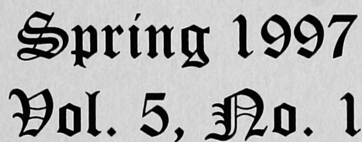
The Elizabethan Review



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The Elizabethan Review — Spring 1997

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Letters to the Editor

Shakespeare in Germany and Austria

To the Editor:

It might be interesting for American readers to hear about activities here on the European continent regarding the Shakespeare Authorship Question.

Things are moving, albeit slowly, here in Europe; and in the right direction, at least as far as Shakespeare is concerned. I had the pleasure of contributing Oxfordian material to the programs of two Austrian theater productions of Shakespeare plays. In Innsbruck, the Kellertheater staged a play called *Cordelias Traum* (Cordelia's Dream), a variation on *King Lear*. They printed a few extracts from my book, *Das Shakespeare-Komplott* (The Shakespeare Plot), in their program. In Salzburg, the Elisabethbuhne staged *Twelfth Night* and asked me to contribute an article on the Authorship Question to their theater magazine, as well as a short text on the possible dating of *Twelfth Night*.

For the first time, a German university professor has expressed interest in the problem and dealt with it in his class on English literature. Professor Werner Bleyhl, of Padadgogische Hochschule Ludwigsburg, included *Das Shakespeare-Komplott* in his seminar on Shakespeare, and informs me that his students were convinced that the Earl of Oxford is the real author of the Shakespeare canon.

Austrian television is producing a 50-minute documentary on the Authorship Question, which is being co-produced by Arte, the German-French culture channel, and the BBC. The documentary is scheduled to be broadcast sometime in 1997.

Barbara Denscher, of Austrian State Radio (ORF), is making a one-hour program on the Authorship Question to be broadcast in early 1997 in a popular series called "Tonspuren. Dokumente zur Literatur." Also, Robert Detobel of Frankfurt has written two lengthy radio programs on the Authorship Question; one has been broadcast by Hessischer Rundfunk, the other has been commissioned by another state radio, Westdeutcher Rundfunk.

Sincerely,
Walter Klier
Innsbruck, Austria

Walter Klier is a writer, journalist, co-editor of the literary quarterly *Gegenwart*, and author of *Das Shakespeare-Komplott* (1994).

Marston, Derby and Shakespeare

To the Editor:

In reference to the probing article, "What did Marston know about Shakespeare?", by professor Patrick Buckridge (ER, vol. 4, no. 2), and his proposition on page 39: "He (Marston) knew that the more active member of the Shakespeare partnership, at least at the time that he was writing satires, in 1597-98, was William Stanley (6th Earl of Derby)..."

In correspondence with the present Earl of Derby, he has advised me:

"Though books have been written trying to prove that Shakespeare was my ancestor, this is a thing that has always been firmly denied by the family. However, the view has always been taken that Lord Derby was a great traveller and that it is highly unlikely that Shakespeare would have travelled. It has always been felt therefore that it was my ancestor who supplied Shakespeare with a great deal of background information for his plays and I think this could be highly probable." Signed: Yours sincerely, Derby.

Sincerely,
Derran K. Charlton
South Yorkshire, England

Patrick Buckridge responds: I find it interesting that the present Derby family (who I believe are Stanleys, though not by direct descent) exercises such a firm "self-denying ordinance" on the authorship question, and wonder if there's anything behind it apart from a reluctance to make waves in a Stratfordian world. Judging from Will Stanley's life-story, that reluctance might be a family trait—perhaps even extending, in his case, to the greatest self-denial in history! Maybe the present Earl is right, but I think there are some connections between his illustrious ancestor and the author of the Shakespeare canon that the "background-briefing" hypothesis doesn't quite explain.

Dering's *Henry IV*

To the Editor:

John Baker's claim to have found Shakespeare's MS of *Henry IV* (ER, vol. 4, no. 1) goes into considerable detail and is impressive but not conclusive except as establishing a possibility, it seems to me. The difficulty is that we have no yardstick by way of an authentic piece of Shakespeare's writing (however you spell him) to enable us to lay anything else alongside so that we can conclude to identity of hand. So one reads with puzzlement Baker's statement (p. 15; cf. p. 29), "handwriting still cannot be employed for the purposes of identification, unlike fingerprints." This is only true of MSS in Secretary hand, produced by professional scriveners, which could be described as Elizabethan typewriting. Other hands were very distinctive, even when attempts were made to disguise

them, as in the case of the celebrated Monteagle letter, which was almost certainly written by the Earl of Salisbury. (See my books on the Gunpowder Plot.)

However, there is a curious instance of Lord Burghley's writing which passes from a relative'v round hand to the curiously angular style which was typical enough to identıfy his hand on all other occasions (see illustration, *The Dangerous Queen*, p. 400). I do not know of any other instance of this conscious or unconscious attempt to display virtuousity, or why Burghley did it. The letter is a copy, or alleged copy, in his hand of a letter from the 4th Duke of Nortfolk to Queen Elizabeth. However, we may concede to so much trouble taken on Baker's part that "proponents of dependence concede that D wasn't a prompt book or foul papers. Could it then be an 'authorial fair copy'? The answer is yes" (p. 24). Again, "...palaeographically... D is a composite manuscript composed of sheets from several drafts of *Henry IV*. Generally, only authors posses such remainders" (p. 28). It may well be so but one would like some references for this.

Sincerely,
Francis Edwards, S.J. FSA
London, England

A Groatsworth of Wit

To the Editor:

W. Ron Hess's article, "Robert Greene's Wit Re-Evaluated" (vol. 4, no. 2), has too many red herrings–like whether Greene wrote the book all at once or not–and strained thinking. It seems to me that the "upstart crow... with his *tygres heart wrapt in a players hyde*" has to refer to Shakespeare, especially as it's coupled with the italicized pun on "Shakespeare," *Shake-scene* (which is also capitalized, and was written by a man who elsewhere had referred to Marlowe as "Merlin").

Oxfordians are thus left with just three reasonable arguments: (1) Oxford acted under the name Shakespeare and Greene was audacious enough to call him an "upstart crow"; (2) Oxford was Shakespeare the actor/dramatist and Greene thought he was really a man named Shakespeare; or (3) Greene was part of the conspiracy to make people think Shaxsper was Shakespeare.

Sincerely,
Bob Grumman
Port Charlotte, Florida

Plucking the Tudor Rose

To the Editor:

Diana Price's article in *The Elizabethan Review* (vol. 4, no. 2), "A Fresh Look at the Tudor Rose Theory," was superb and should cause some individuals to abandon the Tudor Rose nonsense.

Sincerely,
Richard Edblom
Plymouth, Minnesota

To the Editor:

I hope the article by Diana Price in the autumn 1996 *Elizabethan Review* will convince the proponents of the Tudor Rose theory that they are wrong once and for all.

Sincerely,
Martha N. Walker
Baltimore, Maryland

To the Editor:

Few published elucidations of the true authorship of the Works Shakespeare have escaped me since first reading Looney in 1936, but I cannot recollect any so clear, concise, and meticulously documented as Diana Price's "A Fresh Look at the Tudor Rose Theory" in *The Elizabethan Review* (vol. 4. no. 2).

The Tudor Rose theory was introduced to me soon after this by London playwright Donald Anderson, as "a rattling good plot for a play," which he began but discarded "as distracting from the truth with a bizarre hypothesis which could grow into a fable and lead careless thinkers away from the recorded facts."

Congratulations also on Diana Price's simple explanation of the coronet that Lord Oxford sometimes scrawled over his signature, as seen on some English earls' envelopes today.

Sincerely,
Verily Anderson
Norfolk, England

To the Editor:

Diana Price's excellent article on the Tudor Rose theory seems to demolish fairly completely the theory that the third Earl of Southampton was the son of Queen Elizabeth and the Earl of Oxford. (Elizabeth Sears was the latest to propose this.) Most useful is the diagram showing the crown and various

coronets which explains perfectly the curious signature somtimes adopted by the Earl of Oxford.

Sincerely,
Francis Edwards, S.J., FSA
London, England

To the Editor:
In full support of the noetic article by Diana Price on the Tudor Rose theory:

If Southampton had been the illegitimate son of Queen Elizabeth and Edward de Vere, would they have chosen, as the foster-father of their child, a Catholic nobleman who had recently been imprisoned by the Queen for his complicity in a plot to dethrone her?

Why did Southampton so facially resemble Countess Southampton (when they were both in "the April of their prime") if he was not her son?

Most tellingly, would Edward de Vere have christened his legitimate son "Henry" (later 18th Earl of Oxford) when his "supposed" illegitimate son Henry Southampton was still living? To have done so would have been most irreligious. Edward de Vere was most religious, witness: "...he (Edward de Vere) was holy and Religious the Chapels and Churches he did frequent... and the bountie which Religion and Learning daily tooke from him, are Trumpets so loude, that all eares know them..."

Sincerely,
Derran K. Charlton
South Yorkshire, England

To the Editor:
In "A Fresh Look at the Tudor Rose Theory" (ER, vol. 4, no. 2), Diana Price has proved herself a most admirably thorough sleuth in her determination to disprove the finding that Southampton was the son of Queen Elizabeth and Oxford. That the youth was the fruit of such a union is a proposition I resisted for years for obvious reasons and have come to accept only because I have felt I had no choice. No other scenario of which I have heard accommodates the facts in the case.

Ms. Price, it seems to me, has scored a success in nearly all the challenges she has mounted to what she calls "The Tudor Rose Theory." The trouble is that these are all focused on subordinate issues while the central considerations are overlooked until at the end she touches on one and then only to shy away from it. Her argument, unless I mistake her, comes down to denying the possibility of Elizabeth's concealing a pregnancy during the crucial period and of her being able to bear a child in secret. My response to that is: let her think again. Plenty of women, I do not doubt, have succeeded in carrying out what Ms. Price maintains the Queen could not have. Given the costumes available

to a dame of the period and the sealing of gossipy mouths by the knowledge of what indiscretion could cost, I find no difficulty in believing that the Queen could have borne a child with only a few persons in on it. My mother, I might add, had a print of a full length portrait originally designated as of Queen Elizabeth with the subject looking suspiciously full in the midriff, which she told me was later labeled as simply that of a lady of the Court. (Elizabeth's pregnancy actually seems to be broadly hinted at in *Two Gentlemen of Verona* i.e., of One Vere, when a reference is made out of the blue, to Sylvia's "passing deformity," the consequence of Valentine's "present folly." Please see *The Mysterious William Shakespeare* (pp. 521-24). We may recall recent reports of the high-school couple in Delaware in which the young girl carried her pregnancy to full term and gave birth to an infant son in a motel without anyone's ever having been the wiser but for the discovery of the baby in a collection of trash, allegedly done to death by the young father.

