself made for a rather awkward fit in Shakespeare's biography, impresa-making being "an activity which is most unfamiliar and perhaps seems unsuitable to modern readers" (Bentley, Handbook, 82).

Chambers also accepted Shakespeare's five London tax records, which are essentially lists of names. Historians conclude from the tax rolls that Shakespeare lodged in Bishopsgate before moving to the liberty of the Clink

in Southwark. The attraction of the tax rolls is obvious: Bishopsgate was convenient to the Shoreditch theaters, and Southwark was convenient to the Bankside theaters. However, lists that recite a place of domicile, i.e. tax rolls, matriculation records (such as the one for Fletcher), marriage registers, birth or death registers, etc., have frequently proved insufficient for purposes of identification. For example, the record of marriage in Southwark (1603) of a "William Eglestone" to an Anne Jacob was insufficient for Chambers (Stage, 2:315) to identify the groom with the actor, yet Southwark was where Ecclestone worked. No one questions Shakespeare's tax records, because they seem consistent with other biographical records, but the Clayton loan is just as consistent with Shakespeare's documented life.

The loan was made in 1592, the same year that Shakespeare made his London debut in Groatsworth of Wit; it was transacted in Cheapside, located north of the Thames to the east of St. Paul's. Bishopsgate is closer to Cheapside than to the Shoreditch theaters, so there is no difficulty reconciling Shakespeare's first presumed London domicile with this transaction. Suit for recovery was filed in the Court of Queen's Bench in 1600, so the plaintiff was in London at that time as well; the tax records corroborate William Shakespeare's presence in Southwark in that year. Moreover, the loan is consistent with the allusion to Shake-scene in Groatsworth and with other records of Shakespeare's money-lending and lawsuits to recover debts.

Many of Shakespeare's business records involve mercenary activities. In 1598, Shakespeare was hoarding commercial quantities of grain during a famine. The 1598 Quiney-Sturley letters detail the correspondents' hopes of securing a sizable loan from Shakespeare. In 1604 Shakespeare sold commercial quantities of malt to Phillip Rogers, loaned him two shillings, and then sued to recover £1-5s, 10d plus damages. In 1608, Shakespeare sued John Addenbroke for a debt of £6 plus damages. Addenbroke skipped town, so Shakespeare proceeded against the man who served as Addenbroke's security against default. In 1611, Shakespeare and two others filed a complaint to protect their real estate interests, petitioning for compensation in the event of default by other lessees and sub-lessees. The complaint was essentially a tactic for collecting outstanding, or potentially outstanding moneys owed. In 1614, Shakespeare's was "conniving" over the pasture enclosures at Welcombe. As Robert Bearman (49-59, 76) has shown, Shakespeare put his own interests before those of his community in the matter of the enclosures, again demonstrating his pre-occupation with protecting and increasing his estate. These records show that Shakespeare was viewed by others as a likely source of loans, providing the terms were right. They also show him to have been a tight-fisted and shrewd businessman with a mean streak, a portrait to keep in mind when considering the charge that Shakespeare was a usurer.

In Elizabethan England, usury was a bad word, and branding Shakespeare a usurer may seem radical. However, the explicit charge is found in the pamphlet Groatsworth of Wit. Following Warren Austin's case in favor of Henry Chettle, I will refer to Chettle as its author.<sup>1</sup>

Groatsworth of Wit is organized into tales about the character Roberto, a contrite repentance, the famous open letter to three playwrights, and an Aesop's-type fable. The dominant themes, remorse over profligate spending, hatred of poverty, and hatred of usury, unify the sections into a cohesive whole. According to Muriel C. Bradbrook (1962; 65), the excerpt "on Shakespeare, the highest point of [the author's] invective, is not detachable from the rest of the pamphlet, in which the poet tells his life-story as the tale of the prodigal Roberto."

At the beginning of the story, we learn that Roberto's father is a bondsman. Roberto has come to resent usurers and the fortunes they make, but his younger brother is following in his father's footsteps. The brother eventually inherits their father's ill-gotten gains, while Roberto inherits only a groat. After several more episodes, Roberto is feeling sorry for himself when an actor interrupts his train of thought. The actor recruits the destitute Roberto to write plays for him, and the tale ends with a repentance, immediately followed by the letter containing the "upstart crow" diatribe.

The scholar Roberto is intended to represent Robert Greene,<sup>2</sup> but there is little agreement as to whom "Roberto's actor" represents. Although D. Allen Carroll saw him as more of a "type" or composite character, others have proposed particular candidates, among them William Shakespeare. For example, A. L. Rowse (60) asked:

who was the player with a provincial accent who gave himself the airs of a gentleman? It was just seven years since the birth of Shakespeare's twins at Stratford. 'For seven years . . . absolute interpreter of the puppets' would seem to indicate an apprenticeship of that duration at acting.

Samuel Schoenbaum (150-1) acknowledged Rowse's suggestion "that the unnamed player with gentlemanly airs is Shakespeare" but deflected the passage as a specific satire of Greene and Shakespeare by citing factual discrepancies, noting that "Greene, clearly represented as junior, was actually six years older than Shakespeare." Judith Cook (40) took the other side and supposed that Roberto's actor "is again taken to refer to Shakespeare." Many biographers take no side and simply ignore the fable, turning their attention instead to the letter to three playwrights.

Nearly every Shakespearean biographer quotes a passage from the "upstart crow" letter, but almost all of them cut out the charge of usury. When missing pieces from the letter are reinstated, the salvo leveled at Shake-scene turns out to be an attack against an actor who is a money-lender and, like Roberto's actor, a paymaster of playwrights. The letter was intended to be read immediately after Roberto's tale, and when it is, the common elements between the two sections become obvious.

In his thumbnail account of Robert Greene, Chambers (Stage, 3:324) summarized the pamphlet as a biographical narrative, but only as far as Roberto's tale: "His adoption of his profession seems to be described in The Groatworth of Wit. Roberto meets a player, goes with him, and soon becomes 'famozed for an arch-plaimaking poet'." But the open letter to playwrights carries the tale and the characterization a little further. This upstart actor hired Greene to write plays, loaned him money, and then left him to die in poverty.

The language in the open letter alternates between the singular and the plural, a technique often used to blur satiric material (as Ben Jonson wrote in Timber, "where censure is general, there is no injury to individuals"); it shifts at the phrase "Yes trust them not" and again at "let those Apes" [spelling modernized]:

And thou no less deserving than the other two, in some things rarer, in nothing inferior; driven (as myself) to extreme shifts, a little have I to say to thee: and were it not an idolatrous oath, I would swear by sweet St. George, thou art unworthy better hap, since thou dependest on so mean a stay. Base-minded men all three of you, if by my misery you be not warned: for unto none of you (like me) sought



those burrs to cleave: those Puppets (I mean) that speak from our mouths, those Anticks garnished in our colors. Is it not strange, that I, to whom they all have been beholden: is it not like that you, to whom they all have been beholden, shall (were ye in that case as I am now) be both at once of them forsaken? Yes trust them not: for there is an upstart Crow, beautified with our feathers, that with his Tiger's heart wrapped in a Player's hide, supposes he is as well able to bombast out a blank verse as the best of you: and being an absolute Johannes factotum, is in his own conceit the only Shake-scene in a country. O that I might entreat your rare wits to be employed in more profitable courses: and let those Apes imitate your past excellence, and never more acquaint them with your admired inventions.

Most biographers cut the quote here, if not earlier, but the next sentence reads:

I know the best husband of you all will never prove an Usurer, and the kindest of them all will never prove a kind nurse: yet while you may, seek you better Masters; for it is pity men of such rare wits, should be subject to the pleasure of such rude grooms.

While Chettle's language is not obscure, it may be helpful to offer a paraphrase that irons out the shifts from singular to plural:

You [playwright #3] are no less talented than the other two play wrights. You have been impoverished, as I have, but you don't deserve any better luck than I if you rely on such a despicable prop to support you. Contemptible fellows, all three of you, if you don't learn from my misfortune. The actors are only as good as our words make them, and they owe me. They owe you too, but since I have been deserted by them, (by one in particular) in my time of need, beware. Beware of one untrustworthy actor, the "upstart Crow."<sup>3</sup> We make him look good in the roles we write, but this player is callous and duplicitous. He fancies himself able to extemporize lines in blank verse that are as good as any of yours; he even passes off some of your material as his own. And this conceited know-it-all thinks he's the only "Shake-scene" actor in the country. I beg all three of you talented playwrights to re-direct your skills in a more profitable direction, and away from this unscrupulous actor. Let him recite or plagiarize your past plays. Don't give him any new ones. I know that the most financially prudent [most frugal manager of finances] of you would not stoop to usury (i.e., as did Shake-scene), and even the most compassionate usurer is not charitable at all to someone driven to desperation, on his deathbed and needing care. So while you still have a chance to escape my fate, find some paymasters with more integrity. Stay away from actor-paymasters (and usurers like Johannes Factotum), because you three are too talented to be exploited by such contemptuous knaves.

The reinstated passage completes one unbroken paragraph that lambastes Shake-scene not as a budding playwright, but as an actor, paymaster, and money-lender who deserted a writer in his extremity. This Shake-scene is a "Usurer," Greene's erstwhile Master, and now a "Master" for whom the other playwrights work. The letter urged the playwrights to find a more trustworthy "Master":

thou dependest on so mean a stay . . . if by my misery you be not warned . . . trust them not . . . there is an upstart crow . . . never more acquaint them [him] with your admired inventions . . . the best husband of you will never prove Usurer . . . seek you better Masters.

Most biographers interpret the diatribe as a warning to the established playwrights about a novice who presumes to compete with them. But if the passage was a warning about a newcomer who could write better than they, why would the passage advise them to stop writing plays themselves (“never more acquaint them with your admired inventions”)? The “admired inventions” aren’t the problem; the “Puppets” (i.e. one Puppet in particular) are the problem. The passage is telling the three addressees to write for “better masters” and to stop writing for the one with a “tiger’s heart.”

A few critics have argued that Chettle was warning the writers to stop providing plays to ungrateful actors and turn their talents instead to other types of writing. Such an interpretation can work if one ignores the usury reference, the shifts between plural to singular that confirm this outburst as a personal attack, and the two fables on either side of the letter that deliver the same Shake-scene character. Taken in that context, the open letter is not a treatise calling for the liberation of playwrights from their dependency on actors. It is a personal attack, and the epithet “Shake-scene” identifies one of the pamphlet’s principal targets: William Shakespeare. While Schoenbaum (184, 151) acknowledged that “most Elizabethan dramatists . . . eked out their precarious livelihoods — vide Greene — as employees of the players,” he too edited the “upstart crow” letter to avoid revealing that Shakespeare was resented as their disreputable paymaster. The paymaster-cum-usurer is incompatible with the “gentle” Shakespeare of legend, so the “upstart crow” passage has been trimmed to fit. Even the brutal allusion to Shake-scene’s “tiger’s heart” is used only to point to the paraphrase of Henry VI (3), not to a callous and untrustworthy Shake-scene. Yet the tightwad with a mean streak returns in the next fable.

The allegory following the “upstart crow” letter is about a wasteful Grasshopper and a frugal Ant. Bradbrook (1962; 67) described the fable as a “farical afterpiece to his tragic story, [in which] the improvident Poet appears as the Grasshopper, while the provident Ant represents the Player, who refuses succour in time of need.” The Grasshopper, like Greene, dies an impoverished death, and E.A.J. Honigmann (4-6) built a solid case for Shakespeare as the Ant, resented for his business acumen and profiteering:

Is Aesop’s ant a greedy miser, whose thrift is theft? Is it said to work others woe? These surprising charges pick up the very accusations levelled against ‘Shake-scene’. . . . like Shake-scene, it has a tiger’s heart.

Both tales about Roberto and the miserly Ant reinforce and extend the portrait of Shake-scene, and the repeated accusations in all three sections lead to the equation: Roberto’s actor = paymaster = rude groom (painted monster, so mean a stay, Antick, peasant, etc.) = upstart crow = Johannes factotum = Shake-scene = usurer = greedy Ant.

Honigmann (ix) probably did not endear himself to his colleagues by pointing out that “crucial passages from the records have been misread, or have been ignored because they clashed with preconceived ideas.” He certainly broke with tradition when he proposed that (7-8):

in the period 1590-94, when the London acting companies were re-grouping and a new financial strategy was called for, not only Henslowe but others may have emerged as theatrical bankers or paymasters, one for each company; one other, as the far-sighted reader has guessed, being William Shakespeare.

Honigmann (12) further deduced that "Shakespeare's money-lending could have begun as early as 1592" with the £7 loan to Mr. Clayton. But most biographers seem unwilling to follow the trails wherever they may lead, in this case to a usurer.

The Clayton loan not only corroborates the unexpurgated Groatsworth but also adds to what we know about Shakespeare. If Chettle expected some readers of Groatsworth to comprehend a charge of usury against "Shakespeare," it follows that Shakespeare's activities as a loan shark had to be known to some of them by 1592. The Clayton record tells us that Shakespeare had sufficient liquid capital in 1592 to lend £7, and a loan transacted in Cheapside was a commercial loan. According to William Ingram (41), "after 1571 the overt bonding of borrower to lender became a flourishing business in London, and nowhere more than at the church of St. Mary le Bow. . . . facilities for engrossing documents and for administering oaths, and its central location in Cheapside, soon made Bow Church the obligatory place for borrower and lender to ratify their agreements." It would be surprising if a transaction of this magnitude marked Shakespeare's debut as a money-lender, so it raises questions about what he was doing during the so-called "lost years." At the least, it suggests he had already established a lucrative side-line lending money. It is even possible that Shakespeare picked up some tricks of the money-lending trade from his father before he ever left Stratford. Shakespeare's evident accumulation of working capital may also shed light on his sudden appearance as a payee for the Lord Chamberlain's Men in 1595. Payees were invariably shareholders, and shareholders generally provided investment capital (Bentley, Profession of Player, 29-32).