How Ms. Price accounts for the terms in which the young poet addresses the young friend of the Sonnets is unclear to me. Yet this is of key significance. I have been unable to explain it except on the basis of the latter's having been either the poet's homosexual lover or his son and, somehow, his sovereign—and the evidence against the former interpretation is overwhelming and conclusive. But, given these alternatives, the poet was faced by a terrible dilemma. Fully expecting the fair youth to be identified as Southampton, presumably in a dedication similar to those of *Venus and Adonis* and *Lucrece* —"Thy name from hence immortal life shall have"—he could not possibly allow the youth to be seen by posterity as his catamite, while acknowledging him as his son would have been proscribed under the full power of the Crown. His solution? Sonnet 20, making it explicit that the young man, the object of both the poet's idolatry and censure, could not have been either. Thus, we are left with what A.L. Rowse calls "the greatest puzzle in the history of English literature." If, as I am constrained to believe—much against my will, I may repeat—that the identification of Shakespeare as Oxford must lead to that of the young friend of the Sonnets as the son of Oxford and Elizabeth, then the need for dissimulation of Oxford's authorship of Shakespeare's works was absolutely imperative. It was not simply a matter of preserving the reputations of the Queen and those around her, which would be recognized in the plays were these attributed to an insider at Court, though given the unsparing treatment of some of them this would be reason enough. What was at stake in the identity of the poet-dramatist was the succession to the throne of the United Kingdom. For all I know, this may be dynamite even today.

Let us come now to the events of June 24, 1604, which are of critical importance to our story. On that date, Oxford died and King James had the Earl of Southampton clapped in the Tower of London, from which James had released him following Elizabeth's death in 1603. Ms. Price would have us believe the two events were merely coincidental. That is surely incredible. She

asserts that if Robert Cecil and the King considered that Southampton had a claim to the throne as Elizabeth's heir, they would have left the young Earl in the Tower in 1603 or—Ms. Price shockingly attributing to Cecil and James a capacity for cold-blooded savagery—had him killed. Surely the facts are that James had Oxford's assurance in 1603 that Southampton would not claim the throne and could be safely freed, but that when Oxford died James feared that with his restraining hand withdrawn, Southampton might indeed make a bid for the throne. In his *Shakespeare and the Earl of Southampton*, G.P.V. Akrigg writes that "According to the French Ambassador, King James had gone into a complete panic and could not sleep that night even though he had a guard of Scots around his quarters. Presumably to protect his heir he sent orders to Prince Henry that he was not to stir from his chambers." Southampton was released the next day, no doubt upon his assurance that the King was entirely safe in having him at large.

The only explanation I can find is that, as Elizabeth's son, Southampton would indeed, certainly in his own view, have had a rightful claim to the Crown, upon which he might be expected to act, while to me this explanation accords with what we may deduce of the relations of Oxford, Elizabeth and Southampton from Shakespeare's works. I do not know how otherwise the circumstances known to us may be accounted for.

A final point. On the first page of her article, Ms. Price asserts that, were Southampton Oxford's son, Oxford in promoting his marriage with his daughter Elizabeth would have been encouraging incest unless, as proponents of the theory of the Tudor Rose have to argue, Elizabeth had come into being by Burghley's having "impregnated his daughter Anne." Let me refer her to *TMWS*, pp. 333-34, in which it will be seen that we have no reason at all to believe that Oxford favored a match between Southampton and Elizabeth.

Sincerely,
Charlton Ogburn
Beaufort, South Carolina

Diana Price responds: It is dismaying to find myself in disagreement with a position endorsed by Charlton Ogburn, whose book first interested me in the Shakespeare authorship issue. Yet his defense of the Tudor Rose theory does not squarely confront the factual objections, much less overcome them.

Mr. Ogburn may have "no difficulty in believing that the Queen could have borne a child with only a few persons in on it," but I do. An unnoticed pregnancy is not only unusual, but for a monarch with relatively little privacy, it is highly unlikely. Mr. Ogburn expressed confidence that "the sealing of gossipy mouths" could be ensured "by the knowledge of what indiscretion could cost" (a suggestion, by the way, that hints at the

same sort of "cold-blooded savagery" that Mr. Ogburn found so outrageous in my estimation of the Machiavellian Robert Cecil). A recurring topic of court gossip, both in England and on the continent, was speculation on Elizabeth's supposed pregnancies and illegitimate offspring. As far as we know, such gossip was without foundation, yet we are to believe that when there *was* some foundation, all the gossips suddenly went mum.

Who were all these potential gossips? Proponents of the theory have yet to comb the historical archives to catalogue Elizabeth's activities and personal interactions during the last half of her alleged pregnancy. What evidence shows that access to Elizabeth was restricted to those few who were "in the know"? Were documented personal interactions with non-insiders, such as the French ambassador and low-level courtiers, such as one of the Talbot boys, fabricated? If not, how was concealment possible in each circumstance? How can the Tudor Rose theory have any credibility when the critical assumptions on which it rests are based on mis-used secondhand evidence and an absence of primary research?

Mr. Ogburn claims that I focused "on subordinate issues while the central considerations are overlooked" until the end of the article. Is Mr. Ogburn seriously suggesting that the alleged royal birth of Southampton is a subordinate issue? Surely it is *the* central consideration. Mr. Ogburn argues that Elizabeth *could have* concealed her pregnancy — even in the presence of the French ambassador and minor courtiers — by wearing a dress designed for that purpose. Not only is the supposition questionable, it leaves many other objections unanswered. Mr. Ogburn speculates on Elizabeth's maternity disguise, yet he has himself been justifiably critical of reliance on conjecture when no facts exist, or worse, when known facts refute the case as argued. Similarly, his interpretation of the events of June 24, 1604 remains mere speculation.

With respect to the DeVere-Southampton betrothal, Mr. Ogburn points out that he is on record as having no reason "to believe that Oxford favored a match between Southampton and Elizabeth." But the chief promoter of the Tudor Rose theory believes otherwise. Mrs. Sears wrote that "Oxford would have realized at this point that a marriage to William Cecil's daughter/granddaughter would strengthen Southampton's position as heir to the throne. . . . Oxford must have regarded this marriage as a guarantee of Southampton's future inheritance of the Crown" (50).

Mr. Ogburn asks how I would explain the poet's address to the Fair Youth of the Sonnets, but he limits my choices to two. Either the Fair Youth was the poet's gay lover, or he was Oxford's son by the Queen. Mr. Ogburn's proposal strikes me as an example of the false dichotomy, a logical fallacy by which various legitimate possibilities in the spectrum are eliminated from consideration. And if a few historical facts render Mr. Ogburn's preferred option untenable, then is he not obliged either to modify

his hypothesis or to discard it in favor of another?

Mr. Ogburn began by stating that he reluctantly accepted the Tudor Rose theory because he "had no choice." No other explanations or interpretations would serve. Scholars have struggled for years to squeeze convincing interpretations out of the Sonnets, but those who have proposed unifying theories necessarily begin to speculate where the biographical records leave off. It may be that many more facts about the man who wrote the Sonnets will need to come to light before any interpretation can be proposed with confidence.

To the Editor:

All Oxfordians were originally Stratfordians. It was only the recognition of one or more non sequiturs in the Shakespeare story of authorship that caused us to search for an alternative author. Our questioning attitude, however, cannot be handily dropped when we study the life and works from the Oxfordian viewpoint and we are not serving the cause of Edward de Vere when we allow preconceived beliefs to interfere with examining new discoveries and new ideas. We do not have to recant our ideas of the universe like Galileo, nor will we be burned at the stake like Giordano Bruno who refused to recant. Having progressed beyond this kind of censorship, are we not free to report what we have found and believe to be true? There is one fact that must be looked at honestly and fairly. While other members of the nobility and gentry were published posthumously, Oxford was not. His records were destroyed and there has been an obvious cover-up for four hundred years. He states clearly that he was "Tongue Tied by authority" (Sonnet 66). This silence should be recognized as a non sequitur and dealt with accordingly.

Primary sources are admittedly important, but for the reign of Elizabeth I, there is a decided paucity of information. There was little that escaped William Cecil's censorship. While it was customary for the "pipe roll" records for any given year of a sovereign's reign to fill three large rooms, it is significant that in one year of the Regnum Cecileanum there were only nine pipe rolls; barely enough to fill a corner of one small room. This was not pruning of excess information, but a great lopping off, a comprehensive censorship.

Therefore, while primary sources are not always available, there were many veiled clues by writers using allegory and painters using Renaissance impresa or portrait devices to convey messages that could not be expressed otherwise. There was a British Secrets Act and a statute forbidding portrayal of high officials on stage. Allegory was used by most writers in Elizabethan times and it is naive to read Shakespeare, or anyone else in the Elizabethan era, in any way other than with double meanings. Oxford, with his ability to think and write in several languages at once, was the supreme master of double entendre and multiple meanings. To see him as less resourceful is to deny him one facet of his genius.

Ms. Price cites in her article, "A Fresh Look at the Tudor Rose Theory," Charlotte Stopes' foonote on page 2 of Chapter one of her Southampton biography:

> It has always been said he (Southampton) was "the second son," but there is no authority for that. The error must have begun in confusing the second with the first Henry (i.e., 2nd earl).

The second sentence above was not cited by Ms. Price, nor does she quote an important item from Mrs. Stopes's preface that might be relevant to Oxfordian research.

> From a plain statement of facts, however, we may sometimes secure legitimate inferences.

While the second part of the footnote quoted above seems to be one of those "inferences," it may be wrong in this case. In the small community that Tichfield was in the 16th Century, the word would have spread freely from the manor to the town that there had been two sons in the family with an unexplained disappearance of the first. Although there was no record of the burial of the first and no record of the birth of the second boy, such things would have been common gossip in the local pub.

Mrs. Stopes was somehwat handicapped by not having access to letters and papers that were still in private hands when she was researching Southampton. Among these were the Losely Letters which were turned over to the Castle Hill Museum in Guildford after World War II and later bought by the Folger Library. Unfortunately, these letters have been edited and omit what might have been crucial evidence. One of these, a letter from the Second Earl of Southampton to Sir William More of Losely, announcing the birth of a boy on October 6, 1573, has a blank section where the child's name might have been given. (Was this deliberately excised, a possible non sequitur?)

However, the Second Earl of Southampton's original will was surprisingly available for my personal examination at the Ancient Records Office in Winchester, to which Mrs. Stopes did not seem to have had access.

Two important items were added to the Earl's will after the main portion had been drawn up so self-servingly by his Gentleman of the Bedchamber, Thomas Dymocke. Dymocke had been trained at the Inns of Court, and was descended from a long line of Sovereign's Champions, at least as far back as Richard II. His uncle, Sir Edward Dymocke, had been Elizabeth's Ceremonial Champion at her Coronation. Thomas, who would eventually inherit the Barony, was acting as a servant (a non sequitur?). Ostensibly, there was not only a rift (Dymocke-made) between the Earl and his Countess Mary, but also a split with his in-laws. Lord and Lady Montague. Curiously, the first codicil added to the will was a gift to Lord Montague, "in token of perfect love and charitie betweene us" (non sequitur?).

The second addition provided for the education, until the age of twenty-one, of "William, my beggar boye." For this there may be an explana-

tion. If the boy, born on October 6th, 1573, was named for the Earl's devoted, Sir William More, who for many months had acted as the Earl's guardian/ warder when the Earl was first released from the Tower, this clause would have been added to ensure that his own ousted child would be saved from ignorance and desperate poverty. To quote Mrs. Stopes, "From a plain statement of facts, however, we may sometimes secure legitimate inferences." More non sequiturs in relation to these facts may lend them more weight!

If we return to Mrs. Stopes' book, we can find on page 9 more material of interest which she does not recognize as odd. She quotes a letter from the then widowed Countess of Southampton.

> ...Mr. Dymocke voyde of either wytte, abelity, or honesty to dischardg the same (i.e., the will) doth so vexe me as in troth my Lord I am not able to expresse. How to better yt I know no menes to her Majestie but by your menes to her to have consideracion of the man, and *great matters that resteth in his hands unaccomptable but by Her prerogative,* which I trust by your Lordship's menes to procure for the *good of the child.* (Italics added)

Dymocke has been given charge of "great matters" for the Queen (while ostensibly acting as a servant. Non sequitur!). On page 12, another letter from the Countess to Leicester speaks in the same vein of "the child" as opposed to "my child" or "my son."

> Yf possibly yt may be, which truly my Lord can never be (without great hinderance to *the child*) except such travell (i.e., travail) and paynes which may ever be taken *for yt,* as I know none can or will do, but he who is tyed to *the child* both in natur and kynship. That your Lordship shall judge my Lord, my father his meaning or myne, is not to make an *undutyfull motion to her Majestie or her state.*

The Countess speaks of "the child," refers to "yt," "great matters" and her duty to the Queen. (Non sequiturs?).

Ben Jonson speaks of matters in "High Places" in *Bartholmew Fair*, a play written in 1596, but *never performed until 1614.* (A non sequitur?) Act I, scene vi:

Zeal-of-the-land-Busy:... Now pig is a meat, and meat that is nourishing, and may be longed for, and so consequently eaten; it may be eaten: very exceeding well eaten. But in the *Fair*, and as a *Barthol'mew-pig* it cannot be eaten, for the very calling it a *Barthol'mew pig* it cannot be eaten, it cannot be eaten, for the very calling it a *Barthol'mew pig* and to eat it so, is a spice of ***idolatry***, and you make the *Fair* no better than *one of the **high places***. This I take it is the state of the question. ***A High place***. (Italics added)

In *Henry IV 2*, II.iv.250, Doll Tearsheet addresses Falstaff as "Thou whoreson little tidy Bartholmew Boar pig" and "Idolatry" has echoes of Sonnet 105.

Let not my love be call'd idolatrie
Nor my beloved an Idoll show
Since all alike my songs and praises be
To one, of one, still such, and ever so.
Kinde is my love today, tomorrow kinde
Still constant in a wondrous excellence,
Therefore my verse to constancie confin'd
One thing expressing, leaves out difference.
Faire, Kinde, and true, is all my argument.
Faire, Kinde, and true, varying to other words,
And in this change is my invention spent.
Three theams in one, which wondrous scope affords.
Faire, Kinde, and true, have often liv'd alone.
Which three till now, never kept seate in one.

The word "kind" has several meanings. Kind is German for child; it also has a now-obsolete usage meaning "sprung" or "begotten." Kine is an old word for "cattle" and in the cattle family is "Ox." One may claim this is far fetched, but the Renaissance mind worked this way. Hamlet says of Claudius: "A little less than *kind* and more than *kin.*

....Shakespeare was not the only poet who dedicated works to Southampton. Thomas Nashe wrote the following for his *Choice of Valentines*, which was found in an unpublished manuscript.

A prelude upon the name of
Henry Wriothesley Earl of
Southampton
Ever
Whoso beholds this leafe, therein shall reade
A faithful subjects name, he shall indeede
The grey-eyde morne in noontide clowdes may steep
But traytor and his name shall never meete.
Never.

....Oxford repeatedly expresses his pride in Southampton as the heir to the Crown. The Sonnets title page shows a device, or impresa, of a child wearing the Prince of Wales plumes at the top center. At the bottom are two hares (heirs). Above the first Sonnet is another heading with an impresa of two birds at the top, left and right (Phoenix and Turtle Dove) with an urn in the center. And finally, the portrait of the Earl of Southampton in the Tower is teeming with impresa, which would require a complete paper by itself.

Unlike Stratfordians, Oxfordians have a wealth of material to research. We must take advantage of our authorial view to search with open minds, no matter where it leads us. There must have been important reasons to hide the true authorship for four hundred years. Had it been a matter of "convention," Oxford's works would have been published under his own name

posthumously. There had to be a serious reason to hide him behind a pen name. Along with the obvious non sequitur of his *not* being given recognition, there is also the change in his combined *Crown and coronet* signature after Queen Elizabeth's death. The entire signature pictures a crown, the top part includes the coronet and Oxford is making a visual double entendre. The signature is really a Renaissance impresa. Obviously, it was critically important for him to discontinue its use after the Queen died, which is definitely another non sequitur.

Two final points: one is that on page 8 of Ms. Price's article, she mentions that "the death of Charles IX threw Anglo-French relations into fresh confusion. His death destabilized the marriage negotiations with the Duc D'Alençon..." Actually, she had been negotiating with the Duc *D'Anjou*, who, by the death of his brother, had become King of France (Henri III). Elizabeth could no longer play the game with Anjou and really was in a dilemma. The Duc D'Alençon was only 16 years old and it looked a little foolish to pursue him as a consort, but as there was no alternative, she finally did renew the French negotiations with a boy who was almost young enough to be her grandson. Of course, it was only a political ploy, but it worked for quite a few years, until Elizabeth was 54 and Alençon was dead. *Courtships of Queen Elizabeth*, by Martin Hume (MacMillan, NY 1896).

The other point is in reference to the interpretation of the Peyton report; it *did* indeed refer to Oxford, but Ms. Price skirts the issue it presents. It opens up a can of "Verma" that might lead to a revelation re "Ver sacrum."

Sincerely,
Elizabeth Sears
Somerville, Massachusetts

Diana Price responds: The debate over the Tudor Rose theory seems to be as much about critical thinking as it is about the lineage of Henry Wriothesley. Proponents of the theory have relied almost exclusively on literary interpretations, rather than hard facts, to build their case. One of my objectives in writing the article was to show that the theory had never been properly vetted against the existing historical documentation, even though it had gained uncritical acceptance in certain circles. I attempted to do a small portion of the vetting, and the evidence that I found, in my view, disproved the case as argued in Elisabeth Sears's *Shakespeare and the Tudor Rose.*

Mrs. Sears has now submitted her response, but nowhere has she confronted the challenges to the fundamental assumptions on which she based her case. Her theory hinges on the hypothesis that Queen Elizabeth had a baby in May/June 1574. But historical evidence shows that Elizabeth had no opportunity to carry or deliver the baby. I believe I also showed that related assumptions, e.g. that Oxford viewed himself as the royal consort;

that there was a second Southampton son; or that Southampton was viewed as a threat to James I, were based on mis-readings or inadequate evidence. Surely the burden is now on proponents of the theory to show why the evidence I presented is inadequate to support my conclusions, or to introduce new facts in support of their hypothesis that can be reconciled against the contradictory evidence.

Unfortunately, Mrs. Sears has ignored the factual impediments and reiterated her interpretations of the Shakespeare canon and a few documents, all of which are subject to other interpretations. Mrs. Sears might pile hundreds more pages of interpretations onto her theory, but if the underlying assumptions are proven to be factually untenable, no amount of literary interpretations or conjecture will make them tenable.

My article was critical of inaccuracies, misquotations, and lack of primary research in *Shakespeare and the Tudor Rose.* More such errors and gaps are found in Mrs. Sears's response. Some errors appear to be merely careless. For example, she misquotes her own book (56), confusing Thomas Powell's published dedication to Southampton for *A Welch Bayte to Spare Provender* of 1603, with Nashe's unpublished dedication to "Lord S" for *Choise of Valentines.* She misquotes a line from *Hamlet* (I.ii.65). In a final point, she writes that it was only when Charles IX died that d'Alençon was introduced as an alternative marriage candidate. Relying on historian Martin Hume, Mrs. Sears wrote, "Actually she had been negotiating with the Duc D'Anjou." But the negotiations with D'Anjou gave way to negotiations with d'Alençon over a year before Charles IX died. Fénélon "broached the subject" in March 1573. The following September, d'Alençon wrote to apologize to Elizabeth for missing their intended rendezvous at Dover; he also sent her a ring as a "love token" (Hume, 170-5).

Other errors show an absence of basic research. For example, Mrs. Sears cited the printer's ornamentation appearing on two pages of *Shake-speares Sonnets.* According to Mrs. Sears, Oxford:

> repeatedly expresses his pride in Southampton as the heir to the Crown. The Sonnets title page shows a device, or impresa, of a child wearing the Prince of Wales plumes at the top center. At the bottom are two hares (heirs). Above the first Sonnet is another heading with an impresa of two birds at the top, left and right (Phoenix and Turtle Dove) with an urn in the center.

Mrs. Sears is pointing to the ornaments in the 1609 edition of *Shake-speares Sonnets* as iconography using Tudor Rose motifs. This is interpretative nonsense.

If Mrs. Sears is suggesting that the edition was designed with a wink at Southampton's royal lineage, how does she explain the presence of the same artwork on any number of other books published before and after 1609? The ornament with the hares appears on the title pages for *A Mad*

World My Masters (1608), *The Merry Devils of Edmonton* (1608), and in Jonson's 1616 *Workes* over the title *Catiline*. The second ornament featuring the two birds appears in George Chapman's *An humorous days mirth* (1599) and *The Gentleman Usher* (1606), and in *Histrio-Mastix* (1610), among others. Obviously, these ornaments were the printers' clip-art, pulled from regular stock. Yet Mrs. Sears interprets them as specifically symbolic of the Tudor Rose themes that she finds in *Shake-speares Sonnets*. It is difficult to escape the inference that Mrs. Sears is prone to reading meaning into anything supporting her theory, while avoiding the critical analysis that might compromise such "evidence."

The errors in both Mrs. Sears's book and rebuttal cannot inspire confidence, but they pale beside her fundamental error of ignoring the historical evidence that disproves her hypothesis. Chauncey Sanders, the author of *An Introduction to Research in English Literary History* (Macmillan, 1952) offered sobering advice to any student who proposes a new interpretation to a work of literature (228-9):

> Let him amass all the evidence he can find. Let him set down, in orderly fashion, all the arguments in favor of his interpretation, and then, with equal or greater scrupulousness, all those against. Let him study the evidence, giving full value to every argument; for it may very well happen that a single bit of *contra* evidence will make the piling up of *pro* arguments like the adding together of zeros: whether there are twelve or twenty, the total is still zero. Having assured himself that he has a case, let the student then present his hypothesis, not as a revolutionary discovery that must supplant the quaint notions of his predecessors, but as a tentative suggestion for the consideration of those who may be able to bring further evidence to bear on the matter.

The founders of The Shakespeare Fellowship, among them Sir George Greenwood and J. Thomas Looney, clearly respected the critical method. In their 1922 statement of purpose, they expressed a "desire to see the principles of scientific historical criticism applied to the problem of Shakespearean authorship." It does seem that Mrs. Sears and her followers have gone off in the opposite direction.

A scientific method requires us to revise or discard a hypothesis when it is contradicted by documentary evidence, no matter how abundantly the theory may appear to be corroborated by literary interpretations. If proponents of the Tudor Rose theory do not accept that basic tenet of critical thinking, it is probably pointless for those who are skeptical to argue further.

A Debate on "D"

To the Editor,

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

I.iii. 79-84 . . . the foolish Mortimer, 79
Who, on my soul, hath wilfully betrayed 80
The lives of those that he did lead to fight 81
Against that great magician, damn'd Glendower, 82
Whose daughter, as we hear, the Earl of March 83
Hath lately married:

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

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

III.i. 24-26. Diseased nature oftentimes breaks forth 24
In strange eruptions, oft the teeming earth 25
Is with a kind of colic pinch'd and vex'd. 26

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

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

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

are almost by themselves proof against the independence of D.