Most biographers evidently do not consider it seemly for a genius playwright, even a shareholding playwright, to moonlight as a usurer. Yet biographers know full well that during his lifetime, Shakespeare turned up in the company of two other usurers. Francis Langley was one, although his money-lending is frequently downplayed in Shakespearean biographies. Langley is more often introduced as the proprietor of the Swan Theater than as a disreputable usurer. In 1596, he and Shakespeare were accused of assaulting William Wayte in Southwark, who petitioned for surety of the peace (another name-only record). Much ink has been spilt to purge Shakespeare of the onus of assault, and many critics have pushed the blame onto Langley and his previous known skirmishes with William Gardiner and Gardiner's stepson, Wayte. Nevertheless, the writ of attachment finds Shakespeare keeping bad company and being accused of ungentle behavior. John Combe was the other usurer with whom Shakespeare was associated; Shakespeare bought real estate from Combe and his uncle in 1602. Combe made his fortune from usury, and according to the Public Records Office (Thomas, 23)

was the richest man in Stratford and well known as a money lender . . . . In fact the two men must have been close friends as Combe left Shakespeare £5 in his will and the playwright left Combe's nephew, Thomas, his sword.

Biographers can trace few personal relationships in the life of Shakespeare, but Combe is certainly one of them.<sup>4</sup>

All these pieces hang together, but they deliver a rather unflattering character. This emerging portrait of Shakespeare resonates with any number of satirical allusions, e.g. Sogliardo, Jonson's epigrammatic Poet-Ape, and various characters in the Parnassus trilogy that have been introduced piecemeal in various biographies. However, B. Roland Lewis (2:335) compared Shakespeare's financial and social ambitions with two less well-known allusions; the first is to money-lending actors in Henry Crosse's Vertues Common-Wealth (1603):

these copper-lace gentlemen [meaning, of course, actors and dramatists] growe rich purchase lands by adulterous plays and not [a] fewe of them usurers and extortioners which they exhaust out of the purses of their haunTERS so are theye are puft up in such pride as selfe-love as the envie their equalles and scorne theyr inferiours.

Chambers reprinted more of the passage, albeit without comment, in Elizabethan Stage (4:247):

... it were further to be wished, that those admired wittes of this age, Tragædians, and Comædians, that garnish Theaters with their inuentions, would spend their wittes in more profitable studies, and leaue off to maintaine those Anticks, and Puppets, that speake out of their mouthes: for it is pittie such noble gifts, shold be so basely employed, as to prostitute their ingenious labours to inriche such buckorome gentlemen.

Crosse was obviously plagiarizing Groatsworth, but the passage shows that he picked up on the connection between usurers and Puppets. The second allusion is from Thomas Dekker's News from Hell (1606), in which Lewis found a greedy shareholder:

Manie players swarm there [in hell] as they do here, whose occupation being smelt out by the Cacodemon, or head-officer of that country, to be lucrative, he purposes to make up a company and be chief sharer himself.

These thematically related allusions were introduced by Lewis as analogous to Shakespeare's financial and professional progress, but they have rarely been considered for inclusion in Shakespeare's personal portfolio. While many in the theater profession may have had sidelines as money-lenders, there are no other known actor-sharers who also qualified as landholders and who were specifically resented, even despised in print for the greedy methods they used to acquire their wealth. Certainly Edward Alleyn can be eliminated from the running; he was consistently respected in print and his financial records attest to his generosity. We also know, for example, that actor Nicholas Tooley loaned money to his fellows John Underwood and William Ecclestone but forgave both debts in his will, a gesture that suggests friendly lending, not usury. Compare Tooley's provision to one in John Combe's will, which specified that money be lent to needy tradesmen at interest, such interest to benefit the poor. Combe continued to lend money from beyond the grave.

The two allusions that Lewis cited may belong to the same family as

the restored passages from Groatsworth and the Clayton loan. If it seems a stretch to consider such allusions as pointing to Shakespeare, it may be because most readers have been conditioned to think only of “gentle Will.” Yet the ungentle character sketch is consistent with Shakespeare’s undisputed historical records.

Groatsworth was published in 1592; the Clayton loan was made in 1592. Most biographers omit both the loan to Clayton and the reference to usury in Groatsworth, even though the two records reinforce each other. Bentley, Bradbrook, Ivor Brown, Chambers, Marchette Chute, F. E. Halliday, Schoenbaum, Rowse, Peter Thomson, and Ian Wilson omit or reject the Clayton loan; Honigmann and Peter Quennell accept it. Bentley, Bradbrook, Brown, Chute, Halliday, Lee, Quennell, Rowse, Schoenbaum, Thomson and Wilson delete the reference to usury in Groatsworth. Even Chambers, who reproduced the entire open letter for reference in the second volume of his biography commented only on the usual truncated excerpt in volume one. Thomson (17, 35), however, duly noted that Honigmann had “argued that Shake-scene’s real offence is not authorship but money-lending,” and supposed himself that Shakespeare “went in for [usury] on a small scale.” But he missed any connection with the Clayton loan.

In 1949, Leslie Hotson (229-30) tried to drive the final nail into the Clayton loan coffin. He discovered another William Shakespeare, a farmer who resided eight miles south of Clayton’s hometown of Willington and concluded that this Shakespeare was Clayton’s money-lender. However, Hotson did not cite any evidence to show that this Bedfordshire Shakespeare ever loaned any other money, as did Shakespeare of Stratford, or was in London in 1592 and 1600, as was Shakespeare of Stratford. Nor did Hotson postulate why a loan would have been transacted in London rather than in Bedfordshire. However, after establishing the mere existence of a William Shakespeare of Bedfordshire, Mr. Hotson viewed the “question as settled.” Hotson’s discovery hardly settles anything, because it too lacks any corroborating information.

While the Clayton loan sits quite comfortably alongside Shakespeare’s other business records, it has a major downside. If accepted, Shakespeare’s first documented activity in London was money-lending, and that is not a very glamorous entrance for an aspiring poet. Alden Brooks supposed that Chambers rejected the Clayton record because of its negative impact on Shakespeare’s London debut, but equally problematic is the recognition of the reference to usury in Groatsworth. That accusation seriously undermines the traditional interpretation of the “upstart crow” letter and shows that resentment was registered, not against an arrogant writer but against an unscrupulous actor-paymaster and usurer. It may also be difficult to imagine how Shakespeare could have acquired sufficient liquid assets by 1592 to make a £7 loan, a not inconsequential sum in a day when playwrights earned an average £5 to £7 per play. Yet Shakespeare’s steady accumulation of wealth is one of the few sure things biographers know about his life.

How do biographers justify accepting tax records, the *impresa* record, or even the 1596 writ of attachment while rejecting the Clayton loan? Why do they snip out the Groatsworth allusion to usury? It is difficult to avoid the conclusion that most biographers would prefer not to deal with certain troubling records. By failing to introduce and then connect similar and reinforcing records, biographers deflect attention from an otherwise obvious, if unflattering pattern. By minimizing or editing uncomplimentary information, they have sanitized the portrait of Shakespeare for their readers. Although most biographers silently manipulate the information, Anthony Burgess (259)

flatly admitted his philosophy: "Let us try to keep Will likable."

In conclusion, there are good reasons to accept the Clayton loan into Shakespeare's biography and no good reasons to reject it. Unsavory it may be, but it is compatible with other unsavory and undisputed records that cumulatively point to a mean streak in Shakespeare. The Clayton loan may contradict the "gentle" Shakespeare of tradition, but it leads us toward a more coherent portrait of the man from Stratford.

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## Notes

<sup>1</sup> D. Allen Carroll (x, 24-7) acknowledged Austin's research and concluded that while "Greene may have had something to do with the writing of Groatsworth, Chettle certainly did."

<sup>2</sup> Toward the end of Roberto's Tale, we read: "Here (Gentlemen) break I off Roberto's speech; whose life in most parts agreeing with mine, found one self punishment as I have done. Hereafter suppose me the said Roberto."

<sup>3</sup> Æsop's crow was a proud strutter who borrowed the feathers of others, while Horace's crow was a plagiarist. There is no consensus on which crow was intended, but many have seen the "upstart crow" as a conflation of the two.

<sup>4</sup> The friendship between Shakespeare and Combe was the subject of posthumous gossip and legends, as was the story that Shakespeare had composed an epitaph for Combe (see Chambers, Facts and Problems, 2:242-3, 246, 250-1, 253, 268-9). In 1681, John Aubrey reported the following version of the extempore doggerel:

Ten in the Hundred the Devil allowed  
But Combes will have twelve, he swears & vows:  
If any one asks who lied in the Tombe:  
Hoh! quoth the Devill, 'Tis my John o'Combe.

In 1709, biographer Nicholas Rowe described the friendship between Shakespeare and Combe as "a particular Intimacy." While none of these reports are reliable, there may be a grain of truth in them because none of these early writers knew about Combe's or Shakespeare's will bequests (Honigmann, 13). In addition, these legends tell us that Shakespeare, like Shake-scene, could bombast out a verse.

# Found: Shakespeare's Manuscript of *Henry IV*

John Baker

Most scholars remain in the dark about Shakespeare's manuscripts, believing none survive.<sup>1</sup> If one surfaced, it would quickly become one of the world's more sought-after documents. Surprisingly, such a manuscript exists.<sup>2</sup> Its provenance and content are certain.<sup>3</sup> It is a unique exemplar of the action of both parts of Henry IV in a single five-act play, wherein 90% of Part One and 30% of Part Two may be found. Long on history and short on Falstaff, it was published by George Williams and G. Blakemore Evans as a facsimile edition in 1974 titled William Shakespeare The History of King Henry the Fourth as revised by Sir Edward Dering, Bart.<sup>4</sup> The facsimile bases itself on Folger ms. V.b. 34, hereafter called D. D remains a 55-page, ledger-sized manuscript to which a "scrap" has been added; it is written, with the exception of the latter piece, in two unidentified Elizabethan hands. The scrap seems to be in an early Jacobean hand, likely Sir Edward Dering's, in whose collection the manuscript was first discovered.

As to what D represents, there are three possibilities. D may be:

- 1) a "lost" source play by Marlowe, c. 1590 as suggested by Louis Ule;
- 2) Shakespeare's earlier pre-quarto version, c. 1596, as argued by Hardin Craig;
- 3) a dependent period condensation transcribed from quartos, c. 1613-1622, as maintained by Hemingway and, later, by Evans, Williams and others.

This paper purports to show that bibliographic, paleographic and literary evidence favors the early pre-publication and authorial nature of D, as argued first by Craig and later by Ule. This study also will attempt to confirm that the case for independence, which conflicts with theories on how Shakespeare wrote (and, thus, perhaps who he was) has long been suppressed by questionable means.

## Background

D was discovered by Halliwell-Phillips in 1845 among papers and playbooks collected by Sir Edward Dering (1598-1644).<sup>5</sup> Dering lived outside Pluckley, Kent in his manor, Surrenden Hall, which contained an extraordinary library of plays, playbooks and manuscripts. The manuscript contains two unidentified Elizabethan hands - Hand A and Hand B - both bearing evidence of having been written in an unmixed Elizabethan Secretary script. They display no sign of the transitional or italic styles, except for speakers' names and stage directions, which were lettered individually.<sup>6</sup> Authorities generally

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date such hands to the sixteenth century.<sup>7</sup> Hand A appears on flr.<sup>8</sup> Hand B surfaces on its verso (flv) and was responsible for all successive pages. The “scrap,” which may or may not be “Hand A’s,” is in a hand somewhat similar to Sir Edward Dering’s and may be thought his. It is possible some of D’s glosses are in Sir Dering’s hand, as first suggested by Halliwell-Phillips.<sup>9</sup> However, paleographic arguments regarding the actual identity of various hands have not proved trustworthy. Indeed, handwriting still cannot be employed for the purposes of identification, unlike finger prints. The paleographic case for the authenticity of the manuscript, however, remains strong, as do its paleographic features, which can be used to help establish the manuscript’s provenance as either an authorial paper or transcript of printed materials. About these properties (overwriting, corrections, confusions, variations of “style” and lengthy hiatuses) there can be no doubt.

Halliwell-Phillips published his 1845 edition of D without taking a position on what it represented.<sup>10</sup> He hoped the manuscript would “meet with attention from those who have made the text of Shakespeare and the history of his writings a matter of study.”<sup>11</sup> Moreover, he added, “we cannot conceal our anxious hope” for such professional notice.<sup>12</sup> Instead, D has been consigned to relative obscurity by those who maintain that Dering simply condensed the two-part original.<sup>13</sup> While Halliwell-Phillips refrained from attributing the manuscript directly to Shakespeare, he was certainly never a proponent of the dependence camp:

indeed the variations, in almost every respect, are so numerous, that we can hardly believe the MS. was transcribed from any corrected printed source. At all events, we cannot discover any which contains them.<sup>14</sup>

Halliwell-Phillips also noted the professional nature of Hand B:

when we consider that it is the work of a professed scribe, this alone is sufficient to account for mere clerical errors, which after all, testify to the integrity of the text; and it is most unlikely such a person would have introduced so many variations on his own authority.<sup>15</sup>

Despite claims to the contrary, Halliwell-Phillips’s opinion stands totally at odds with the dependence consensus. Indeed, he took the trouble to point out sundry superior readings in D which have no printed source, such as D’s opening, “To chase these pagans from those holy fields” rather than Q’s “in” them, which is obviously wrong. Along with this D reads correctly, “shallow jesters and rash brain’d wits,” rather than the confusing “rash bavin wits,” as found in the printed sources. He also noticed that Poins’s reply to Falstaff’s query, “call you him?” is, in the manuscript, given correctly as “Owen Glendower,” whereas in the quarto it is “O, Glendower.” Since Falstaff answers “Owen, Owen; the same,” Falstaff must have heard Poins say “Owen Glendower” not Q’s “O, Glendower.” Thus, D is superior to the printed text.<sup>16</sup>

One must ask why D fell from professional attention for nearly a century. Perhaps it was because the Collier debacle, which soon enveloped both Phillips and Collier, caused professionals to be wary of newly discovered manuscripts supposedly emanating from the period.<sup>17</sup> More likely, it was a combination of factors, the primary one being the rise of the biographic construct of the “rustic Shakespeare” who never produced fair copies or revised his works.