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

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

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

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

Sincerely,
Gerald E. Downs
Redondo Beach, California

John Baker responds:

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

Motivation aside, I'm not surprised that Downs' bibliographic analy-

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

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

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

Indeed, in an effort to widen his case into paleography, which is part of the broader picture, Downs asserts it is not "at all significant that the

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

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

On this same issue, Downs attempts to cast doubt on the likely dates of the handwriting, noting "common sense says an older, provincial scribe could well produce the work in 1623." However, the point I made was that the scribe named by Dering in 1623 wasn't provincial and wrote in a lovely, quite remarkable Italic hand. Dering's own hand was a transitional hand, so the point stands that the hands of the manuscript are consistent with men born ca. 1565. Moreover, its total absence of transitional forms indicates a transcription date far more likely to be ca. 1592 than ca. 1623. Just consider the missing question marks. Q5 contains hundreds of *printed* question marks, but D doesn't evidence any. Why not? Even if Hand B was trained not to employ them, as these marks were just entering the language, an inadvertent mistake or two seems natural enough, but there aren't any. Why? As I noted in the monograph, Dawson and Yeandle, writing about transitional hands regarding Walter Bagot's letter of 1622, observed, "though essentially secretary, [it] exhibits a reduction in the exaggerated ascenders and descenders and so shows the writer to be a child of the seventeenth century." There are no such transitional elements in either of D's hands. Indeed, Dawson and Yeandle, writing about Hand B, conceded "the hand is...*pure* secretary." If Downs believes such a hand was writing in Kent in 1623, let him produce extended samples. I was not

able to find it while visiting the Maidstone archives. Nor was Yeandle.

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

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

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

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

However, take the reverse example. D could be a copy of Q5 even if it were *completely* different than Q5. We might imagine a dyslexic scribe who

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

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

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

Who Was Joyce's Shakespeare?

Gary Goldstein

As Vincent Cheng and other scholars have noted, James Joyce had a lifelong admiration for William Shakespeare, to whom Joyce compared himself throughout his life (Cheng 1). Indeed, this fascination led Joyce to incorporate into Finnegans *Wake* a thousand allusions to the person and works of his English rival... as well as to the claimants of Shakespeare's crown.

I offer these prefatory remarks because Joyce left provocative evidence in *Ulysses* and *Wake* that, thoroughly examined, enables one to hear the echoes and see the shadows of the man who may be Joyce's Shakespeare.

Ulysses

At the beginning of the Shakespeare chapter in *Ulysses*, Joyce dismisses Francis Bacon with dispatch. "Good Bacon: gone musty" (U 195). He then has a librarian spur on the conversation by declaring: "I hope Mr. Dedalus will work out his theory for the enlightenment of the public" (U 196). Joyce proceeds to do this by listing the Shakespeare authorship speculations of George Bernard Shaw and Frank Harris (U 196), Walt Whitman (U 201) and Samuel Taylor Coleridge (U 205). He then writes:

> Gentle Will is being roughly handled, gentle Mr. Best said gently.
> Which will? gagged sweetly Buck Mulligan. (U 206)

Joyce has his characters continue questioning the traditional authorship of the Shakespeare plays.

> When Rutlandbaconsouthamptonshakespeare or another poet of the same name in the comedy of errors wrote Hamlet... (U 208)

Joyce later has a character talk briefly about the theory that the Earl of Rutland had written the works of Shakespeare (U 214). Obviously exasperated with all the talk about Shakespeare's true identity, someone exclaims:

Gary Goldstein is editor of The Elizabethan Review.

> I believe, O Lord, help my unbelief. (U 214)

Despite this ironic appeal to God or a nobleman, Joyce still hadn't closed the discussion on who wrote Shakespeare, for he issues a final comment on the matter at the end of the chapter.

> Manner of Oxenford. (U 217)

The reference is to Edward de Vere, 17th Earl of Oxford (1550-1604), for Oxford had signed his poetry, in manuscript and in published form, as E.O., E. of Ox., or Earle of Oxenford.

What makes Stephen Dedalus's comment unique is the manner in which Joyce positions the statement. Until this point, Joyce doesn't mention Oxford; when he does, he turns it into the conclusive comment on the authorship of Shakespeare's works. As if to emphasize this, Joyce highlights the final but unexpressed thought of Stephen Dedalus about Shakespeare by making it a three-word paragraph.

After Dedalus is led to silently draw a conclusion on the authorship question based on the preceding conversation, he chooses not to share it with his friends, although Joyce shares this conclusion with the readers of his novel.

> Here I watched the birds for augury. Aengus of the birds. They go, they come. Last night I flew. Easily flew. Men wondered. Street of harlots after. A cream-fruit melon he held to me. In. You will see.
>
> The wandering Jew, Buck Mulligan whispered with clown's awe. Did you see his eye? He looked upon you to lust after you. I fear thee, ancient mariner. O, Kinch, thou art in peril. Get thee a breechpad.
>
> Manner of Oxenford.
>
> Day. Wheelbarrow seen over arch of bridge.
>
> A dark back went before them. Step of a pard, down, out by the gateway, under portcullis barbs.
>
> They followed. (U 217-8)

Joyce also highlights the paragraph's inference—that Shakespeare wrote in the Earl of Oxford's manner, or manor—by making it the only statement on Shakespeare in *Ulysses* not rebutted by another character, even in humor. Equally important, Joyce inserts the statement within the chapter on Shakespeare, a chapter written entirely in doubt about Shakespeare's identity. Earlier, Joyce has a character voice his concerns about that identity.

> Certainly, John Eglinton mused, of all great men he is the most enigmatic.We know nothing but that he lived and suffered. Not even so much. Others abide our question. A shadow hangs over the rest. (U 194)

The tenor of the preceding paragraph, especially its last sentence, echoes Hamlet's dying words as well as a contemporary comment about the Earl of Oxford's life, connecting the English Bard with the chief claimant to his title.

At the conclusion of Hamlet, Prince Hamlet prophesies that the new monarch will be Fortinbras, yet doesn't finish saying what the preceding events have prompted, thereby leaving behind a mystery. Thus, his dying words, "—the rest is silence" (V.ii.360). In commenting upon this line in *Ulysses*, Joyce uses the word "shadow" probably because it represents the physical and outer equivalent of the ear's silence.

Indeed, Eglinton's remark—"A shadow hangs over the rest."— directly echoes Dr. A.B. Grosart's published view of the 17th Earl of Oxford: "An unlifted shadow lies across his memory."

Grosart's edition of the Earl of Oxford's poetry, the first such collection, was published in 1872 in the Miscellanies of the Fuller Worthies' Library, Volume IV. J. Thomas Looney included Grosart's assessment of Oxford in his book, *Shakespeare Identified in Edward de Vere, 17th Earl of Oxford*, published in England in 1920 (Looney 155). Since *Ulysses* was later printed in 1922, it's likely that Joyce had read Looney's book and was conversant with the theory that the Earl of Oxford had written the Shakespeare plays and poems under a pseudonym.

Two well-known contemporaries of Joyce, novelist John Galsworthy and Sigmund Freud, both believed in Looney's hypothesis. Freud wrote: The man of Stratford... seems to haven nothing at all to justify his claim, whereas Oxford has almost everything" (Ogburn 146). Galsworthy handed out copies of Looney's book to friends, writing it up as "the best detective story I have ever read" (Ogburn 146). Such actions by Galsworthy, a contemporary and a literary peer of Joyce's, may have aroused the latter's curiosity to examine evidence in support of the hypothesis.

Such a proposition is borne out by the references to Oxford and Looney that Joyce incorporated into *Finnegans Wake*, a book published 17 years after *Ulysses*.

Finnegans Wake

Adaline Glasheen and other Joyce scholars have discovered that Joyce punned upon the names of Vere and Oxford in *Wake* at least half a dozen times, often combining allusions to Oxford and Shakespeare in his puns.

The first allusion to Oxford also alludes to his father-in-law, William Cecil, Queen Elizabeth I's principal advisor for 40 years, first as Principal Secretary of State (1558-1572), then as Lord Great Treasurer (1572-1598).

> ...cutting a great dash in a brandnew two guinea dress suit and a burled hogsford... (FW 182.26)

The pun refers to William Cecil who, by virtue of marrying his daughter Anne to the 17th Earl of Oxford, was created Lord Burghley by the Queen only months before the wedding in 1571.

Within the context of Joyce's sentence, one's first impression of the phrase "burled hogsford" is of a furled or closed-up umbrella. In fact, that visual pun corresponds to what transpired after Oxford married into the Cecil family.

Burghley acquired much of Oxford's wealth by undervaluing his lands while Oxford was his ward, from the time of his father's death when he was 12, until his 21st birthday and subsequent marriage to Burghley's daughter in 1571. Burghley later purchased these estates after Oxford sold them to finance his social and political obligations at Court. Burghley even ordered Oxford to pay an extravagant marriage fee at the age of 40, after Anne had died, leaving Oxford destitute. Indeed, the family of Cecil would eclipse that of the Vere's politically, socially and financially during the lifetimes of both men, due largely to the efforts of Queen Elizabeth's all-powerful Treasurer and Secretary of State.

This reading is confirmed by examining the other puns about Burghley in *Wake*, several of which refer specifically to him as a "bully."

> Bullyclubber burgherly shut the rush in general... (FW 335.13)
> Bully burley yet hardly hurley... (FW 511.24)

In other references ot Oxford in *Wake*, Joyce abandons the Burghley connection and proceeds to praise Oxford's musical talents.

> And he can cantabb as chipper as any oxon ever I mood with, a tiptoe singer! (FW 467.31)

De Vere had signed his poetry and letters in a variety of ways: E.O., E. Ox., and Edward Oxenford. Moreover, de Vere often was referred to in state documents as the Earl of Oxon. Ever is an obvious pun upon Edward de Vere, as it represents a phonetic trace of his name: E. Ver.

Joyce also alludes to the musical reputation of Oxford, to whom Elizabethan composer John Farmer dedicated two books of compositions. Farmer, a native of Ireland, was at times an employee of Oxford's, as well as Organist and Master of the Children's Choir of Dublin's Christ Church Cathedral. Farmer's second book was dedicated to Oxford in 1599 as follows:

> Without flattery be it spoken, those that know your Lordship know that, using this science as a recreation, your Lordship have overgone most of them that make it a profession.

To the greatest composer of the Elizabethan era, William Byrd,

Oxford conveyed the manor of Batayles for 31 years in 1574. Additional evidence of Oxford's musical interests is reflected in the compositions named in his honor, such as *Earl of Oxford's March*, and *Earl of Oxford's Galliard.*

The phrase "a tiptoe singer" may also refer to Grosart's comment on Oxford's poetry that Looney included in his book. "They [Oxford's poems] are not without touches of the true Singer..." (Looney 155). As this quote comes on the same page in Looney's book that contains Grosart's other comment about "an unlifted shadow" lying across Oxford's memory, it points to Joyce having read Looney's book.

Joyce later makes an extended and significant reference to Oxford in *Wake* that also ties him to Shakespeare.

> From Daneland sailed the oxeyed man, now mark well what I say. (FW 480.10)

The "oxeyed" family of de Vere traced their ancestry to Zeeland in Denmark (Ogburn 417). Joyce then connects Oxford to Shakespeare by adding the phrase, "now mark well what I say," a phrase used by Shakespeare in all 37 of his plays. Throughout the canon, characters ask one another more than a hundred times to "mark" what they are about to say (Concordance), giving Joyce's sentence the imprimatur of the Bard himself.