The dependence theory was first advanced by Hemingway (1936), and afterwards by Evans, Williams, Lennam, and Yeandle. The consensus for dependence rests on the theory that Shakespeare's plays were written blotlessly and required or received no revision. The dependence hypothesis ignores both methodical revision and an increasing sophistication in Shakespeare's texts. The consensus view explains all improvements to Shakespeare's quartos, whether by subsequent quarto editions or appearances in the First Folio, as resulting from collation with a manuscript more authorial than the one which produced the "less perfect copy," rather than by his own improvement or revision to earlier versions.<sup>18</sup> By contending that Shakespeare did not revise his work, Hemingway and his followers theorize that the author's "simple genius" allowed him to write without customary, let alone sophisticated, methods.<sup>19</sup> This led to the notion that Shakespeare's plays were pirated as the earlier and less sophisticated versions, represented by numerous quarto editions, cannot have been drafts.<sup>20</sup> D must therefore be an enigmatic pirated version, purloined from the quartos. This dependence of D on printed materials was proven by bibliographic arguments given more weight in a previous generation, and upon equally flawed paleographic analyses - which ignored the 16th century nature of D's hands while maintaining that Hand A was Sir Edward Dering's 17th century hand.

The case for D's originality is straightforward: it holds that bibliographic dependence cannot be based on the correspondence of accidentals. It embraces common knowledge as to how writers improve works through a process of revision and on how cuts and abridgements are made. It understands, for example, that a 6,148 line play cannot be cut back to 3,401 lines without requiring "bridge lines" or displaying embedded materials that refer to deleted episodes which may have been overlooked during the cutting. It is also bolstered by paleographic analysis demonstrating that transcriptions of foul papers are unlike those found in cursive transcripts of printed materials, and that 17th century Jacobean hands differ from 16th century Elizabethan hands. However, these realities have not carried much weight with adherents of Hemingway's hypothesis perhaps because the nature of the manuscript implies that the author was a scholar who operated a scriptorium wherein he and staff methodically proofread and revised his works.

Support for D's independence may be found among scholars who understand the merit of Shakespeare's text and at what price such finely wrought lines were purchased.<sup>21</sup> Indeed, A. W. Pollard's argument that Shakespeare routinely produced fair copies of his works embraces aspects of this view, but was rejected by W.W. Greg at about the same time Craig first noticed the authorial nature of the manuscript.<sup>22</sup> According to Greg, however, Pollard's thesis lacked

a connecting link: either evidence that Shakespeare sometimes made fair copies of his plays or that foul papers normally became prompt-books. Without it Pollard's case fails....<sup>23</sup>

Greg obviously believed Shakespeare produced complete plays on his first or second attempt, that is, a foul paper followed, perhaps, by a document similar to the rough playbook of Sir Thomas More.<sup>24</sup> These scholars, who believe Shakespeare never revised his own works, also theorize he never produced anything as good as D; so, according to the dependence thesis, D must, *prima facie*, rely on printed materials. Unfortunately, Pollard did not know of D. His

argument was rooted in the First Folio's claim that Shakespeare routinely produced authorial fair copies.<sup>25</sup> Obviously, if D has an authorial provenance, biographic and bibliographic suppositions regarding Shakespeare will require revision. In any event, since the case hasn't been proven, what should be front-page news has escaped general notice.<sup>26</sup>

### Toward Establishing a Hypothetical Date

Can D be dated? An overlooked topical allusion in Henry IV, Part II testifies to its date of composition as 1594/5, or several years earlier than commonly thought.<sup>27</sup>

Prince [to Poin]: Saturn and Venus this year in conjunction!

What says th' almanac to that?

Poin: And look whether the fiery Trigon, his man,  
be not lipping to his master's old tables.... (II.4.261-5)

Following Dr. Johnson, who stipulated that such conjunctions do not occur, modern scholars do not accept this as being a topical allusion.<sup>28</sup> However, two powerful astronomy computer programs, Distant Suns and Dance of the Planets, show this conjunction<sup>29</sup> taking place in Leo, a constellation in the fiery Trigon, over London on 6/16 September 1595.<sup>30</sup> Assuming the versions to be sequential, this means Part Two must have been written late in 1594 (English date) or early in 1595 (European date), since it suggests an upcoming conjunction.<sup>31</sup> Part One must therefore have been on the boards in 1594, a year earlier, while a unified version, like D, could easily date to 1593. Since the hands, paper and ink are Elizabethan, the 1593 date remains consistent with the paleographic evidence.<sup>32</sup>

### Why the Dependency Case Fails

The case for dependence was said to be solely bibliographic.<sup>33</sup> It argued that correspondence to bibliographic errata would show that D was a transcript and also suggest 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 Q 5. The case was then claimed closed. Was it?

### The Missing Pronoun and Misplaced Apostrophe

D does not include Q's pronoun "I" on f28r in the line: "weare here: would cudgell him like a dog: if he would saie so:." Since D is missing much of Q, a deletion seems hardly surprising. The editors, however, maintain "the scribe, recognizing an omission, left a small blank space for Dering."<sup>34</sup> They offer no explanation how a scribe, who had never seen an edition of Q containing the pronoun, would have noticed the omission, the line scanning without it. Nor do they explain why Dering did not fill in the space with his own "I," as Hand A had done on f9r, where no space was provided. Lastly, they offer no suggestion as to why the scribe didn't simply insert the pronoun, since he freely corrected his own transcription. Perhaps there wasn't any space. The editors qualified the space as being "small." It is, since the trailing serif of the "e" from "here:" extends into most of this interval:

weare here:—would cudgell him like a dog

By measurement it is no different from dozens of other paleographic gaps in the manuscript caused by similar serifs which the editors ignore (see f44v, “people:—saying that”). For these reasons, the missing pronoun must be considered accidental.

As to the “misplaced apostrophe,” Williams and Evans locate it at D’s 4.2.76 (Globe V.ii.76), i.e., f37r. However, the typescript of the Folger editors diverges from the Elizabethan hand. They print six more capital letters than D shows yet don’t print four capital letters which D does. They typeset “curtesie:” for D’s “Caurtesie”, fail to print seven commas, and they add a full colon where D shows nothing. Even with “corrections” D isn’t identical to Q5, and without them, radically dissimilar. Little if any of this is mentioned, but they fall on the Elizabethan apostrophe, which may or may not appear above “fellows”,

and fellows:, souldiers:, friends:,

However, to know that Hand B had copied Q’s misplaced apostrophe into D, one would have to know that Hand B was consistent in his placement elsewhere. D proves he wasn’t. Indeed, the editors concede they

modernized...lowered superscripts and expanded abbreviations and contractions; the thorn we have printed as “th.” We have capitalized the initial letter of each line, and of speeches. We have supplied the scribe’s standard colon at the ends of speeches and of speech headings where it has been omitted; the scribe’s internal period and his common “:,” we have printed as a colon. We have regularized the placing of the apostrophe in contractions. [Emphasis added.] We have set in italic type...we have corrected the scribe’s frequent minim errors where his intention was entirely clear. All of these changes have been made silently and without record. [Emphasis added.]<sup>35</sup>

Consequently, if one follows D, one discovers apostrophes in literally hundreds of places not to be found in Q. So why focus on this one? Coincidentally, in dealing with a selection from D, Dawson and Yeandle noted a similar mistake in B’s placement of the apostrophe:

there’s] Though he misplaced it, we may assume that the writer [Hand B] intended the apostrophe to go in the right place. The modern use of the apostrophe in contractions of this sort begins to be met with in the first decades of the seventeenth century.<sup>36</sup>

This is the conclusive evidence offered for dependence: a scribe with habits so irregular he was corrected “silently and without record”. This silent correcting, along with the editors’ conviction that D was a transcript of printed materials, also tempted them to “correct” their typescript by restoring many other readings from Q; so much so, their typescript often more closely parallels Q than D. This editorial activity seriously undermines the scholarly value of their typescript.

Along this line, an inapt phrase of theirs has led readers to believe the editors had cited Hardin Craig in support of their thesis, rather than in opposition to it.

The proposition that the Dering manuscript reflects a single, five-act, Shakespearean play on Henry IV earlier than the two printed parts

now extant is not tenable; the Dering version is not anterior to the printed texts but derivative of them and hence posterior to them. See Hardin Craig....<sup>37</sup>

Most would take this footnote to imply that Craig had reviewed earlier propositions regarding the anterior nature of the manuscript and found them "not tenable." In fact, this turns out to be the 1954 landmark article on the Dering Manuscript in which Craig realizes that the manuscript is the original version of the play. Craig never deviated from this opinion. He returned to it more forcefully in his A New Look at Shakespeare's Quartos (1961), where he calls the dependency thesis, "stupid," "unnecessary," "incredible," and "impossible."<sup>38</sup> Evans and William ignore Craig's longer study. Indeed T.N.S. Lennam, writing in the Folger Library's Shakespeare Quarterly (mentioning the work of Evans and cited by Evans in the Facsimile Edition) also misquotes Craig and seems to believe that Craig thought D was dependent on printed materials. Lennam writes, Dering's "interest in the theater is reflected in...an adaptation of 1 and 2 Henry IV," after which he cites Craig as proof of this notion!<sup>39</sup> In subsequent works on Henry IV, both Eleanor Prosser and David Bevington circumvent Craig, believing D to be a secondary source, as had Louis B. Wright.<sup>40</sup> It seems safe to say that a generation has matured either not knowing of D, or have been misled into believing that D lies outside the authorial stream.

Craig, who knew of Evans' dependency hypothesis, wrote:

it is incredible that any scholar who has had to do with 1 Henry IV and, especially, 2 Henry IV and who has read the Dering play could fail to see evidence that the Dering play is a single drama antedating the two-part play and representing as it stands Shakespeare's treatment of the wild life of Prince Hal, his reformation, his winning of glory, and his rejection of Falstaff.... Of course these things could not be if the Dering version were based, as it was thought to be, on the fifth quarto of 1 Henry IV.<sup>41</sup>

Craig added that the notion that the Dering version is abridged or condensed "would be impossible, beside being stupid and unnecessary."<sup>42</sup> Writing the following year in the introduction to Hamlet: The First Quarto, Craig confesses he had grown "weary of arguing against error."<sup>43</sup> Craig's detractors, however, hadn't, so new errors continued to surface. Indeed, while the Folger scholars were moving to squelch Craig, making his university life difficult, their director, Louis B. Wright, wrote that any scholar who offered new evidence about the authorship question would

assure himself of an undying reputation, he would be certain of translation to the best university post in the land; and his book would pour in royalties.<sup>44</sup>

Though not, it seems, without first being maligned and ignored until decades after his death. Craig addressed this danger to his professional reputation in his introduction to Albert B. Weiner's edition of the Q1 Hamlet, which offered extensive proof that popular theories regarding its pirated or derivative nature were mistaken:

We are in agreement as regards scholarly approach to the well known

problem presented by...earlier and less perfect version[s of Shakespeare's texts], since we are both disposed to rely on such records and bare facts as exist rather than on the brilliant theories of certain great Shakespeare scholars that are now widely accepted. Our approach is inductive rather than deductive. We both try to make the facts speak for themselves; whereas the scholars preferred to rely on ingenious theories. This puts us both in danger, [~as] in a sense, we are both responsible for published disagreement with great scholars whose standing as scholars and whose contributions to our knowledge of Shakespeare we both profoundly respect.<sup>45</sup>

One cannot help but note that, regardless how benign the origins of paradigm blindness, the consequences are dangerous business indeed.

The editors also asserted a "fidelity" in punctuation on f1r to Q5.<sup>46</sup> The first problem here is that Q5 is indistinguishable, bibliographically, from Q1 in matters of punctuation, particularly on its first page. This means the proof becomes circular, as there is no way to relate the punctuation of f1r to the accumulated errata of any specific quarto. Worse, by actual count there are 38 punctuation conventions on f1r whereas only 17 can be found in Q. Since D's f1r differs from Q in title, characters, lines and content, and is more "compositional" than other sheets, its fidelity to Q5 seems far too tenuous for a case of this magnitude. What the editors may have meant to say is that f1r resembles the style of punctuation followed in Q more than other sheets. This would have been a true statement. However, since this sheet is in Hand A's, the difference in punctuation may be idiosyncratic. Or f1v's descendant may have been used as the style sheet for Q.

Another important and somewhat curious feature of f1 is that Hand A transcribed all of its recto, while Hand B transcribed the verso. In order to explain this paleographic feature, the editors have suggested that Hand A "after page one [i.e. f1v]... gave the chore of transcription over to a scribe," i.e., Hand B.<sup>47</sup> This seems unlikely, particularly since such devices are known to paleographers as a means of "interlocking," or identifying an unknown hand with that of an author's.<sup>48</sup> This, and not Hand A's conjectured lack of stamina, is likely to account for the two hands on f1.

### Problems Concerning the Copy Text for Part Two

As to Part Two, Williams' and Evans' claim fails outright. Here the editors have asserted that the fidelity or correspondence is to the Second Issue of Q. What they've overlooked is that the Second Issue of Q wasn't bibliographically distinct from the First Issue, except locally as regards the "bedroom scene," which was somehow omitted. Since there was only one edition, there are no bibliographic possibilities for the entrance of accumulated errata.<sup>49</sup> Any correspondence between D and the printed version of Part Two may thus arise from the fact that D preceded it.

Incidentally, D is so radically different from Part Two the editors theorized that Dering provided Hand B with his own transcript—implying that most of it isn't a transcript of Q but a copy of a transcript. Yet these same editors had earlier discounted this notion, saying,

he began to transcribe the play himself, adapting the text from the quarto as he went along, but after page I he decided to make his revisions on the pages of the quarto directly, and he gave the core of transcription over to a scribe.<sup>50</sup>

With all this in mind, the editors' case for dependence can be safely set aside as unproven.

Scholars have, however, two plays. How can one determine which came first? Since D is shorter, unified, and isn't printed, assumptions that D came first are now obvious and, with dependence unproven, superior. At this point the case for D's primacy becomes tacit and can be established by straightforward paleographic and literary means, were it not for several points raised by proponents of dependence.

### **A Consideration of Dering's Order for a Copy of Henry IV**

While the editors have asserted that their case for D's status is entirely bibliographic, they have, nonetheless, proposed several alternate or supplemental conditions. Suggesting Dering as Hand A was the first. While extensive paleographic evidence indicates Dering's transitional hand wasn't A, it should be noted that, even if it were, it remains possible that Dering furnished a missing sheet to D in his own hand in the way another Elizabethan supplied a missing first page to Marlowe's Edward II.<sup>51</sup> In that case, bibliographic authorities argue that the source of the missing material wasn't a printed one.<sup>52</sup> A similar manuscript fragment of Marlowe's Massacre at Paris is also held to be genuine based on its lack of perfect fidelity to any known quarto; the same is true for the manuscript page of Titus Andronicus.<sup>53</sup> Independence for Edward II's manuscript pages has been accepted on the slender evidence of three variant readings, whereas f1r has numerous substantial and accidental variations from Q5, its presumed source, including a different title.

The second non-bibliographic argument constitutes one of the most puzzling aspects of this case. It began with Lennam's discussion in 1965 of Dering's account ledgers and was followed by Yeandle's 1986 essay, which showed payment "for writing oute ye play of K: Henry ye fourth" on the twenty-fifth anniversary of its initial registration.<sup>54</sup> Both the occasion and the substantial payment indicate that the copy was a presentation version, that is, a grade higher than D. The point is essential to this essay. Yeandle believes the ledger pays for D and that D is a copy of printed materials.<sup>55</sup> However, a close reading proves her case to fail on five points.

First, Dering's order specified both the copyist's name and the rate "p[er] sheet" at which he was paid, 1/2d. It also records the total disbursed: 4 shillings. As D contains 55 folio sheets, whatever Dering paid for had to have been either much shorter (32 sheets) or longer (64 leaves), depending on what was meant by the words "p[er] sheet." Yeandle suggests it implies a "bifolium," denoting two leaves after being cut. Yeandle uses the term "leaves" to mean a double-sided page or sheet. D contains 55 folio leaves (though Yeandle, curiously, counts 56).<sup>56</sup> If Yeandle's bifolium notion is correct, Dering paid for 128 pages; if not, 64 pages. Since D's pages are continuous and number only 110, D cannot be the manuscript paid for by Dering. Indeed, W. R. Streitberger concluded on the basis of this discrepancy that Yeandle's case had "failed."<sup>57</sup> To avoid this issue, Yeandle suggested that Dering caused his copyist to retranscribe six sheets. Even if he did, Yeandle concedes, Dering still overpaid by several sheets. And to account for this she then offers the curious suggestion that this literary antiquarian paid for blank sheets.<sup>58</sup>

Second, there is the issue of D's appearance: the manuscript simply doesn't look like a presentation copy. It is doubtful a contractor would have paid any copyist for D in its present state. He would have returned the manuscript to the copyist and demanded a uniform copy, similar to the standard

represented by fols. 10 and 11. D's paleographic nature thus indicates it was not what Dering ordered. Third, we note that Dering's ledger supplies the name of his copyist, a "Mr. Carington." This man was not, as William and Evans asserted, on his staff at Surrenden. He was, obviously, a well considered outside professional.<sup>59</sup> Yet D isn't the work of a professional scribe, according to both Yeandle and the editors. So, we have an impasse.

Fourth, while Yeandle cogently suggested that Mr. Carington was Dering's friend, Master Samuel Carington, the rector at nearby Wootton, Kent, the script in which Carington's will was written failed to match up with that found throughout D.<sup>60</sup> Though the will appeared to be a holograph, Yeandle's phrasing equivocates, implying it might, nevertheless, be a copyist's.<sup>61</sup> When supplied with copies of Carington's church register, which proved to be in the same hand as his will, Yeandle privately conceded that Carington wasn't Hand B.<sup>62</sup> One wonders why she thought so in the first place, since one must ask why a copyist who was paid by the sheet increased, at random intervals, his number of lines per sheet from 25 to 47, as evidenced in D, thereby reducing his payment, per sheet, by nearly half.

Finally, a detailed examination of D's leaves, undertaken for the Folger by Peter W. M. Blayney, proved that D was woven or pieced together in places that hadn't been noticed by scholars for centuries.<sup>63</sup> The most obvious weave corresponds to the point where the two plays now diverge.<sup>64</sup> Yeandle offers a complex explanation for the variation in paper stock which helped prove the weaving, with the purpose of demonstrating that there were once "two large quires," one for Part One and one for Part Two. Yeandle continues, "the evidence strongly suggests that the manuscript originally consisted of a single large quire for each of Shakespeare's two plays..." into which the slightly larger sized six-leaved quire "stands between."<sup>65</sup> Yeandle evidently forgot that the union in D isn't in the middle but near the end, since D's Part One materials are twice those of Part Two. Yeandle also seems unaware that the editors of the Facsimile never suggested a transcript had been made of both parts of Henry IV. What, then, is the solution to the six-page "quire" standing two-thirds through the manuscript? The most obvious one is that it was of slightly longer or oversized paperstock, as proven by Yeandle and Blayney. Since it was oversized, the scribe merely sorted it out, to be stacked together and used when he came to it. It happens to encompass the point where the two texts conjoin. Less obvious piecings are evidenced by the various styles of Hand B. One should also note that the point in D where Part Two diverges is not at the beginning of the last act but at the beginning of D's Act iv, scene 9, which in D is followed by Act iv, scene 10 (which contains material from Part Two, Act ii, scene 2). If D were a transcript of the two-part play, the obvious place for the act change would have been at the beginning of f42r, where this "mysterious" quire lies. For these reasons, D cannot have been a transcript of the printed text nor what Dering ordered to be copied.

### Dering and Fletcher

For what could the order have been if not for D? Given the absence of the manuscript by Carington from Dering's collection, Dering's order may well have been for a gift, i.e., not intended for Dering's personal collection. Given the anniversary date, it seems that it was for someone associated with the publication of Henry IV in 1598, exactly twenty-five years earlier.<sup>66</sup> Other than the author, only the playwright John Fletcher, who followed Shakespeare with the King's Men, and Shakespeare's publishers, Edward Blount and



William Jaggard, seem likely recipients for the missing manuscript. Though Blount's name is not associated directly with Shakespeare until 1608 (with the publication of Pericles), Blount was active in 1598, having appointed himself Christopher Marlowe's literary executor.<sup>67</sup> The most likely possibility is that the lost Carington manuscript, paid for by Dering, represents the literary copy of Henry IV: Part Two used almost immediately by the Folio publishers.

### **Dering's Order and the Gap in the First Folio**

If D stands apart from Dering's order for a copy of Henry IV, we may inquire into the nature of Dering's order and the manuscript it produced. What was it? Where did it go? Prosser has proven that Henry IV, Part Two was set from a literary transcript furnished to the printers in 1623.<sup>68</sup> Could this have been what Dering's order produced? Two lines of thought suggest it. First, Part Two was likely to have been the rarer of the two, having been issued in only one edition.<sup>69</sup> Second, the evidence of the date and the length of Carington's copy (see footnote for calculation) suggests this scenario, for it seems unlikely that two literary copies of Henry IV were set out in 1623.<sup>70</sup>

### **How Dering Obtained D**

The historical record supports the existence of a friendship between Dering and Fletcher, as well as links to Blount and Jaggard. Perhaps through his association with Fletcher, Dering came across D among Fletcher's books and papers, it having passed directly into Fletcher's hands from Shakespeare. Dering would have prized D for himself, as collectors do, and coaxed Fletcher into an understanding.<sup>71</sup> What is the evidence to support this? The record of Dering's library of playbooks proves Dering enjoyed pre-publication access to Fletcher's manuscripts, since D includes a scrap containing, on one side, a character list from Fletcher's Curate, dating several decades prior to Curate's publication.<sup>72</sup> Additionally, Dering's personal records prove he knew Fletcher, who was also from Kent, along with Fletcher's brothers who lived nearby.<sup>73</sup> Lastly, Dering married into the Beaumont family, and Francis Beaumont, of course, was Fletcher's collaborator.<sup>74</sup> A close connection between Dering and Fletcher is therefore likely.

Is there anything else concerning publication of the First Folio that might suggest a connection between D and the publishers? There is. A mistake in pagination within Henry IV caused the duplication of pages between the numbers 69 and 100 (102, if the unnumbered sheet is counted). This double numbering was created when the printers skipped Henry IV and moved on to Henry V after the partial printing of Richard II.<sup>75</sup> To do so they allocated 23 pages for Henry IV.<sup>76</sup> There is no way both parts of Henry IV could fit into this gap, a space which begins at page 45 and runs on to 69. (The Folio's two-part Henry IV required 57 pages.) No bibliographic account has resolved the issue, but it seems the printers considered typesetting a short, unified version of Henry IV that would run to about 23 pages. Greg and others don't expand on this, not believing in a shorter version. However, faced with the double numbers, Greg suggests the problem arose with Matthew Law.<sup>77</sup> Whatever caused the problem, there is tangible evidence that D, or something very much like D, was considered by Blount and Jaggard for publication in the First Folio as the original Henry IV, for, as shown in the footnote, only D could have fit into a space this small.<sup>78</sup>

Was D then Dering's and, perhaps, Fletcher's best attempt at re-establishing Shakespeare's first version of this famous play, or was it the Bard's

original manuscript? The absence of bridge lines (see below) suggests that it was Shakespeare's, since no editor could have excised the text in this manner. Indeed, Craig noted it would have been "impossible" as well as "stupid."

### What is D?

Obviously, the printers had set from manuscripts of some kind. Greg allows that "everything points to Q[I] having been printed from foul papers, and there is no need to suppose that these had even been annotated by the book-keeper."<sup>79</sup> Greg suggests a prompt-book with a literary editor lies behind Part Two; Prosser and others, the literary copy of 1623.<sup>80</sup> We thus have bibliographic evidence that Q1 was set from the author's papers and not from an intervening prompt-book, and that the First Folio was set from a literary copy. Proponents of dependence concede that D wasn't a prompt-book or foul papers.<sup>81</sup> Could it then be an "authorial fair copy?" The answer is yes.

Is there additional evidence that Sir Edward Dering collected original fair copy manuscripts apart from D and the Curate, or that he prized such pieces? There is—the companion manuscript from Dering's holdings, Love's Victories, sold along with D in 1845, and now at the Huntington.<sup>82</sup> It is undeniably in the author's holograph.<sup>83</sup> This is powerful evidence that Dering collected, prized and kept authorial fair copies of period plays. Importantly, Love's Victories is not marred with Dering's glosses.<sup>84</sup> D, for the most part, seems no different. D is an authorial fair copy similar to Love's Victories, one that Sir Edward obtained from Fletcher, who, in turn, obtained it from the author. D is the sort of copy the Folio editors testified they routinely received from the author, but disallowed by Greg for want of an exemplar.

### How Can D's Status be Established?

Normally, topographical order and coherent orthography enters an author's work at the printers. The supposition is that any Q would be more orderly than its manuscript and any transcription of Q more orderly yet. Indeed, the editors implied this when suggesting Hand B noticed the missing pronoun. Yet, contrary to this expectation, D is much less orderly and less consistent than any Q. To avoid this, the editors proposed that Hand B was disorderly:

It is not suggested that the scribe was of a high order of professional competence—not a London master, by any means, but more likely one of the staff at Surrenden.<sup>85</sup>

Yet this assertion cannot be documented from D, which is quite systematic. One need only glance at fols. 10 and 11 to see how tidy Hand B could be. These are presentation-style sheets that certify Hand B to be the equal of a London master and strongly suggest what his "fair copy" must have looked like. Their placement among lesser sheets indicates the earlier nature of those drafts and of D. This, along with the passage of time, I think accounts for D's "jumbled" nature, as opposed to Hand B's supposed incompetence.

One of the most obvious forms of order imposed on a manuscript is its reduction to regular lines. Q5 managed 38 lines a page. Yet D's sheets range from as few as 25 lines (f19v) to as many as 47 lines (f3). Two factors account for such irregularity. Hand B was practicing "compression" and "expansion"—controlling where his stints ended. Why? Most likely, to match up with pre-existing materials. Indeed, Yeandle has proven it.<sup>86</sup> A less involved example can be seen on f19v where the styles change a third of the way down

the sheet. Additional explanations are that Hand B was taking dictation or following foul papers, where the number of lines per sheet wasn't obvious. If Hand B had a printed text to follow, he would have regularized his lines per page and remained with it throughout.

Another disparity between transcripts of authorial papers and printed materials is that the latter should display no indecipherables, i.e., no words, phrases or expressions the transcriber couldn't read. (Mistakes, certainly, but no indecipherables.) Yet D displays indecipherables throughout. In case after case, Hand B attempted various readings of what, in Q, were clearly resolved words and phrases. He had, for example, continual problems with distinguishing among "God," "good" and "gold". There are ubiquitous examples of words that Hand B could not read and which he passed over, holding his space with a light dash of the approximate length, as in "bowcase" (f17v). With the dash holding his space, he finished his sheet, checked his reading and later inserted it. In "bowcase" his ascenders cross over the light line above, proving this beyond a doubt. There are also several examples of larger spaces left in the manuscript into which entire speeches seem to have been transcribed later. For instance, see f20v, the Prince's speech, "Swearest thou:...."