Perhaps the most clear-cut and positive reference that Joyce makes to Oxford in *Wake*, and the entire Joycean canon, is the line:

> ...my dodear devere revered mainhirr was confined to guardroom... (FW 492.16)

The phrase represents a series of admiring puns on Oxford's name. In addition to "dear" and "revere" is the phrase "mainhirr," a multilingual pun on the Dutch and German expressions for "my dear sir"—mijn heer and mein herr—similar in pronunciation and meaning but not spelling. The phrase also provides another pun on "dodear." Moreover, playing on the German and Dutch with "main" offers up a final pun—my main gentleman—that broadens Joyce's praise of de Vere even further.

The phrase "confined to guardroom" also is historically accurate, for de Vere was confined to the Tower of London in 1581 for several months after Queen Elizabeth uncovered his liaison with Anne Vavasor, one of her ladies in waiting, who had just borne de Vere an illegitimate son, Sir Edward Vere (Ogburn 646).

Is Oxford being revered by James Joyce or by a character in *Wake*? Either way, it lauds him in a way that no other Shakespeare claimant was ever lauded in Joyce's works, including Bacon, Rutland, Southampton, and William Shakspere of Stratford on Avon.

Joyce also included in *Wake* two puns that refer to J. Thomas Looney, probably commenting on Looney's situation after publication of his book, *Shakespeare Identified*, which came under sustained public attack, along with its author. Note the line, "Loonacied! Marterdyed!" (FW 492.5), which precedes the previous explicit allusion to Oxford by just 11 lines.

Equally resonant is the line, "Loonely in me loneness" (FW 627.34). As Joyce placed this statement on the next-to-last page of *Wake*, perhaps Joyce was comparing Looney's experience with his own artistic situation vis-a-vis contemporary critics, to whom Joyce and his creative works aroused an intense and antagonistic response.

The preceding poetic evidence in *Ulysses* and *Wake* shows that Joyce had extensive knowledge about Oxford which he chose to include in his two masterpieces. It also shows that Joyce believed Shakespeare wrote in the Earl of Oxford's manner. Moreover, Joyce made his reverence for Oxford explicit in a willfully obscure book, *Finnegans Wake*. Equally important, Joyce connects Oxford to Shakespeare in allusions in *Wake*. Finally, as both books were published 17 years apart, the positive references to Oxford, spanning an entire generation of time, I believe represent much more than an awareness of the debate of who wrote Shakespeare.

Notes

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Why I'm Not an Oxfordian

David Kathman

William Shakespeare was baptized in Stratford-upon-Avon on April 26, 1564, and was buried in the same town on April 25, 1616. In between, he married and had three children, went to London and became a successful actor and theatrical shareholder, bought the second-largest house in Stratford as well as othe property, and wrote the greatest body of plays and poetery in the English language. For the last 150 years or so, a steady stream of writers, many of them quite intelligent but generally without training in Elizabethan literary history, have argued that William Shakespeare of Stratford did not write the plays and poems attributed to him, and that "William Shakespeare" was actually a pseudonym for the real author. In recent decades, Edward de Vere, Seventeenth Earl of Oxford, has become the preferred candidate as an alternate Shakespeare, with many Oxfordians arguing their case passionately in books, journals (including *The Elizabethan Review*), and most recently on the Internet. Despite their passion and industry, Oxfordians and their theories have generally been ignored by the mainstream Shakespeare establishment. When orthodox Shakespeare scholars have responded to Oxfordians, they have often done so in a dismissive and condescending way, leading to accusations that these scholars are afraid to face up to the "real issues" involved. Many Oxfordians believe that such a reaction is motivated by self-interest, and that only the formidable vested interests of the Shakespeare industry prevent Oxford's authorship from being universally recognized.

In this article I will try to explain some of the major reasons why mainstream Shakespeare scholars do not take Oxfordians seriously. I will not attempt to deal with every assertion which has been made by Oxfordians, because that would require at least a book-length treatment; many of the most popular issues are addressed on the Shakespeare Authorship Page on the World Wide Web (http://www.bcpl.lib.md.us/~tross/ws/will.html), and have also been discussed vigorously on the humanities.lit.authors.shakespeare newsgroup.

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Instead, I will focus on Charlton Ogburn's book, *The Mysterious William Shakespeare*, using it both as a springboard for discussing larger issues and as a case study in Oxfordian methodology. Obviously, Ogburn does not speak for all Oxfordians on every issue, as he would be the first to admit; nevertheless, his book is generally accepted as the most thorough and detailed exposition of the Oxfordian position, and every serious Oxfordian is familiar with it.

First of all, it may be useful to give a summary of the reasons for the traditional attribution. All the external evidence says the plays and poems were written by William Shakespeare. A man named William Shakespeare, from Stratford, was a member of the acting company which put on the plays. Heminges and Condell in the First Folio explicitly say that their "friend and fellow" Shakespeare was the author of the plays, and a monument to his memory was built in the Stratford church. There was no other William Shakespeare living in London at the time. There is no evidence that anyone else, including Oxford, was ever known as "William Shakespeare". Shakespeare of Stratford was consistently recognized as the author after his death and throughout the seventeenth century. There were abundant resources in Elizabethan London for such a man to absorb the knowledge displayed in the plays, despite Oxfordian attempts to claim otherwise; furthermore, there is no documentary evidence to connect the Seventeenth Earl of Oxford with any of Shakespeare's plays or poems, despite the fact that Oxford's life is quite well documented.

All this is perfectly standard evidence of the type used by literary historians; indeed, the evidence that William Shakespeare of Stratford wrote the plays and poems published under his name is abundant compared to that for many of his fellow writers. Oxfordians, however, see such external evidence as an annoyance to be rationalized away; they have built up a picture of who the author must have been from reading the plays themselves, and that picture does not look like William Shakespeare of Stratford. A large part of the "evidence" used by Oxfordians is internal to the works themselves: reconstructions of what the author "must have" thought and what his background must have been like, and supposed allusions to events in Oxford's life, all taken from the plays and poems. Literary scholars have always treated such internal evidence with the utmost caution, especially when dealing with works written 400 years ago; interpretations are notoriously subjective, and whenever possible should be backed with external evidence. Indeed, such a great literary figure as T. S. Eliot recognized the unreliability of such reconstructions when he wrote the following: "I admit that my own experience, as a minor poet, may have jaundiced my outlook; that I am used to having cosmic significances, which I never suspected, extracted from my work (such as it is) by enthusiastic persons at a distance; and to being informed that something which I meant seriously is *vers de societe*; and to having my personal biography reconstructed from passages which I got out of books, or which I invented out of nothing

because they sounded well; and to having my biography invariably ignored in what I *did* write from personal experience; so that in consequence I am inclined to believe that people are mistaken about Shakespeare just in proportion to the relative superiority of Shakespeare to myself" (Eliot, 108).

Surely such testimony, especially coming from a great literary figure who lived to see voluminous criticism of his works, should make us cautious of relying too much on internal reconstructions of an author's life and opinions. For Charlton Ogburn, though, such internal "evidence" is primary, and if the documentary record does not support it, that is simply evidence that the documentary record has been tampered with. Ogburn is not a humble man; he is absolutely certain that his interpretations of Shakespeare are correct, and sometimes he seems genuinely baffled that any honest person could disagree with him. When he attempts to justify these interpretations using evidence and arguments, though, he invokes an enormous double standard — actually a series of double standards — in which completely different standards of proof apply to Ogburn and his opponents, and which renders his thesis essentially unfalsifiable. Everything about William Shakespeare of Stratford is put under a microscope and interpreted in the most unfavorable way possible; everything about Oxford is interpreted as favorably as possible. Ogburn throws out documentary evidence and ridicules even the most reasonable inferences made by Shakespeare's biographers if they do not accord with his preconceived notion that "Shaksper" was a greedy, illiterate boor; on the other hand, he freely engages in far more fanciful speculation about Oxford in the absence of any documentary evidence. Ogburn expresses strong disagreement with the opinions and interpretations of past Shakespeare scholars, which he has every right to do; but then he presents his own opinions as obvious truths which only a fool could disagree with. All in all, the relentless double standard pervading Ogburn's book makes it difficult to take anything he writes at face value.

For example, Ogburn relentlessly criticizes orthodox Shakespeare scholars for uncritically accepting what their predecessors have written, but he is even more guilty of uncritically accepting what previous anti-Stratfordians have written; he seems unwilling to apply any except the most trivial critical standards to Oxfordian arguments. The result is a disturbing tendency to confidently, even arrogantly, insist on the truth of statements which can be easily shown to be false, simply because they have been a part of anti-Stratfordian dogma for so many years. For example, in discussing the 1609 Quarto of the Sonnets, Ogburn asserts that the title "Shake-speares Sonnets", with the writer's name first, is "a plain indication that the author was dead" (206). He cites *Greenes Groatsworth of Wit* as an example and even invokes the orthodox scholar Sidney Lee in support of his claim. But a quick check of the Short Title Catalogue reveals that this claim is completely groundless. There are dozens of examples of the name of living authors coming first in Elizabethan titles, including *Brittons Bower of Delights* by Nicholas Breton

(1591), numerous works by Thomas Churchyard and Thomas Coryat (e.g. *Churchyardes farewell* (1566), *Churchyards challenge* (1593), *Coryats crambe* (1611), *Coryats crudities* (1611)), and even other works by Robert Greene, such as *Greenes farewell to folly* (1591).

Ogburn makes a similarly false claim when he insists that those who occasionally hyphenated Shakespeare's name in print "can only have been showing that they recognized Shakespeare as a pseudonym", and that "the hyphenation is so clearly inexplicable except as designating the name as fictitious that I do not see how there can ever have been any question about it" (98). Despite the vehemence with which Ogburn expresses it, this claim also has no factual basis. Even if we limit ourselves to title pages, it is possible to find numerous other hyphenated names which clearly referred to real people. For example:

* Charles Fitzgeoffrey's name was regularly hyphenated on the title pages of his works, published between 1596 and 1637 as by "Charles Fitz-Geffry", "Charles Fitz-Geffrey", or "Charles Fitz-Geffrie". Fitzgeoffrey's name was hyphenated much more regularly than Shakespeare's was, yet no one has suggested that he was using a pseudonym.

* When four of Phillip Henslowe's writers wrote a play about Sir John Oldcastle in response to the success of Falstaff, the printed version of the play had the title "The first part of the true and honorable historie, of Sir John Old-Castle, the good Lord Cobham."

* When Anthony Munday wrote a pageant in honor of Sir Thomas Campbell's installation as Lord Mayor of London in 1609, the title of the printed version was "Camp-bell, or, The ironmonger's faire field." (This is actually taken from the running title, since the title page of the only surviving copy is missing.)

* The printer of Munday's pageant, Edward Allde, was quite fond of hyphens, and in fact he often hyphenated his own name as All-de on the title pages of works he printed (e.g. Henry Fitzgeoffrey's *Satyres* (1617), Thomas Middleton's *The Sun in Aries* (1621), and John Bradford's *Holy Meditations* (1622)).