Nothing in D suggests ineptitude on the part of Hand B, so when he doesn't know whether his copy text reads "France" or "Francis", he writes what he sees, even though it is incorrect and must be corrected later, likely by Hand A, to "Francis" (f14r). The same is true for "the seat of god" which should have been "the seat of Gaunt..." (f34v), and which B caught in time to correct himself. This immediately precedes his problem with "Duckdome of Lancaster" which he seems first to have attempted as "Duck onne of Lancaster." Just above, Q5's correct "Doncaster" stands in D as "Dancaster," showing B's difficulty in seeing the difference between the author's "a" and "o." Since Q5's readings are clear, the problem here was with the author's foul papers. Dozens of other "indecipherables" occur in D's text, including the problem with "discarded", which he attempted as "disgorged" (f7r). Numerous problems on this same face (f7r) indicate confusion in the author's papers. See line 76, "And Let them grapple: the blood more stirres" where "more" proved indecipherable and Hand B held his space with the light line now seen, returning with a proper reading and another quill and ink. Two lines above "from the east to west" were modified by B, likely with the same new quill and ink to "from the east unto the west", via a bridge  $\wedge$  above.

Q's fine "fiery-eyed maid of smokey war" (f31r) was attempted no fewer than three times, each time more polished than the last. The sequence, by no means clear, seems to have run like this. Hand B first attempted

& to the firry maid: of smokie Warre:

He then corrected it by striking off the "y" in "firry" and changing it to "fiere". Still not correct, he added above the line "eiede",

eiede

& to the firry maid: of smokie Warre:

Having some problem with his quill, he left a blot here. Indeed, Hand B may first have attempted "to the furry man of smokie Warre:," depending on how one judges the depth of his confusion.

With the exception of f1r, this sheet (f31r) is D's roughest. It is, thus,

of interest to know that Dover Wilson has pointed out that this passage, containing “the fiery-eyed maid” in Q, carefully conjoins materials lifted from several primary sources.<sup>87</sup> They include Daniel’s Civil Wars, Nashe’s Unfortunate Traveller and Spencer’s Faerie Queene. Wilson concludes, “a close connection [between all three] is indisputable.”<sup>88</sup> It is also certain when drawing closely upon parallel sources that authorial papers are not likely to be as clear as when one is inventing materials de nova. That D reflects confusion, false starts and problems, at this point, strongly suggests D was much closer to the author’s foul papers than to Q.

### Interaction Between Hand B and A

Proof that Hand B was interacting frequently with Hand A, contrary to the editors’ assertions, can be found on f6r. As with half- a- dozen sheets in D, f6r doesn’t show the customary “cue word,” the word placed at the end of a page which will then appear at the top of the next page to facilitate reading. This suggests the material on f6r, its verso, as it is called, wasn’t ready for transcription when Hand B completed the recto or “face” of f6. This is proven when one notices that Hand B has concluded his stint with an ending flourish:

&

after which he supplied an erroneous entry for “Worcester.” What had happened here? Clearly, Hand B had come to the end of his materials. Since he had no idea what the next word was going to be, he couldn’t supply the cue word. Something, however, indicated a new scene with Worcester in it, but no more than this. He thus finished his sheet and put away his papers, ink and quills. Afterwards, Hand A proofread this sheet. To resolve this and other muddles, Hand A revised the sheet. In the process of revision or review, Hand A made several glosses on the face of the sheet, the most important of which was the stage direction which cleared the stage and ended the act. This was done by adding beneath the exit cue for the King:

{Lanc: er Blunt.

Just below this he also supplied an entry for Worcester using a long curved line, at the end of which he wrote:

Enter Worcester.

Finally, Hand A wrote over the face of the sheet, in his bold ink, what must have been, at that moment, a compositional line:

But I will lift th downe-trodd Mortimer.

After and only after this was accomplished, Hand A returned to the sheet and added the scene break, writing in red ink:

---

Act.Ii.Scae:4ta.

---

(Q, of course, did not contain act and scene information, so this material cannot be thought a copy of Q.)

At this juncture Hand A gave the rest of the sceneover to Hand B, who

then turned the sheet over and supplied the verso material, which begins with Q's line, "But I will lift the downtrodden Mortimer":

As high In' th ayer: as this unthankkefull kinge

There is simply no other way to resolve this sheet paleographically, if for no other reason than the verso would have been insensible without the addition of A's gloss, "But I will lift th downe-trodd Mortimer." All this is quite clear in Q, where it appears as a speech without a break or ending. Here as elsewhere, there is no evidence of an underlying Q. The similarity of styles, on both sides of this sheet, indicates the proofing soon followed Hand B's transcription of the recto.

A copy of Q should also reflect the topographical order of Q in its general orthography regarding spelling, punctuation and the like. Additionally, no copy of Q should evidence "ghost" characters or confusion regarding the characters, since this too is generally eliminated at the printers. Nor should one discover "false starts." Yet none of these "regularities" are discovered in D. D does not follow Q's spelling, punctuation, lining or other elements of published style. Ghost characters, or confusion as to characters, may be found throughout. See the first sheet (f1r), where Blount is excised and then returned in a gloss. Older word forms also appear, such as "yow" for "you" and "on" for "one." This includes a knowledge of the Christian names and family relationships of the characters, not evidenced in Q; see f55r where D's reads "Broth:—" for Earl of Lancaster. These designations make the play more primitive and less formal than Q. Interestingly, the character list generated by the editors does not carry the primitive names Peto and Rossill; D mentions them, as well as turning Q's Gads-hill into "Bardolff" (f10r), so where did Hand B find these names if they were not in Q5? Finally, once unfamiliar place names had been standardized in Q, why should D use its own quaint and often misleading spellings, such as "Dancaster" for "Doncaster?" That many of these spellings have long been thought to be Shakespeare's is also worthy of note.<sup>89</sup>

Consider the evidence of false starts in D. This begins on f1r, where two lines have been crossed out, several changed, and the face marked for deletions and insertions. A less involved example appears on f51r:

...thie dew from me  
is teares: and heavy sorrowes of the blood,  
~~—drownes— it— self— from—~~  
which nature: love: and filiall tendernes:  
shall (o heere father) paie thee plenteously:

"drownes itself from ....", which is crossed out in D, cannot be found in Q. Where did it come from? Isn't it a first draft of the next line, which not changed, would have read:

drownes itself from filiall tendernes

What else could it be?

There are several places in D marked for insertion which in Q contain expanded materials at these same points (see folios 1v, 20v, 55v). Notice the curved line under "Prin:" on line 22 on f55v:

Prin.,—

This is where 60 odd lines, plus three scenes are added in Q. So we have conclusive proof this sheet was marked for insertions which entered Q. The missing material is to be found at v.ii. 64-122 and afterward. The editors are silent about this.

### Paleographic Proof of D's Independence

While the above are strongly suggestive of D's primacy, two aspects of D offer decisive evidence for its authorial nature: "layers" and "proof-marks." One need only place f31 and f55 side by side to see they were once part of the same "stint." A stint is defined as a single period of transcription, usually unmarked by a change of quill, ink or paper stock. Stints differ from "drift," or the change of a hand over time. D evidences both stints and drifts. Hand B's many styles indicate that his work on this manuscript was sporadic, taking place over the course of a year or more, during which time his styles "drifted".<sup>90</sup> The styles, quill, ink and paper of f31 and f55 are essentially the same, so much so they can safely be said to have once been part of the same stint. As f31 is from Part One and f55 is the ending of Part Two, this means D displays the remainders of a draft which once placed the ending within a page or so of the Battle of Shrewsbury. Who would have such a draft but the author? As pointed out, fols. 10 and 11 are also part of a now lost, letter-perfect draft. Most likely it was the fair copy which became the copy text for Q0 of Part One.<sup>91</sup> How did these sheets come to be in D? They seem to have been culled after they were glossed by Hand A and left behind to become part of D, woven into it seamlessly by Hand B's "cut and paste" finesse. This means, paleographically, that D is a composite manuscript comprised of sheets from several drafts of Henry IV. Generally, only authors possess such remainders.

The second proof is a system of silent proof-marking. This paper has pointed out that Hand A glossed the manuscript at will; Hand B did likewise. The reader's attention is now directed to a less obvious system of corrections which cannot be attributed to either Hand A or Hand B. These proof-marks prove that a third party, Hand C, proofread this manuscript from beginning to end, but proofread it "silently". Why? Obviously, if this was a manuscript that the hypothetical Dering intended to turn into a prompt copy, the proofing would not have been mute. The proof-maker would have wanted the proof to be seen when scanning the page. The only rationale for this universal but covert proofing was to alert the next copyist to problems without marring D's face. This means that whoever added proof-marks to D was outside the authorial stream, yet someone who regarded the manuscript with respect. One might add that the notion that Elizabethan printers routinely destroyed manuscripts while printing them is incorrect.<sup>92</sup> Printers generally were careful with manuscripts, returning them to their owners in good condition.

Who might have made these marks? So far as scholars know, only printers and scriptorium clerks employed them. The marks are faintly made, as follows:

Λ, —/—, —//—, ———, /, +, \*, !, and one ?

That these marks are from the period can be proved by examining the one which descends the line on f39r in the King's phrase "& shewed I thow makst some tender of my life", where the pronoun "I" has been excised from the copy

between “shewed thou.” Most of these marks were made horizontally, rather than vertically. They were made above the line thus: —//—. They indicate that the line is to be opened up and the proper form inserted. In this case the mark was vertical and indicates closure of the line. Since these proof-marks cannot be accounted for by the dependence thesis, Williams and Evans ignore them, except to make their own corrections of the text, as when they alter D’s “I am a gentle noe more” to “I am a gentleman:, come: noe more,” which follows triple proof marks:

——//—; Λ; (

on f44v’s last line. They could not have resolved this line without understanding the proof marks. Throughout D a light dash, sometimes called a “spur,” can be found above an open letter “a” that could be confused with “u,” and also above the letter “c” when it might be taken for the letter “r.” Since these dashes are not to be found above all the “a”s and “c”s, they are likely to be Hand C’s proof-marks and not Hand B’s spurs.

Regarding spurs made by William Herbert, as seen in a letter dated 26 August 1619, Yeandle remarks how curious it was to find such marks at so late a period.<sup>93</sup> Indeed it is. She is, however, quite silent about them in D. Perhaps she just overlooked them. That they seem to have been made by a proof-reader rather than Hand B is all the more evidence that D dates to the early 1590s, for a 1623 reader would have simply changed the letter forms to their italic counterparts. Further evidence of the marks can be seen in most, if not all, contractions of “with” written t/w. They display a horizontal line through the “t”, implying the contraction is to be written out, as the editors often do.

One cannot identify Hand A with certainty. The script does not match that of Dering’s transitional hand or any author whose writing has come down to us, with the exception of Hand D in Sir Thomas More. This is not to say the hands are the same, just that they are quite similar: strong compositional Secretary hands, obviously that of a writer trained in the 1570s and 80s. Hand A is more thoroughly Secretary than Hand B, indicating the writer to be the older of the two, most likely someone born in the 1560s and educated in the 1570s. Attempting to trace anyone’s identity through their handwriting, however, has usually proven forensically unreliable, which is why it is not employed as a means of identification like fingerprints.<sup>94</sup> Paleographic identifications have been proven more in the eyes of the beholder than in fact.<sup>95</sup> The odds are strong, however, that Hand A was the author’s.<sup>96</sup> Identity aside, the paleographic case for the independence of D is conclusive: D was not transcribed from printed materials.

### Literary Considerations

There are the differences between D and the printed texts to weigh. It has long been suspected Shakespeare once wrote a play similar to D, i.e., a unified Henry IV.<sup>97</sup> Indeed, Williams and Evans concede in their commentary that the organization of D solves the structural problems caused by the two-part version and thus parallels more closely the events in Samuel Daniel’s Civile Wars. But they dismiss this as a form of accidental convergence.<sup>98</sup> This seems an unlikely explanation since a condensation of the two printed texts would likely expunge the history and highlight the Falstaff materials, whereas D does the reverse. The first quarto of Hamlet is a case in point. There, much of the marvelous language and detail is missing, with the exception of the comedy.<sup>99</sup>

Since D is shorter than the printed editions, and most of that reduction comes from what is now Part Two, which is present in D only in outline form, one must ask: why wasn't it equally cut? Moreover, if D is dependent, it must have been reduced from cuts and condensations. Williams and Evans write:

But in literary terms, the Dering version of Henry IV was successful. It skillfully condensed the two parts of Shakespeare's play (6,148 lines together) into a single play (3,401 lines).<sup>100</sup>

However, is it true to say this material was condensed? While condensation can mean "to shorten," it more often means "to summarize." Yet there are no summaries in D; it is shorter simply because it does not contain lines which appear in Q. In other words D, if it has been reduced, was reduced by cutting, not by summarizing. Yet D evidences no obvious bridge lines, i.e., lines created by editors when they perform cuts since cuts do not mesh perfectly with pre-existing material. Consider that D displays continuous lines which appear in it "jointlessly" but appear in Q unrelated and even located entire scenes away! Moreover, all the cuts in D were perfectly executed and the remaining material does not refer to any missing text. This is a phenomenal development, since casual cutting always leads to such quandaries. Craig had called it "impossible." How could it have been accomplished? More important, why bother? A period editor would have no reason to avoid writing himself into the text. Such "revised" and "improved" versions were commonplace, as can be seen from the revisions of The Tempest, titled The Enchanted Island: a Comedy (1670), and of The Taming of the Shrew, titled Sauny the Scot.<sup>101</sup> All similar revisions display the overburden of a lesser poet, much more obvious when the lesser one writes alongside William Shakespeare.<sup>102</sup> Yet D displays no such presence. Indeed, as shall be shown, it is Q which contains evidence of a second, less able writer.

The most obvious proof of this is the virtual impossibility of finding in D a line not in Q. When one does it is unquestionably superior, such as "I'll take say of thee" (f20r), which appears as an authorial gloss, or "then fear gave wings to flight," which is embedded on f43r. Greg attributes these "attractive readings" to Dering having "recalled" them from performances, but this seems doubtful, if for no other reason than these lines are not likely to have been in any hypothetical performance. Indeed, according to the dependence consensus, the splendid "fear gave wings to flight" line is a condensation of "Mourton's" account of the battle of Shrewsbury in Part Two and thus original to D. If so, the line could not have been from a stage version. Even Greg can not have it both ways.<sup>103</sup> Surely, this line was the author's earlier poetic conception of the rout at the battle of Shrewsbury, with the metaphor getting lost during the expansion of the play.

Consider f46v, where D is ready to "jump" from Q's III.i to IV.iv—a leap of four scenes! It will manage this stellar juxtaposition without a single bridge line. Does the reader believe anyone could have planned and executed such an excision? As a matter of fact, the absence of bridge lines verifies that D was expanded into the two-part version. This is because any author expanding a text furnishes bridge lines as he writes; thus, the original material becomes embedded. It is unlikely, however, that any one could cut back to it. Those who claim dependence should point to joints in D and show where and how these cuts and abridgements were made.<sup>104</sup>



### The Reverse Contention Considered

A strong reverse case also establishes the primacy of D. If D is the primary text, then those lines in Q not found in D result from revision, perhaps a hasty one. Computer analysis, such as Ule's and my own, demonstrates this, but it can be seen by a cursory visual scan.<sup>105</sup> The lines in D are shorter and more compact, whereas the additional lines in Q are longer and more expansive.

Q also contains material which one doubts the author responsible for, Part Two reading:

'Let us make head': it was your presurmise,<sup>106</sup>  
That, in the dole of blows, your son might drop:  
You knew he walked o'er perils, on an edge,  
More likely to fall in that to get o'er: (I.i.167-71)

This material is not to be found in D. It is the only time Shakespeare uses the expression "presurmise," a thought which would have been better served with "fear." Just above is another phrase not to be found in D, "the rude scene may end," (I.i.159.). Was this an example of the author's cheek at having to revise this scene under pressure? Consider Morton's speech at this same point in Q (I.i.187-210), which is not in D. Does it not smack of revision? Dover Wilson has noticed some problems with this scene, including an "ugly repetition" or fold.<sup>107</sup> There are several "ugly repetitions" in the opening scene of Part Two where Northumberland parrots back bits and pieces of lines given to him by Travers and Morton. D has none of these problems.

### Order from Chaos

On the other hand, consider how much effort must have gone into D if D is a copy of an author's foul papers—a copy which dates to the unified period of Henry IV, when the author was under pressure to bifurcate it in order to expand the character of Falstaff. D seems to represent the version which he preferred, and kept with him, as we see by the gloss, "vide the printed book" (f18v). This gloss most likely dates to his review of this manuscript at the time he was writing Henry V, i.e., ca. 1600. Since a seven-year hiatus has been established between D (1593) and Henry V (1600), during which time other plays intervene, an authorial review of D prior to writing Henry V seems likely. Several of the glosses may date to this review, rather than to earlier ones, and several may date to Sir Edward's ownership of D. Indeed, the final gloss on f55r may be seen as a bridge between Henry V and D, a bridge which would not have been required until Henry V was well underway, or more likely, when D was considered for the First Folio, where Henry V would follow it on the next page.<sup>108</sup> In fact, these lines link to Henry V, II.iv.36.

Scholars provide other examples of similar transcriptions (see Knight's transcript of Bonduca, Crane's of Demetrius and Enanthe and Game at Chess).<sup>109</sup> These manuscripts confirm that presentation copies of unpublished plays were made from the author's papers for private hands. Indeed, when D is compared to one of these or to the Woodstock or Timon manuscripts, the similarity in "layouts" is striking. For it is not likely that the style of D should, in and of itself, parallel the style of authentic handwritten stage materials. This fact indicates a closer relation to such materials than to printed ones. Overall, D does not show itself to be homogeneous enough to have been a presentation copy, certainly not one transcribed from a printed text. D can only be an authorial fair copy of Henry IV.

## Conclusions

I believe D to be a pre-quarto version of Henry IV, in the style of an authorial fair copy, as first thought by Hardin Craig and afterwards by Ule. This has been rejected by authorities who believe that Shakespeare never revised his texts or produced fair copies. The proof provided herein should expose the failure of bibliographic and biographic arguments and the superiority of paleographic and literary proofs for independence and primacy. These show the manuscript to have an authorial provenance, parts of which reflect an even earlier draft of Henry IV than D itself. For those who hold that Shakespeare wrote Shakespeare, he must have written D. Since D proves the author revised and spent considerable efforts on D, beliefs to the contrary are false. Pollard's arguments were, thus, correct. That Pollard's views, as well as Craig's, have been circumvented and suppressed by those propagating contrary paradigms has been clearly established.

External evidence suggests that Dering obtained D from John Fletcher. Similar evidence suggests that Dering furnished the printers of the First Folio with the literary copy used to set Henry IV, Part Two. That copy may have been presented to either Blount or Jaggard at a celebration held at Dering's home, Surrenden, on Friday, 27 February 1623, which marked the twenty-fifth anniversary of the registration of Henry IV, an event of obvious importance to those involved. Finally, the mysterious gap in the pagination of the First Folio and the double pages between Henry IV, Part Two and Henry V, may have been caused by the printers when they considered setting D rather than the two-part version. D's silent proof marks may have originated from Jaggard's shop or, if Greg was correct as to his role as editor, from Jaggard himself.

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9. Henry IV: MS. Folger Library Manuscript: V.b. 34. Facsimile. Virginia. 1974. William Shakespeare The History of King Henry The Forth as revised by Sir Edward Dering, Bart. George Walton William and Gwynne Blakemore Evans, eds.

10. Henry IV, extracts from a contemporary MS., Additional MS 64078, British Library. Associated with reflections by Thomas Harriot dated 1594 in Paris. See Hilton Kelliher below. Also of considerable moment is the commonplace book of Edward Pudsey (1572/3-1613) which contains extracts from The Merchant of Venice, Titus Andronicus, Romeo and Juliet, Much Ado about Nothing, Richard II, Richard III, Hamlet and Othello, mentioned by Kelliher. That these notes provide a manuscript link between Thomas Harriot and the author of Henry IV remains of primary interest.

11. Additional Manuscript to Holinshed Chronicles re. the murder of Master Arden. British Library: Harleian Mss 542, ff 34-37v. Entered the published works in the edition of 1587.

12. Edward II: title page and first page of 1598 edition, supplied in a 16th century hand to the Dyce Copy of Edward II, Victoria and Albert Museum. Important as to nature of transcript vs. authorial source.

13. Edward III, MS. source of its description of the Armada battle, Naval Records Society. O.R.14.A.X. [Not seen, cited by Wraight.]

14. Halle's Chronicle, The Annotator [in], Putnman, 1954, Alan Keen and Roger Lubbock. Authors suggest "Shakespeare" annotated this particular edition.

15. Ironsides: Egerton MS., British Museum, Collection of William Cartwright. See Shakespeare's Lost Play, Wildwood House, 1986. Eric Sams, ed.

16. Love's Victories, MS., HM600. Huntington Shakespeare Library. The companion manuscript to D in Dering's collection. Proves he wasn't in the habit of annotating original documents. Library Annotation reads: "Worth, Lady Mary (c.1586-1640). Love's Victorie, 21ff. anon. play, imperfect at beginning and end. Autograph with author's corrections." That it is imperfect at the beginning suggests Dering might have been tempted to refurnish its initial sheet. However, its absence indicates he lacked another copy. That copy, perhaps in Carington's hand, was nearby at Peshurst: "A complete anon. copy is to be found in the collections of Viscount De L'Isle and Dudley, Peshurst Place, Kent, England," according to the Huntington's annotation. This connection between Dering and the Sidneys' manuscript collections should be further explored.

17. Massacre at Paris. MS. leaf. Folger Shakespeare Library. The standard discussion is found in J. Q. Adams', "The Massacre at Paris Leaf," The Library, 4th ser., xiv (1934), 447-69 and J.M. Nosworthy's, "The Marlowe Manuscript", The Library, xxvi (1946), 158-71.

18. Merry Wives of Windsor, MS., An Account of the only known Manuscript of Shakespeare's Plays, AMS 1972. James Halliwell-Phillips, ed.

19. Misogonus, MS., H.M. 452. Huntington Shakespeare Library. Printed in Bond's Early Plays from the Italian, Oxford, MDCCCXI.

20. Sir Thomas More, MS., British Library, Harley 7368. Full Plates of Hand D in the Riverside Shakespeare, Partial Plate in Harrison's CW.

21. New Verses. MS. E.L.34.B.9. Huntington Shakespeare Library.

22. Parnassus (Three Plays). Oxford 1886. W. D. Macray, ed. (From MSS. in

the Bodleian Library.)

23. Timon, MS. MS. 52. Dyce Collection. Victoria and Albert Museum, South Kensington.

24. Titus Andronicus, MS. and drawing. Seen only in Facsimile in A Documentary Life of William Shakespeare. Sam Schoenbaum. Oxford 1975. However see: Eugene M. Waith's Titus Andronicus (Oxford 1984, pp.20-7) for a discussion proving the source of the material wasn't transcribed from a published quarto.

25. The Wasp or The Subject's Precedent, MS., Alnwick Castle (MS. 507), seen only in Malone Society Reprint, G.R. Proudfoot, ed. 1976.

26. Woodstock, MS. A Moral History, Chatto and Windus, 1946. A.P. Rossiter, ed. Egerton MS., British Museum, Collection of William Cartwright. Said scribal copy but in-line revisions indicate it a good foul paper. B.M.M.S. Egerton 1994, art. 8.

In addition to these see the full list of "private transcripts" cited by Greg in Editorial Problems [pp. 162-63; his discussion in this chapter, pp.106-177, is of particular value. Curiously his opinion that there were no private transcripts prior to 1624 contradicts even the consensus date for D, as well as his own date]. Most are available in Malone Society Reprints and have been consulted therein.

**Primary Period Records:** These documents have also been consulted in the original, unless otherwise noted.

1. Documents from the Dering Archives are housed at Maidstone Records Office, Maidstone, Kent, see U350 C2/22, U350 C2/94, U350 218, U3500211, etc. Also Folger MS V.b.297.

2. Carrington's Will: Kent Record Office. PRC 16/232 bdl 37.

3. Carrington's Church Register: Wootton. u3/136/1/1 f.43. Canterbury Diocese. Taken together these documents prove that Carrington was not Hand B in D. No other "Carrington" has been suggested.

4. Shakespeare's Will: Public Records Office, Case XI.

5. Kathyrine Benchkin's Will: Kent Archives Office PRC 16/36.

6. Katherine Marlowe, Widow of John Marlowe, Kent Archives Office PRC 16/127.

7. Papers relating to Arbella Stuart: Middleton MS Mi 6.

8. The Diary of Edward Alleyn, Vol.II, MS. IX, Dulwich College. (Not Seen.)

9. Henslowe's Diary. London. 1904. 2 vols. W. W. Greg.

10. Letter concerning Marlowe as Arbella's "Reader," 21 September 1592, BL Lansdowne MS 71, f.3. (Not seen.)

11. The Ledger of Edward Whalley. 18 September 1589. Folger MS. v.b.308. (Seen only in xerox.)

**Facsimiles, Microfilms or Printed Transcripts of Original Materials:**

1. The Letters of Lady Arbella Stuart. Oxford 1994. Sara Jayne Steen, ed.

2. The Diaries and Papers of Sir Edward Dering. Her Majesty's Stationery Office. 1976. Maurice F. Bond. Focuses on Sir Edward's son, but useful.

3. A Descriptive Index to [Edmond Tyllney's] Diplomatic Manual on Europe, AMS, 1986. W. R. Streiberger, ed. Folger MS and Illinois, MS. A phenomenal thousand-page manuscript covering English secret affairs over a thirty-year period in six languages. Appears to be based on the missing papers of Sir

Francis Walsingham. The attribution to Tyllney seems doubtful since Tyllney commanded neither the languages nor the diplomatic background required. Also curious is why the manuscript was not published and why "Sir Lewis Leuknor" replaced Tyllney in the favor of King James.

4. Shakespeare, William. First Folio, (London, 1623), A3; The Norton Facsimile The First Folio of Shakespeare, Paul Hamlyn, London, 1968). Charlton Hinman. Proves the "hole" in the histories into which only D could have fit.

Hamlet (Q1) Barron. New York. 1962. Albert B. Weiner, ed. His discussion devastates the theory that Shakespeare's plays were "pirated."

Hamlet (Q2) Scolar Press Facsimile. 1969.

Hamlet (Q2) California. 1964. Oscar James Campbell, ed.

Henry IV (Q5) University Microfilms.

Henry IV Part Two (Second Issue) ".

Shakespeare's Play of King Henry the Fourth. Printed from A

Contemporary Manuscript. James Orchard Halliwell, London. 1845.

William Shakespeare The History of King Henry The Forth as revised by Sir Edward Dering, Bart. Facsimile and Transcript. Virginia.

1974. George Walton William and Gwynne Blakemore Evans, eds.

Richard III. (Q1) Valentine Sims, 1597, Tudor Facsimile, 1963.

Lycreece. Richard Field, 1594. Scolar Press Facsimile. 1973.

Venus and Adonis. Richard Field, 1594. Scolar Press Facsimile. 1973.