* Another printer, Robert Waldegrave, also regularly hyphenated his own name as Walde-grave on the title pages of works he printed from 1582 on. In fact, Waldegrave was the printer of the first four Martin Marpretlate pamphlets, written under the most famous pseudonym of the Elizabethan era. The pseudonym "Marprelate" is not hyphenated once in the many times it appears in the text of the pamphlets — but the name "Waldegrave", which also appears many times in the text, is hyphenated every single time.

Another anti-Stratfordian chestnut which Ogburn defends with unfortunate vehemence is the absence of Shakespeare's name from Henslowe's Diary. Ogburn finds this absence highly significant, and he follows many Baconians and Oxfordians before him in claiming that "the absence of [Shakespeare's] name from the most comprehensive rolls of the players in his day is strong indication that his alleged career on the stage is illusory" (101). However, the way he manipulates the facts to reach this conclusion is instructive. Ogburn correctly notes that Henslowe put on some of Shakespeare's plays, and he finds it odd "that while producing Shakespeare's plays Henslowe never once mentioned his name" (100). He does not tell the reader that these plays were all performed in 1592-94, before Henslowe began mentioning the names of any playwrights or actors at all in the Diary; by the time Henslowe did start writing down names in 1597, Shakespeare was a member of the rival Chamberlain's Men and had no association with Henslowe. Ogburn states that "the names of all other prominent playwrights of the time... find a place in [Henslowe's] diary", which is simply a blatant falsehood; the names of Robert Greene and Christopher Marlowe are absent despite the fact that Henslowe performed their plays many times, and Thomas Kyd, George Peele, Thomas Nashe, and Thomas Lodge are similarly missing. Ogburn then snidely remarks that "if Professors Evans and Levin and Dr. McManaway could have cited another case of an actor of Shakspere's alleged prominence not mentioned by Henslowe or Alleyn it is a fair assumption that they would have done so." As Irving Matus has already pointed out (Matus, 52-3), Richard Burbage, Augustine Phillips, John Heminges, and Henry Condell are among the well-known actors not mentioned by Henslowe; this is because the Diary is a record of Henslowe's company, and by the time he began mentioning any actors by name, all these men (along with Shakespeare) were members of the rival Chamberlain's Men.

The above is just a sampling of the way Ogburn manipulates or ignores facts in order to support elements of anti-Stratfordian doctrine which he is unwilling to criticize; the fact that such statements are so prevalent in the book, and the fact that Ogburn makes them so forcefully and arrogantly, do not speak well for either his standards of scholarship or his objectivity. This lack of attention to facts which disagree with his thesis is only one aspect of Ogburn's bias, though; another major double standard involves standards of evidence for William Shakespeare vs. other contemporary playwrights. Ogburn clearly believes that Shakespeare[2] was an ignoramus, probably illiterate and almost certainly not an actor, despised by all who knew him, spending most of his energies amassing property and collecting on debts, and not seriously believed by anyone at the time to be the author of the Shakespeare canon. In order to support this dubious conclusion, Ogburn contrasts Shakespeare with contemporary playwrights who were presumably not ignoramuses. A major problem is that he unquestionably accepts the same type of evidence for other

playwrights that he rejects for Shakespeare.

For example, Ogburn correctly notes that there is no contemporary documentary record of Shakespeare's schooling. The earliest explicit evidence we have is Nicholas Rowe's statement in 1709 that Shakespeare had been "bred at a free school", and Ogburn interprets this documentary lack to mean that Shakespeare never went to school at all, brushing aside all the circumstantial evidence of the school's quality and the likelihood of Shakespeare's attendance.[3] He then goes on to list the educational accomplishments of a number of contemporary writers, inviting his reader to "[c]ontrast the known facts about these writers' education with the absolute blank regarding Shakspere's, whose life's record is supposed to be so much better known to us than theirs" (280). There are a couple of problems here. First, Ogburn neglects to mention the many other writers besides Shakespeare who also lack documentary evidence of schooling; this list includes such luminaries as Michael Drayton, John Webster, William Warner, Thomas Dekker, Henry Chettle, and many less-known names. A more serious problem is the fact that for several of the writers whose education Ogburn confidently discusses, the actual documentary evidence is nonexistent or questionable. For example, he says that John Fletcher attended Cambridge, but there are good reasons for believing that the "John Fletcher" in the Cambridge records was not the dramatist (Taunton 1990). He similarly says that George Chapman attended Oxford, but Chapman's name is nowhere to be found in the records there, and our only source for this story is Anthony a Wood, writing a century after the fact (Spivack 1967, 14). Ogburn asserts that Ben Jonson "went first to a private school in St. Martin's Lane and later at Westminster studied under one of the foremost Elizabethan scholars, William Camden" (279-80). In fact, the story about the St. Martin's school was first told 26 years after Jonson's death by William Fuller, whose reliability is elsewhere (19-20) questioned by Ogburn, and Jonson's name is conspicuously absent from the records of Westminster School (Miles 1986, 284n10). The only evidence for Jonson's attendance at Westminster is his later friendship with Camden combined with a reference to "his Master Camden" in Jonson's conversations with Drummond. These conversations were not published until 1711, and the original manuscript has mysteriously disappeared from among Drummond's papers, surviving only in a transcript from around 1700 (Herford and Simpson I, 128-31). I think it is safe to say that if any of the above evidence pertained to Shakespeare, Ogburn would reject it out of hand as unreliable, given that he rejects far more solid evidence relating to the Stratford man; yet for these other writers, such evidence counts as "known facts".

Another example of this double standard can be seen in Ogburn's account of William Shakespeare's death in 1616. Ogburn writes that "apart from the entry in the burial register, Shakspere's death as far as the record shows went entirely unremarked", and he claims that this "was in an age when the

passing of noted poets called forth copious elegies from their fellows" (112). Such claims have become a mainstay of Oxfordian arguments, but unfortunately they do not stand up to scrutiny. First of all, the claim that Shakespeare's death went entirely unremarked will be puzzling to any Shakespeare scholar. The poems in the First Folio are the most famous tributes, but there is also William Basse's poem "On Mr. William Shakespeare, he died in April 1616". From its title this clearly refers to William Shakespeare of Stratford, and it was circulating in manuscript by 1623 (since Ben Jonson's Folio poem responds to it); it survives today in around a dozen manuscript copies (Chambers II, 226; Munro I, 286). Another manuscript elegy is written in a copy of the First Folio now at the Folger; it, too, clearly refers to Shakespeare of Stratford, since the same hand has also transcribed the verses from the tomb and monument in the Stratford church (Evans, 60). Neither of these poems can be dated precisely; the first tribute to Shakespeare which can is a poem in John Taylor's *The Praise of Hemp-seed* (1620) which lists Shakespeare along with Spenser, Sidney and other famous dead English poets who Taylor says will live on in their verses (Chambers II, 226). Three years later came the First Folio with its well-known poetic tributes, and over the next twenty years many more eulogies for Shakespeare were printed, including those in the Second Folio (1632) and the 1640 Poems.

Of the first three poems noted above, the only one Ogburn mentions is Basse's, which he dates to 1622 and calls "the first comment we have on Shakespeare's passing" (40). (He also gives the full title, complete with "he died in April 1616", without comment.) Thus when Ogburn says that the death of Shakespeare of Stratford went "entirely unremarked" in the record, what he apparently means is that there is no tribute which can be precisely dated to the few years after Shakespeare's death. But there is nothing suspicious about this at all: the same thing is true of all of Shakespeare's contemporary playwrights until the death of Ben Jonson 21 years later, and most of their deaths received far less notice overall than Shakespeare's. The vast majority of printed eulogies in Shakespeare's day were for members of the nobility, or sometimes for prominent churchmen; when poets did write posthumous tributes to each other, these generally circulated only in manuscript, sometimes for decades at a time.[4] The seven years before the first printed eulogies to Shakespeare appeared in the First Folio is actually remarkably fast, unprecedented for an English playwright, and the number of tributes written to the Bard is more than for virtually any of his contemporaries.

But, the reader may ask, what about the other writers Ogburn mentions who were showered with "copious eulogies from their fellows"? It is true that Francis Bacon and Ben Jonson were both honored with volumes of tributes soon after their deaths, as Ogburn notes (112), but they are not comparable: Bacon was a Viscount and thus the type of person who normally received printed elegies, and Jonson died 21 years after Shakespeare, right around the

time it was starting to become acceptable to honor poets in this way.[5] As for the other writers Ogburn mentions, a close look at his sources reveals that he is once again applying a tremendous double standard: he unquestioningly accepts evidence for these other writers which is in some cases much flimsier than the evidence he rejects for Shakespeare.

* Ogburn quotes an account of Spenser's funeral in which Spenser's fellow poets "vied with each other in Elegiac tributes to his memory" (53) and threw their verses into his grave along with the pens that wrote them. In fact, no contemporary account of Spenser's funeral exists; the story Ogburn quotes comes from the third volume of William Camden's *Annales*, which was not published until 1627, 28 years after Spenser's death (Wells, 139). Given Ogburn's suspicion of eulogies published only seven years after Shakespeare's death, one might reasonably expect him to question an account of Spenser's funeral published 28 years after the fact; yet he accepts this account without hesitation.

* Ogburn asserts that "Francis Beaumont had been mourned with a similar shower [of praise] on his death in the month before Shakespeare's" (112). He cites no source for this statement, undoubtedly because there is no documentary evidence for such a shower. Other than the record of his burial, the earliest notices we have of Beaumont's death are Taylor's poem in *The Praise of Hemp-seed* and Basse's MS eulogy to Shakespeare — the same two poems which contain the earliest mention of Shakespeare's death. The first printed eulogy specifically for Beaumont was "An epitaph upon my dearest brother Francis Beaumont", in the posthumous edition of his brother Sir John Beaumont's poems; this did not appear until 1629, thirteen years after his death and six years after the Shakespeare First Folio.

* Ogburn writes that "Michael Drayton, upon his passing in 1631, was honored by a 'funeral procession to Westminster escorted by gentlemen of the Inns of Court and others of note'" (112). However, there is no contemporary record of Drayton's death or funeral; the exact date of his passing is not even known. The story Ogburn quotes comes from a manuscript note by the antiquary William Fulman, who was born in 1632 — the year after Drayton's death — so it obviously was not based on firsthand knowledge (Newdigate, 219). Ogburn notes that there is a monument to Drayton in Westminster Abbey — which is of course parallelled by Shakespeare's monument in Stratford — and he asserts that "verses attributed to Ben Jonson and others were contributed". Actually, the anonymous verses on Drayton's monument were first attributed to Jonson in 1687, 56 years after Drayton's death; around the same time, John Aubrey (whose reliability Ogburn elsewhere ridicules) attributed the same

verses to Francis Quarles, and yet another manuscript note attributes them to Thomas Randolph (Newdigate, 221).

* Ogburn quotes Charlotte Stopes as saying that "the city and the Stage were clothed in gloom" when Richard Burbage died (112), and he quotes one of five eulogies which Stopes prints.[6] In fact, the only mention of Burbage's death in the contemporary record is passing mention in two letters, one by John Chamberlain and one by the Earl of Pembroke (Stopes, 116-17). All the eulogies which Stopes reproduces are in undated manuscripts (just like Basse's elegy to Shakespeare), and in fact no eulogy for Burbage was printed during the fifty years after his death.[7] Ogburn goes on to claim that "Camden... observed Richard Burbage's passing ('On Master Burbidge the Tragedian: Exit Burbidge') and recorded its date (9 March 1618/19) but had nothing to say of Shakspere's three years earlier". He cites Camden's *Remaines* as his source, but in fact Camden never mentioned Burbage, in his *Remaines* or elsewhere, before his death in 1625. In 1636, John Philipot edited the fifth edition of the Camden's *Remaines* and added material of his own. It was this edition which first contained the epitaph Ogburn quotes, but without the date of death; this was apparently added by the editors of the 1870 edition of the *Remaines* which Ogburn used.