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3. Emilia Lanier. Salve Deus Rex Judaeorum, London, 1611. Valentine Simmes for Richard Bonian. (See A.L. Rowse's, The Poems of Shakespeare's Dark Lady, Potter. 1979.) Published by Shakespeare's printers, the book reports itself to be by Emilia Lanier, a well known courtesan.
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  8. Famous Victories of Henry V. A Case for Shakespeare's Authorship of, New York, 1961. Seymour M. Pitcher.
  9. anon. History of the Uniting of the Kingdome of Castile and Portugal. London. 1600. An important translation of Conestaggio's work, erroneously attributed to Edward Blount. Blount specifically says he wasn't the author and that it was given to him by a gentlemen who he cannot name.
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  8. Clara Longworth de Chambrun. Shakespeare A Portrait Restored. Hollis and Carter. London. 1957.
  9. Hardin Craig. An Interpretation of Shakespeare. Citadel Press. New York. 1948.
  - "The Dering Version of Shakespeare's Henry IV," Philological Quarterly, XXXV (April 1956), 218-19.
  - A New Look at Shakespeare's Quartos. Stanford University Press, Stanford, California, 1961.
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  13. G. B. Evans, "The 'Dering MS' of Shakespeare's Henry IV and Sir Edward Dering," Journal of English and Germanic Philology, LIV (October 1955), 498-503.
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## Notes

<sup>1</sup> Of several manuscripts, Woodstock, Arden, Cardenio, Sir Thomas More, Ironside and Timon, only D involves a canonical play.

<sup>2</sup> Hardin Craig, "The Dering Version of Shakespeare's Henry IV," Philological Quarterly, XXXV (April 1956), 218-19.

<sup>3</sup> James O. Halliwell, Shakespeare's Play of King Henry the Fourth. Printed from a Contemporary Manuscript. (London: Shakespeare Society, 1845).

<sup>4</sup> University of Virginia Press, 1974; Folger Library Manuscript: V.b. 34.

<sup>5</sup> Dering's life is fascinating. His title depended on a bold forgery to the face of the Magna Carta, which was effected by an unknown benefactor when he was five. (DBN)

<sup>6</sup> Surprisingly, both Yeandle and Dawson once agreed to the 16th century nature of D's hand, writing, "the hand is, except for stage-directions and speakers' names, pure secretary," 104. See footnote 7 for a glaring contrast.

<sup>7</sup> Giles E. Dawson and Laetitia Kennedy-Skipton [Yeandle], Elizabethan Handwriting: A Style Manual, (Norton, 1966) p.96. Concerning the copy of Sir Francis Drake's letter to John Foxe (#35), originally written in 1587, they have assigned the date of transcription to c. 1615. They write, "At first sight this hand might seem to have been written about the time of the original letter. But the restrained and controlled ascenders and descenders ... are more characteristic of the first decades of the seventeenth century". In discussing the Bagot letter (#38) they note, "he wrote a transitional hand reflecting the growing preference for simplicity and clarity which was effecting the gradual shift from the secretary to the italic hand." The letter is dated to 1622 and according to these authorities "shows the writer to be a child of the seventeenth century." Dering, born in 1598, was such a "child," and his many papers prove his hand more similar to Bagot's transitional hand than to Hand A.

<sup>8</sup> Hand A has been identified with Dering's. Evans asks that we take this



ascription on “faith,” see “The ‘Dering MS’ of Shakespeare’s Henry IV and Sir Edward Dering,” Journal of English and Germanic Philology, LIV (October 1955), 498-503. The ascription had to be taken on “faith,” since neither he nor the Folger had, at that time, adequate samples of Dering’s hand. When furnished such samples in 1983, Yeandle wrote, “How kind of you to send me dated examples of Dering’s handwriting covering twenty years of his life. I am glad to have these.” (30/08/1983) Those samples document that Dering wrote, throughout his life, in a transitional secretary hand, distinctive from Hand A of the manuscript. W. R. Streitberger, Professor of English, University of Washington, came to the same conclusion in 1983 when asked for a written opinion, “Dering’s hand can be separated from Hand A...[it was] transitional,” as did the archivists at Maidstone and Anne Oakley, formerly the archivist of Canterbury Diocese, when asked conversationally in August of 1988.

<sup>9</sup> Op.cit., p. [xxi].

<sup>10</sup> Op.cit.

<sup>11</sup> Ibid.

<sup>12</sup> Ibid.

<sup>13</sup> S. B. Hemingway, Henry the Fourth Part One, New Variorum, (Philadelphia, 1936), pp. 495-501; Craig, Ibid.; and Louis Ule, A Concordance to Shakespeare’s Apocrypha, 3 Vols., (Greg Olms, Vertag, 1988), see Introduction, which cites my work on D.

<sup>14</sup> Ibid., pp. xiii-xiv.

<sup>15</sup> Ibid., p. xvi.

<sup>16</sup> Ibid., xvii.

<sup>17</sup> See Dewey Ganzel, 1982.

<sup>18</sup> See Knight’s discussion in Romeo and Juliet, New Variorum, Horace H. Furness, ed., (American Scholar, N.Y., 1963, reprint of 1871 edition, with a bibliography by Louis Mauder), p.416. Knight writes, “we know of nothing in literary history more... instructive, than the...minute attention... consummate skill, exhibited by S[hakespeare]., in correcting augmenting and amending the first copy.[Q1] of this play.”

<sup>19</sup> Alfred Harbage, The New York Times Book Review, Vol, CIV, No. 35, 568, June 12, 1955, pp. 1,10-11, “Shakespeare’s plays are not learned...are even less aristocratic than learned.”

<sup>20</sup> Craig and Albert B. Weiner offer cogent critiques of these peculiar notions regarding “piracy” of Shakespeare’s play in their critical edition of The First Quarto of Hamlet 1603, 1962. They document the Folio’s claim to have replaced copies “maimed and deformed by the frauds of stealthes of

injurious impostors” was commercially motivated, since Folios as a general rule simply reprinted quartos.

<sup>21</sup> We have cited Knight above. See also Swinburne, A Study of Shakespeare. (Chatto & Windus 1909), p. 163. Swinburne stresses that Shakespeare worked through a process of revision, going over “scene by scene, line for line...all the old belabored ground again.”

<sup>22</sup> W. W. Greg, Editorial Problems in the Folio, (Oxford, 1955), 95.

<sup>23</sup> Ibid.

<sup>24</sup> Ibid. See also Craig’s and Weiner’s remarks in the introduction to Hamlet, op.cit.

<sup>25</sup> First Folio, (London, 1623), A3; The Norton Facsimile The First Folio of Shakespeare, (Paul Hamlyn, London, 1968), Charlton Hinman, ed., p.7.

<sup>26</sup> Laetitia Yeandle, “The Dating of Sir Edward Dering’s Copy of ‘The History of King Henry the Fourth,’” Shakespeare Quarterly, 37, 1986, pp.224-6. The TLS has not covered this important debate.

<sup>27</sup> G.B. Harrison dates Part Two to the spring of 1598/9. Works, p. 653.

<sup>28</sup> Wilson writes, “Saturn and Venus are never conjoined,” (Johnson), Henry IV Part 2, (Cambridge University Press, 1968), 166.

<sup>29</sup> Distant Suns, Virtual Reality Laboratories, Inc., 1992, R. Michael Smithwick. Dance of the Planets is ARC Science Simulations, 1993.

<sup>30</sup> The programs compute several conjunctions during the period, but only one corresponds to a sidereal location in the Fiery Trigon.

<sup>31</sup> Elizabethan dates are confusing. They lagged ten days behind European dates for nine months and a year and ten days behind between 1 January and 25 March, when England began its new year. Thus, 15 March 1594 in London was already 25 March 1595 in Europe, where almanac dates were calculated. So when Falstaff suggests we check our almanacs for this year, he could have been in 1594, while the 1595 almanac was already operant.

<sup>32</sup> See notes 6 and 7 above.

<sup>33</sup> Facsimile Edition, Op.cit., vii-viii, the editors write:

The scenes transcribed from Part I of Shakespeare’s play derive in direct transmission from the fifth quarto, The History of Henrie the fourth, printed in London in 1613. This assertion is based on two characteristics of the manuscript: (1) the closeness of the punctuation of page I of the manuscript to that of the quarto, and (2) the fidelity of the manuscript to two readings unique to this quarto. The first of these is at 3.3.80 (Globe III,iii,100); here Q5 (uncorrected) lacks a pronoun before the phrase “would cudgel him,” and the scribe, recognizing an omission, left a small blank space for Dering to supply

the correct form. The second is at 4.2.76 (Globe V.ii.76); Q5 prints “fellow’s souldiers,” and the scribe copied the erroneous, misleading apostrophe.

The scenes transcribed from Part II of the play derive from the second issue of the single quarto, The Second part of Henrie the fourth, printed in London in 1600. This assumption is based on the inclusion in 5.2 of selections from the King’s soliloquy on sleep (III.i), omitted in the first issue; on the fact that none of the lines unique to the Folio of 1623 (the second edition of the play) appears in the manuscript; and on general fidelity of the manuscript to the characteristics and manifest errors of the quarto...[except] for these the scribe presumably copied Dering’s holograph.

The bibliographical evidence is conclusive that the copytext for Part I was Q5 and that the copytext for Part II was Q (second issue).

<sup>34</sup> Op.cit., vii-viii.

<sup>35</sup> Facsimile Edition, unnumbered sheet [xii].

<sup>36</sup> Elizabethan Handwriting: A Style Manual, (Norton, 1966) p.104.

<sup>37</sup> Facsimile Edition, viii.

<sup>38</sup> Hardin Craig, A New Look at Shakespeare’s Quartos, Stanford University Press, Stanford, California, 1961, 43-51 and 112-117.

<sup>39</sup> T.N.S. Lennam, “Sir Edward Dering’s Collection of Playbooks, 1619-1624,” Shakespeare Quarterly, XVI (Spring 1965), 145-53.

<sup>40</sup> Prosser, op.cit.; Bantam Classics, Henry IV, Part One and Part Two, 1988, N.Y. and London; The History of Henry The Fourth [Part 1], The Folger Library General Reader’s Shakespeare, Washington Square Press, 1960. That Wright is circumventing Craig is clear, for he provides his readers with an annotated listing of sources, including one by Evans, without mentioning Craig.

<sup>41</sup> Craig, A New Look at Shakespeare’s Quartos, op.cit., 112.

<sup>42</sup> Ibid., 112.

<sup>43</sup> Hamlet, op.cit., i. Weiner’s introductory analysis destroys the view that Shakespeare’s plays, alone among Elizabethans plays, were ever pirated. His analysis shows the first quarto of Hamlet to have been an authorized stage abridgement. The most obvious proof of this is the equality and distribution of lines among the various actors, p. 49.

<sup>44</sup> Shakespeare for Everyman, Washington Square Press, N.Y., 1964, 96.

<sup>45</sup> Op. cit., p. iii. The argument shows Q1 to have been an edition abridged for acting, based on the assignment of the remaining lines.

<sup>46</sup> Facsimile Edition, Introduction, unmarked page.

<sup>47</sup>Facsimile, op.cit., vii.

<sup>48</sup> Hilton Kelliher, manuscript authority for the British Library and author of "Contemporary Manuscript Extracts from Shakespeare's Henry IV, Part I," English Manuscript Studies, 1100-1700, 1 (1989) 144-81, agreed on this point during a discussion held in his office in August 1988. He pointed out several similar manuscript examples, where such a "tie" was used, including one in a manuscript by Sir John Harington. The device connects the two hands in a "lock" and serves to indicate to a third party familiar with the first hand that the second hand is working for him.

<sup>49</sup> See Greg, First Folio, 266. As a matter of fact, bibliographic scholars only assume the sequence of the omission; it remains possible it was taken out of the second issue, rather than left out of the first.

<sup>50</sup>Facsimile Edition, Introduction, unnumbered page.

<sup>51</sup> Fredson Bowers, The Complete Works of Christopher Marlowe, Cambridge, 1973, Vol. 2, 3, footnote 3.

<sup>52</sup>Ibid., 3, "That the transcript was not made from the 1598 quarto is clear from the readings..."

<sup>53</sup> See Hilton Kelliher's discussion, op.cit.

<sup>54</sup> Yeandle, "The Dating of Sir...Dering's Copy...", op.cit.

<sup>55</sup>Ibid., 225.

<sup>56</sup>Ibid., 224, "there are only 56 leaves in the manuscript..."

<sup>57</sup> Personal conversation. Streitberger, a professor of English at the University of Washington, has been consulted on this problem for well over ten years and offered many valuable suggestions for which I am much indebted.

<sup>58</sup>Op.cit., "The Dating of..." 224-6.

<sup>59</sup>Op.cit., Facsimile Edition, first unnumbered page, footnote 3.

<sup>60</sup> "Carington's will in the Kent Archives Office (Reg. PRC 16/232 dbl 37) does not appear to be in the same handwriting as the play," 225.

<sup>61</sup>Ibid.

<sup>62</sup> Yeandle's letter to this author, 27 September 1988, admits "you have shown that the Mr Carington whom Sir Edward Dering paid to copy the Henry IV manuscript was not the clergyman...." Not one to concede, she goes on to suggest Dering's Mr. Carington must not have been his friend, but another Carington. Carington's church register is Canterbury Diocese,

U3/136/1/1 f.43, was kindly furnished to me by Anne Oakley.

<sup>63</sup> Ibid., 225, footnote 4.

<sup>64</sup> Prosser has noticed that the opening of Part Two, the point where the two plays join in D, looks like revision in Q. She writes, “the Prologue of Rumour seriously undercuts the first scene...on the whole, the poetry is uninspired...” Op.cit., 172. This suggests Q is an expansion of D.

<sup>65</sup> Ibid., 115-6.

<sup>66</sup> Henry IV stood for registration on 25 February 1598/9, the last Friday in February. In 1623, the last Friday in February fell on the 27th, i.e., the same week/month day as Dering’s payment for his copy. Someone was keeping track.

<sup>67</sup> See Blount’s dedication to Hero and Leander, 1598.

<sup>68</sup> Op.cit., Shakespeare’s Anonymous Editors.... Greg disallowed this (267) in order to simplify his analysis, but Hinman and others have accepted Prosser’s evidence.

<sup>69</sup> Some authorities have suggested that since more copies of Part Two have been preserved, that Part Two was rare at all. This view is speculative, however, and the fact remains that Part Two had only a single issue, whereas Part One was issued many times.

<sup>70</sup> The calculation is as follows: Dering paid 1 1/2d per sheet, making the manuscript 32 sheets. This number implies 64 folio leaves or 128 pages as normally numbered. Since Part Two of Henry IV is 3,180 lines in Q (according to Williams) and 3,322 in Folio, according to Hinman, this implies that if Carington maintained 51 lines per page, the transcription of Henry IV, Part Two would have fit precisely within such a manuscript. Carington’s will, proven in holograph, contains 55 lines on the same sized sheet as D (self-ruled) and his church register 44 lines (unruled and also the same size) with considerable white space. Thus 51 lines per sheet was about average for Carington.

<sup>71</sup> Op.cit.; Shakespeare’s books and papers did not go to his family. The consensus assumption hold they were left to his theater, i.e., to Fletcher. In fact, Evans believes several of Shakespeare’s books later belonged to Dering. These, as with D, he must have obtained from Fletcher.

<sup>72</sup> We have not discussed the scrap. It is curious that Christian materials were excised from Q for D, supposedly to be replaced by materials from the insert, which were overtly pagan. The scrap is why D has been thought to date to 1622. However, it may be spurious, dating to Dering’s 1623 interest in the Curate. Forensically, one should determine which side of the scrap was first transcribed. Such a study indicates that the Curate’s character list was first copied onto its face and, later, the verso material. How was this

notion arrived at? Examination of the ink stains indicates ink from the Curate's materials soaked through to the verso and was written over.

<sup>73</sup> Fletcher also attended Marlowe's college and a plaque there states they shared rooms at Corpus Christie, Cambridge.

<sup>74</sup> DBN.

<sup>75</sup> See 1623 Folio, op.cit. No discussion of this is really clear, but the double pagination is obvious when seen.

<sup>76</sup> For some reason Prosser counts "thirty-nine" pages, Op. cit., p. 73.

<sup>77</sup> Greg, Op.cit., 68, "negotiations [for H4] appear to have been difficult." See also his discussion 443-4. Greg writes, "the only thing to do was to calculate the amount of space needed...and proceed at once with Henry V."

<sup>78</sup> Based on computer typesetting projections, D could have fitted, particularly if the space was tallied not by the actual number of pages lying between 45 and 69 (23), but by the common compositor's mistake of counting so as to include 45 and 69 into the count, i.e., producing a total projected space of 25 pages, as one does if one counts on one's fingers. Hinman allows 132 lines per side, or a space of 3,290 lines. D, containing 3,401 lines, would have fit with slightly over 100 run-on lines. (See, Hinman, op.cit., xvii.)

<sup>79</sup> Greg, Editorial Problems, Op.cit., 264.

<sup>80</sup> Ibid., 266.

<sup>81</sup> Facsimile, op.cit., xi, "he did not add all that are necessary for a promptbook."

<sup>82</sup> Facsimile ed, op.cit.

<sup>83</sup> H.M. 600. I am indebted to Mary Robertson, Curator of Manuscripts at the Huntington for our discussions and our correspondence on this subject.

<sup>84</sup> Indeed, as it is a holograph which originated outside the theaters, it will be seen that, unlike D, it doesn't follow the formalities of stage manuscripts.

<sup>85</sup> Facsimile Edition, Introduction, first unnumbered page, footnote 3.

<sup>86</sup> See cf. 21 above.

<sup>87</sup> Op.cit., 172-174.

<sup>88</sup> Ibid.

<sup>89</sup> A.C. Partridge, Orthography in Shakespeare and Elizabethan Drama: A Study of Colloquial Contractions, Elision, Prosody and Punctuation, University of Nebraska, 1964, 67-79.

<sup>90</sup> Many examples of “drift” can be found. See fols 2, 10, 23, 44, 48, and 55. The difference in these hands is so profound that to the novice they may appear to be different writers.

<sup>91</sup> Q0, a fragment of the first known quarto, was discovered after Qs 1-5 had been numbered. Rather than change these, scholars designated it Q0.

<sup>92</sup> See Eleanor Prosser’s, Shakespeare’s Anonymous Editors: Scribe and Compositor in the Folio Text of 2 Henry IV, (Sanford, 1981), p. 16. Prosser quotes Percy Simpson’s Proof-Reading in the Sixteenth, Seventeenth, and Eighteenth Centuries, and Simpson’s finding that “copy would always be returned to the author, or to the friend working for him, or, if the author were dead, to the person responsible for sending the work to press.” I am indebted to phone conversations with Ms. Prosser concerning her work on Part Two and its possible impact on D.

<sup>93</sup> Elizabethan Handwriting, 100, “the strongly spurred a[s and] c[s]...are survivals not often seen at so late a date. We might reasonably assume that the writer was not a very young man.” Herbert was born in 1580, Dering in 1598.

<sup>94</sup> Charles Hamilton, William Shakespeare and John Fletcher Cardenio or The Second Maiden’s Tragedy, Glenbridge, 1994. Hamilton kindly acknowledges my work on his manuscript in a postscript, 258.

<sup>95</sup> Hamilton also ties Cardenio to Shakespeare’s will, a tie tradition has resisted as the will’s obvious deficiencies would imply the actor incapable of having written the works.

<sup>96</sup> See Samuel A. Tannenbaum, Shaksperian Scraps and other Elizabethan Fragments, Kennikat, N.Y., 1933 and 1966, a slippery slope indeed.

<sup>97</sup> Harold Jenkins, The Structural Problem in Shakespeare’s Henry the Fourth. Methuen, London, 1956.

<sup>98</sup> Facsimile Edition, op.cit., ix.

<sup>99</sup> Op.cit. Weiner.

<sup>100</sup> Ibid., ix.

<sup>101</sup> Both are found in Facsimiles published by Cornmarket Press, London, 1969. Shrew was revised by J. Lacey.

<sup>102</sup> Studies confirm that when two writers collaborate, a second vocabulary can be detected by computers using Type/token ratio programs. A “Type”

being a word counted once, while “tokens” are counted in all appearances. “Pace” realized the fixed T/r “ratio” could be seen as a “rate,” i.e., a fluid quality which allows a text to be studied scene by scene, if need be, for evidence of revisions. It discovered that Marlowe and Shakespeare deployed Tokens among their types at a relatively steady rate independent of the length of their plays. Consequently Two Gentlemen of Verona and Henry IV, Pt. II, were produced within .01% of the same “pace” even though Two Gentlemen is nearly half the length of Henry IV, Pt. II. See footnote 94, “Pace, A Test for Authorship...”

<sup>103</sup> Several neologisms including “tristful” and “blighten” are found in D. The first as a gloss (f20), the second embedded (f45v). Both terms are superior readings. D’s “dew” and “due” (f51) is also evidence of a closer tie to the author’s wordplay and meaning than Q. Even modern editions persist in error here, printing “due” twice, see IV.v.37-41. The lines should read, “this dew from me is tears...My due from thee is this imperial crown.” D reads “thie,” an easy mistake for “this.”

<sup>104</sup> What is represented in D, the point where the two texts diverge, will be found on f42r. Though much material is missing and the scene looks radically different as a result, when studied it will be noticed that the lines on f45r are either whole lines or fragments of lines to be found in the Quarto. So again we find no evidence of a second writer. We do find evidence of Shakespeare in the wonderful stage direction “alone in his garden and Night-Cappe:” and the line “But yett my hart is dull:, & slowe believe Takes but faint houldinge:” and afterwards “My sad heart saies,” and “O thie sad brow:,” These expressions are not required here if D is a condensation, and smack of authorial presence and sensitivity.

<sup>105</sup> Op.cit., A Concordance to Shakespeare’s Apocrypha and my article, “Pace: A Test of Authorship Based on the Rate at which New Words Enter an Author’s Text,” Oxford’s Literary and Linguistic Computing, Vol 3, No.1, 1988, 36-39.

<sup>106</sup> Could “let us make head” have been “let us take heed?”

<sup>107</sup> Op.cit., 132, note to line 189-209.

<sup>108</sup> This gloss may date to 1623 when D was considered for insertion into the First Folio, and thus may be Dering’s.

<sup>109</sup> Greg, Editorial Problems..., op.cit. Greg lists all three (153), but does not reference D. Indeed, he dismisses the possibility of a pre-Folio transcript, even though he knew of D’s conjectured 1622 date.



# Reviews

## Harold Bloom and His Discontents

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**The Western Canon: The Books and Schools of the Ages**  
by Harold Bloom (Yale University Press, 1994)

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*Reviewed by Warren Hope, PhD. Dr. Hope is author of The Shakespeare Controversy.*

Back in the 1920s, the novelist Ben Hecht and his perpetually impecunious friend Maxwell Bodenheim, the poet, agreed to stage a literary debate for pay. Hecht and Bodenheim mounted the podium in front of a gathered crowd of literary devotees in Chicago. Mr. Hecht announced the subject of the debate: "People who attend literary debates are fools." Mr. Bodenheim stated that he would uphold the affirmative, pointed to the attentive audience and declared, "I rest my case." Mr. Hecht conceded and they rapidly vacated the premises to eat and drink well on the fee they had collected.

I found myself yearning for those simpler and gaudier, not to say more honest times, while wading through Harold Bloom's self-important, immodestly entitled and thick book: *The Western Canon: The Books and Schools of the Ages* (1994). This yearning became most poignant when Bloom diverted the main stream of his narrative to explain away Sigmund Freud's opinions on the Shakespeare authorship question. The betrayal of a master by a disciple is never a pretty sight. But when the disciple resorts to misrepresentations of fact and ponderous jokes at his master's expense, you seem to be witnessing that decline in literature, criticism and scholarship that Bloom pretends to oppose.

Harold Bloom has established for himself a pleasant and profitable line of criticism. He accuses authors of the critic's occupational disease—anxieties over literary influence. Even his name for his imaginary or, if you prefer, rhetorical condition shows his debt to Freud.

More than that, though, Bloom is not only anxious about literary influence. He is also apparently terrified of competition. All those other schools of criticism that have recently sprung up like mushrooms are gaining adherents and he lays about him with resounding phrases to exorcise their hold on people—New Historicists and Feminists are written off as the School of Resentment and he hopes to deliver them a severe blow by linking them with zanies who do not even exist so far as the academic world is concerned, those poor befuddled "partisans for the idea of Sir Francis Bacon or the Earl of Oxford as the true author of *Lear*."

No critic these days says in print what he thinks. That would be simple-minded, naive and unprofessional. Instead, critics engage in rhetorical strategies that forward their scholarly enterprises. In short, they are con men, forgive me, con persons, shaking the plum tree. Bloom's attempt to preserve his pet plum tree by attacking his competition leads him into one difficulty. Sigmund Freud, his master, his mentor, was one of those zanies who thought Edward de Vere, the seventeenth Earl of Oxford, wrote the plays and poems attributed to William Shakespeare.

What to do? No problem. Bloom can publicly ridicule his master and forward his own brand of criticism by plucking out the root of Freud's aberration and declaring it to be (surprise, surprise) "literary influence and its anxieties."

But why must Freud's view of Shakespeare be aberrant? Only because Bloom does not share it. He does not refute the Oxfordian theory of authorship or even raise serious doubts about it. He merely declares the theory "crazy," not a technical term Freud reached for with frequency.

Bloom's case for the craziness of the Oxfordian theory rests on two points.

First, the name of the man responsible for the theory was J. Thomas Looney. You might think someone named Bloom would take a pass on reviling writers because of their names. But no. Bloom shows how much higher his regard for evidence is than Freud's by indulging in elephantine and juvenile jokes—"the Looney hypothesis," "Freud's Looney fantasy," "nothing could be loonier," and so on.

Second, as Bloom repeatedly puts it, "It did not matter that the Earl of Oxford was dead before *Lear* was composed..." We know the earl died in 1604. We do not know when *Lear* was written. Its date is a matter of faith. And Bloom wholeheartedly places his faith in the traditional dating of the plays, worked up and revised repeatedly for the past one hundred years by scholars who must try to fit the writing of the plays to the dates of the supposed author's life.

While we know when some of the plays were first performed or published, all we can say with certainty is they must have been written sometime before those dates. The dating of the writing of *Lear* is sheer guess work—guess work which, to the faithful like Bloom, becomes knowledge, hard irrefutable fact.

Bloom has the mendacity to pretend that Looney sought to solve the problem posed by the dates of composition of the plays by arguing that the late plays were finished by Oxford's friends after his death. Looney made no such claim. He simply concluded that the scholarly fabrication of the dates of composition for the plays was wrong, mistaken. Humanity's strength rests, he like Freud realized, in its ability to learn from its mistakes. Bloom's shrill illusion of infallibility would seem silly by comparison if it was not an attempt to doom the race to ignorance.

For a scholar to transform fiction into fact in order to publicly ridicule a thinker like Freud calls for no Freudian explanation. Freud had the audacity to challenge the authority of Harold Bloom and his fellow English professors. And that is why Bloom has to go out of his way to attack him, unfunny jokes, misrepresentations of fact, and all.

Still, there is much to be learned from this display of rhetoric posing as criticism and of slipshod debating tactics posing as scholarship. The life of the mind is not being attacked by barbarians at the gates, despite Bloom's lamentations to the contrary. It is instead daily betrayed by those well within the gates who draw their pay for ostensibly defending it. No wonder taxpayers are easily convinced by politicians that they should stop funding the scam. It would be less harmful and more entertaining if Bloom had the high spirits and honesty to point at his audience, declare them fools, take the money, and run.