The above examples illustrate the double standard Oxfordians apply in order to make William Shakespeare of Stratford look bad in comparison to other contemporary playwrights. The flip side of this is the double standard they apply in order to make Oxford look as good as possible: the slightest evidence favorable to Oxford is seized upon and expanded into elaborate scenarios, even as far more solid evidence for Shakespeare is dismissed with little more than a wave of the hand. One good example is Ogburn's treatment of the acting careers of Shakespeare and Oxford; he recognizes that the "William Shakespeare" who wrote the plays was recognized as an actor, so he tries to deny that the Stratford man was an actor while constructing an acting career for Oxford out of whole cloth. Ogburn pulls out all the stops in an effort to discredit the considerable evidence for Shakespeare's stage career. His deceptive and selective discussion of Henslowe's Diary, discussed above, is one part of this effort, and Irvin Matus has effectively countered Ogburn's efforts to cast doubt on the documentary evidence of Shakespeare as an actor (Matus, 52-64).[8] When Ogburn is unable to find any excuse for doubting the authenticity of a record, he simply declares that the record is doubtful without providing any evidence. For example, the name "William Shakespeare" appears second in the list of players in the patent for the King's Men and first in the list of King's Men who received red cloth in 1604 for James' coronation procession, surely indicating his prominence in the company. Ogburn, how-

ever, simply announces that "the identity of the Shakespeare in the two cases remains problematical" (30); presumably we are supposed to imagine that the reference is to Oxford under a pseudonym, despite the massive problems with such a scenario. The bequest to "my Fellowe William Shakespeare" in the will of King's Man Augustine Phillips cannot be to Oxford, since it occurs after his death, so Ogburn declares that "Shakspere [of Stratford] could have been meant, or another" (31). We are not told who else this "William Shakespeare" could be, if it was not the man who left bequests in his will to Richard Burbage, John Heminges, and Henry Condell. Ogburn in these instances is acting like a defense attorney rather than a scholar, using any means possible (including baseless innuendo) to cast doubt on facts he doesn't like.

In sharp contrast to his hyper-skepticism of the perfectly ordinary records of Shakespeare's acting career, Ogburn is eager to construct a stage career for Oxford, even in the absence of any documentary evidence. The closest thing to a record of Oxford acting is a letter by Gilbert Talbot, describing "a device presented by the Earl of Oxford, the Earl of Surrey, and the Lords Thomas and Windsor" at Court during Shrovetide 1579. Ogburn mentions this letter in passing (617), but his chief "evidence" for Oxford's alleged stage career is John Davies of Hereford's epigram "To Our English Terence, Mr. Will: Shake-speare", published in 1610 in Davies' *The Scourge of Folly*. The entire poem is worth quoting here.

> Some say (good Will) which I, in sport, do sing,
> Had'st thou not played some Kingly parts in sport,
> Thou hadst been a companion for a King;
> And been a King among the meaner sort.
> Some others rail; but, rail as they think fit,
> Thou hast no railing, but, a reigning Wit:
> And honesty thou sow'st, which they do reap;
> So, to increase their stock which they do keep.

After quoting the poem, Ogburn says that "I cannot see any interpretation but one to put on it: 'Shake-speare' was a nobleman who lost caste by appearing on the stage, though he took kingly parts and played them only in sport" (104). A little later he says that Davies appears to be telling us "that Shake-speare was indeed a man of high birth, probably an earl, who lowered himself by taking parts on the stage, albeit under a pseudonym" (105); the clear implication (which Ogburn never makes explicitly) is that Davies was addressing Oxford in this poem.

Despite Ogburn's characteristic certainty, a reading of the entire volume in which the poem appears makes it abundantly clear that Ogburn's reading is doubtful in the extreme, and that Davies was *not* addressing Oxford here. First of all, there is the tone: casual, playful, jocular, in keeping with

Davies' other poems to stage figures and poets. In contrast, Davies' poems to member of the nobility (of which there are many in the volume) are serious and respectful. For example, his poem to King James begins, "For bounty, clemency, and chastity, / (Three virtues which in Caesars seldom meet) / No king that ever swayed this monarchy / To rules of grace and peace, hath made so meet" (Davies, 51), and his poems to various Earls contain similar flattery. It is difficult to imagine Davies addressing an Earl with the poem reproduced above without committing a serious breach of etiquette. A more important objection, though, is the fact that the poem is addressed directly to Shakespeare and written in the present tense. All the other present-tense poems in the volume are written to people who were alive in late 1610; whenever a poem is addressed to a dead person, Davies clearly indicates this fact (e.g. "In praise of Sr Henry and Sr Phillip Sidney, Syre and Sonne deceased" (Davies, 16) and "An Epitaph upon the death of the most noble Sr Thomas Gorge, decesing in March, Anno Salutis, 1610" (Davies, 23)). Since Oxford had died in 1604, Davies was clearly not addressing him as "Shakespeare".

But what about the references to "kings" which Ogburn finds so significant in the poem? These are simply examples of Davies playing on the name of the King's Men, the acting troupe to which Shakespeare belonged. There are two other poems in the volume addressed to members of the King's Men: "To the Roscius of these times Mr. W[illiam] Ostler" (Davies, 31) and "To honest-gamesome Robin Armin, That tickles the spleene like an harmless vermin" (Davies, 60). Both of these poems play on the word "king" just as the Shakespeare poem does: Davies asks Ostler, "where was thine action when thy crowne was riven, / Sole king of actors", and his poem to Armin mentions "kings" three times. The only other poem in the volume which mentions "kings" is the one addressed to King James himself, though the poem to John Fletcher (who wrote plays for the King's Men) plays twice on the word "reign".[9] It appears that Davies was merely fond of wordplay, and that Ogburn's elaborate exegesis of the poem to Shakespeare — along with his "evidence" for Oxford's stage career — collapses when looked at in context.

Ogburn's absolute certainty that his interpretation of the Davies poem is correct, even though he has apparently not even looked at the context of that poem, involves yet another double standard he applies in his book. This standard can be illustrated by looking at Ogburn's discussion of two prefaces well-known in Shakespeare studies: Henry Chettle's 1592 preface to his *Kind-Harts Dreame*, and the anonymous preface to the 1609 Quarto of Shakespeare's *Troilus and Cressida*. First, consider Chettle's preface, which was part of the well-known controversy surrounding *Greenes Groatsworth of Wit* in late 1592. *Groatsworth* was one of a spate of books which came out upon the death of Robert Greene in September of that year, purporting to have been written on his deathbed[10]; it contains the well-known epistle to three playwrights, most likely Marlowe, Nashe, and Peele, which in turn contains the attack on "the only

Shake-scene in a country" which has generally been taken to be an attack on Shakespeare. Three months later Chettle published *Kind-Harts Dreame*, containing a preface in which he wrote the following:

> About three moneths since died M. Robert Greene, leaving many papers in sundry Booke sellers hands, among other his Groats-worth of wit, in which a letter written to divers play-makers, is offensively by one or two of them taken, and because on the dead they cannot be avenged, they willfully forge in their conceites a living Author: and after tossing it two and fro, no remedy, but it must light on me. (Chettle, 5-6)

Chettle then went on to describe the reactions of the two offended playwrights: one of them ("whose learning I reverence") has generally been taken to be Marlowe, and the other, to whom Chettle apologizes handsomely ("I am as sory, as if the originall fault had beene my fault") is generally taken to be Shakespeare.

Ogburn, however, will have none of this. He insists that the second offended playwright cannot be Shakespeare, because "Chettle wrote that the playwright who had taken offense and whom he was sorry not to have spared *was one of the three playwrights addressed by Greene*" Actually, Chettle did not write this: he wrote that "a letter written to divers play-makers, is offensively by one or two of them taken", which is not nearly so unambiguous as Ogburn would have us believe. As other writers (e.g. Chambers, I, 59; Marriott, A7) have pointed out, Chettle's language here is sufficiently vague ("divers", "one or two") as to make us believe that he was not writing with Greene's exact words in front of him, but rather was recalling the episode in general terms. Given that the "famous gracer of Tragedians" (Marlowe) and the "upstart Crow" are the only two people likely to have taken offense at what was written in the epistle, and given Chettle's reference in his apology to "the qualitie he professes" (a reference to acting), it is entirely reasonable to interpret the apology as referring to Marlowe and Shakespeare. Yet Ogburn sneers at E. K. Chambers for considering the context and allowing for the looseness of Chettle's language: "Chettle was confused, it seems, and Chambers straightens him out. Chambers, three and a half centuries later, knows better than Chettle what Chettle meant" (62). A strictly literal interpretation of Chettle's words is all that is possible, we are told.

Ogburn sings a different tune, however, when he discusses the anonymous prefatory epistle to the second issue of the 1609 Quarto of *Troilus and Cressida*. Ogburn finds much that is mysterious in the epistle, which consists mostly of praise for the author and this "new play, never stal'd with the Stage, never clapper-clawd with the palmes of the vulger" (Chambers, II, 216). The epistle refers to the "scape" this play has made from the "grand possess-

ors"; Ogburn baldly asserts that these possessors must have been "members of the nobility" (205) rather than an acting company, despite the abundant evidence (some of it discussed by Matus, 73) that acting companies were reluctant to have their plays printed. Ogburn also asserts that the epistle appeared in the first edition of *Troilus*, but was omitted from a second edition because it was too "daring"; in fact, it has long been established that the edition with the epistle was the second (Williams, 25-33). More interesting for our purposes, though, is the following sentence from the epistle, referring to the author of the play: "And beleeve this, that when hee is gone, and his Commedies out of sale, you will scramble for them, and set up a new English Inquisition" (Chambers, II, 217). This sentence appears to be telling us very clearly that the author is still alive: *when* he is gone, you *will* scramble for [his comedies]. Since Oxford had died in 1604, Ogburn cannot allow such a straightforward interpretation, so he has to scramble. He writes: "But this situation has already come to pass" (206), ignoring the fact that editions of five of Shakespeare's plays were printed in 1608-9, along with the Sonnets, an edition of *Venus and Adonis*, and another play (*A Yorkshire Tragedy*) falsely attributed to him. Ogburn further writes: "one does not in any case in referring to a living writer coolly speak of how things will be when he is dead, even if one could foresee how they would be." It is difficult to decipher exactly what Ogburn is trying to say here; he seems to be simply asserting that the sentence in question does not mean what it seems to mean. Apparently, the writer of the epistle was confused, and Ogburn straightens him out. Ogburn, nearly four centuries later, knows better than the writer of the epistle what the writer meant.

I have tried in this article to explain the major ways in which Oxfordian methods differ from those used by literary scholars, using Ogburn's book as a case study. Oxfordians typically ignore or rationalize away the external evidence; they apply a sometime radical double standard in order to make Shakespeare look bad in comparison to other playwrights, and to make Oxford look good; they confidently interpret texts without looking at the context those texts appeared in; they are distressingly reluctant to criticize previous Oxfordian writers, even when those writers are clearly wrong. Not all Oxfordians are equally guilty of these things; there are some who, to their credit, have tried to raise the standards of the movement and put it on a more scholarly footing. Even if the worst of the bad scholarship is trimmed away, though, the heart of the Oxfordian case rests on double standards and enshrinement of subjective interpretations as fact. Ogburn's book is essentially an elaborately presented rationalization for his fiercely-held ideas about who should have written Shakespeare's works, dressed up in the trappings of scholarship but employing a series of double standards which make it impossible to disprove his basic thesis. This is a harsh assessment, but one which I believe would be shared by any Shakespeare scholar who took the time to work through Ogburn's book. I realize that Oxfordians will disagree with much of

what I have written, but I hope that it nevertheless causes them to take a second look at some of their assumptions and methods. The one thing which unites Oxfordians and orthodox Shakespeareans is a love for Shakespeare's works, and even if we disagree about some very basic issues, we can agree that it does matter who wrote those works.

Notes

1] Marlowe (p. 44) and Nashe (pp. 63, 67) are mentioned in "entries" in the diary which are nineteenth-century forgeries by John Payne Collier.

2] I will use the spelling "Shakespeare" for the man from Stratford, because that was by far the most common spelling used to refer to him during his lifetime. I have discussed the spelling of Shakespeare's name at great length in my essay "The Spelling and Pronunciation of Shakespeare's Name", available on the Shakespeare Authorship web site at http://www.bcpl.lib.md.us/~tross/ws/name1.html.

3] Several of Shakespeare's Stratford contemporaries unquestionably knew Latin and apparently received good educations, despite coming from very similar backgrounds and never attending a university — most notable among these being Richard Field and Richard Quiney (cf. Eccles, 54-62, and Fripp, 30-32). Ogburn snidely dismisses T.W. Baldwin's monumental study *William Shakspere's Small Latine and Lesse Greeke* as a "postulation" of an "ideal grammar school" (278), ignoring the mass of documentary evidence Baldwin compiled about the curricula of Elizabethan schools.

4] The one possible example prior to Shakespeare of a printed elegy within seven years of a playwright's death is *Greenes Funeralls* by "R. B.", a 17-page pamphlet of poems printed in 1594, two years after Robert Greene's death. But the poems make no mention of Greene's plays, concentrating entirely on his prose works, and the printer's introduction states that it was published "contrarie to the Authors expectation" (McKerrow, 69), consistent with the practice that elegies for poets circulated in manuscript and were not printed.

5] The volume in honor of Jonson, *Jonsonus Virbius* (1638), was an unprecedented honor for an English poet, and was a turning point after which printed posthumous tributes to "mere" poets became acceptable. Nevertheless, the volume almost never came about; Doctor Brian Duppa had been gathering manuscript elegies for Jonson, but Sir Kenelm Digby had to write Duppa to urge that the collection be printed, or else it would have remained in manuscipt (Bradley and Adams, 201).

6] Ogburn mistakenly attributes the eulogy he quotes to Thomas Middleton, when it is actually anonymous. The quoted poem immediately follows Middleton's four-line poem in Stopes' book (Stopes, 117), and Ogburn has apparently mistaken it for a continuation of Middleton's.

7] In 1671, the third edition of Richard Flecknoe's *Epigrams* contained a poem entitled "To Charles Hart. The praises of Burbadge, or of an Excellent Actor" (Flecknoe, 56). As the title implies, this poem honors Flecknoe's contemporary actor Hart by comparing him to the great Burbage.

8] One series of relevant documents which are not discussed by either Ogburn or Matus are a series of legal documents which list "Richard Burbage and William Shakespeare, gentlemen" as the primary tenants of a "playhowse" which is obviously the Globe (Kathman, 73-78). The designation "gentlemen" shows that the reference is clearly to Shakespeare of Stratford and not to a nobleman in disguise, and in any case pseudonyms had to be clearly spelled out in legal documents such as these.

9] As for the specific reference to playing "kingly parts in sport", this could of course refer to Shakespeare playing the parts of kings in his own plays; in fact, Donald Foster's reconstruction of Shakespeare's acting roles using the SHAXICON database (Foster, 25-32) shows that he most likely played the King of France in *All's Well*, King Henry in the two parts of *Henry IV*, King Ferdinand in *Love's Labour's Lost*, Duncan in *Macbeth*, and Theseus in *A Midsummer Night's Dream*.

10] Ogburn spends several pages describing Warren Austin's computer study which concluded that *Groatsworth* was most likely written by Chettle and not Greene, claiming that the study "invalidates an essential premise of Stratfordian biography" (62). Even if one accepts Austin's conclusion, it is difficult to see what difference it makes whether the pamphet was written by Greene, or by Chettle posing as Greene; the substance of the attack on "Shake-scene" is the same regardless.

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Book Reviews

The Man Who Lived Twice

Sir Philip Sidney and the Circulation of Manuscripts 1558-1640
by H.R. Woudhuysen
(Oxford: Clarendon Press, 1996)

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

We try to organize and understand the past by calling it names. In the nineteenth century, Lewis Henry Morgan, the father of anthropology, came to the conclusion that the past can be divided into three phases—savagery, barbarism, and civilization. Each phase is characterized by the way humanity sustains and perpetuates itself, how it makes livings and lives, organizes work and sex, production and reproduction.

The twentieth century has called the past names based on how humanity "communicates"—a word that has been so widely used and abused that it has the ring of tin. Especially since Marshall McLuhan published his *The Gutenberg Galaxy* (1962), students of history and literature have tended to divide the past into new phases—oral culture, manuscript culture, and print culture, with electronic culture now in the ascendant.

Oral cultures rely on memory and mnemonic devices because there are no written records. Manuscript cultures rely on the limited distribution of handwritten documents, with all their potential for misreadings and errors. Print cultures achieve wide, inexpensive distribution of uniform texts through the invention of movable type and the technological improvements to printing that followed in its wake. Electronic cultures communicate by radio, television, and so on—an instantaneous transmission of images and words. Tolstoy conveyed the soul of electronic culture when he imagined a future dominated by Ghengis Kahn with a telephone.

This new way of calling the past names alters our view of the past. What were once called the Dark Ages became the Middle Ages and are now known as the early medieval period. What was once called the Renaissance is now known as the early modern period. As we have all been repeatedly told, we live in the post-modern period and there are no doubt the hopeful among us who look forward to the day when our time will be christened the early post-modern period. I am in no rush. Progress begins to have the look of taking great strides backwards.

What all of this, of course, implies is that in our time there is more interest in what we call the past than in what our ancestors did and thought. H.R. Woudhuysen is one of the happy few who refuses to give in to this adolescent and egotistic tendency. He is that refreshingly odd bird, a nineteenth century scholar who finds himself operating in the post-modern period. The result is a massive but highly readable compilation of facts and rational speculations on how literature—writing—was preserved and transmitted during the English Renaissance. The sheer labor that went into the making of this book would have made figures of that period think of Hercules.

One sign of this immense labor is that the volume really contains two books in one. Woudhuysen's thesis has the virtue of simplicity. He sets out to show that manuscript or scribal culture continued to flourish longer and was of greater importance in the England of the Renaissance than scholars have realized—despite the presence of the press and its good work. The first half of his book is dedicated to establishing this thesis by showing the vocations—writing master, secretary, scrivener, seller of manuscripts, collector of manuscripts, and so on—that supported this culture and the networks of understanding, the "scribal communities," that made up this culture.

The second book in the volume is in effect a case study. If manuscript culture continued to flourish well into the age of print, who is a representative writer of that culture and what light does a knowledge of the existence of that culture shed on the representative figure? Woudhuysen's apt answer is Sir Philip Sidney and he proceeds to re-evaluate Sidney as a participant in a scribal or manuscript culture.

When Sidney died of wounds received in battle at Zutphen in the Low Countries and his corpse was accorded a procession of grave pomp and elaborate mourning long after his death, his reputation was fixed. He was a Protestant martyr, a Protestant knight, a courtier, a soldier, and a scholar cut down prematurely in the war against imperial and tyrannical Catholic Spain.The funeral seems to have been arranged by Sidney's father-in-law, Sir Francis Walsingham, the titular head of the Elizabethan secret service, perhaps at the bidding of his master, William Cecil, Lord Burghley, to coincide with the execution (or judicial murder, depending on your point of view) of that lingering focal point of Catholic opposition to the reign of Elizabeth, Mary, Queen of Scots.

Sidney was a public figure, a nephew of the Earl of Leicester and a politician who had been entrusted with diplomatic missions and military commands. If he was known as a writer outside of small circles of family and friends—"scribal communities"—it was as the author of a letter to the Queen opposing the proposed French alliance through marriage, a political position that had been reflected in Sidney's famous tennis court quarrel with the Earl of Oxford, when Oxford memorably called the Protestant knight a "pup."

Woudhuysen accurately provides the context for this sole source of Sidney's public fame as a writer at the time of his death, a fame that supported rather than damaged the image of him established by his politically inspired public funeral:

> Sidney's *A letter to Queen Elizabeth* was part of a carefully orchestrated campaign to dissuade the Queen from marriage and to whip up opposition to her suitor the duc d'Alencon. There seems little doubt that Leicester was the figurehead behind the propaganda offensive and that Sidney's letter was intended to circulate initially among courtiers and nobles, while its companion piece Stubbs's *The discoverie* was designed to attract a mass readership. To be effective in the campaign against the Queen's marriage, Sidney's letter had to circulate in fairly large numbers of copies.

But Woudhuysen misses an opportunity here to make clear one of the benefits of staying within the limited bounds of a "scribal community" in merry old England.

Stubbs,who used print "to attract a mass readership," had his writing hand cut off as a reward for his published outspokeness and spent the rest of his days in the household of that noble Protestant, Peregrine Bertie, Lord Willoughby, the earliest of Elizabeth's military commanders in the Low Countries. Sidney continued a trusted favorite at court, a promising young man on the make who was rewarded with promotions and posts of honor.

But it was not merely the relative safety, the limited circulation and potential anonymity, of script that made Sidney cling to it, as Woudhuysen convincingly argues:

> Sidney's preference for manuscript publication arose from a variety of factors. The first may well have been the result of a fear of the so-called 'stigma of print,' that it was not fitting for the man of his rank to let his works be sold in shops to anyone who could afford to buy them. He was after all writing for personal pleasure rather than in the hope of gaining patronage or of selling his works for profit: hewas a courtier, not a hack. A doubt about the final value of what he was doing may have been linked to this. Sidney probably had few anxieties about the literary worth of his writings, but he may have felt he was destined personally and politically for higher and greater things.'My youth doth waste,' he makes Astrophil say, 'my knowledge brings forth toyes.' Was this really the best he could do with his life, which had promised so much?

Precisely. And it was this concern that determined Sidney's posthumous reputation as a writer—perhaps the most interesting part of the long, detailed story Woudhuysen has to tell.

In November, 1586, soon after Sidney's death, Fulke Greville, who worshipped the memory of Sidney and eventually wrote a life of him, wrote a letter to Sir Francis Walsingham that Woudhuysen reproduces and modernizes. The letter alerts Walsingham that Greville has been told by "one Ponsonby, a book binder in Paul's Churchyard," that there were plans afoot to "print Sir Philip Sidney's old Arcadia." Ponsonby wondered if the enterprise had the blessing of Walsingham and Sidney's friends. Greville urges Walsingham to not only prevent the printing of the "old Arcadia" but also to "make a stay" against a translation of Monsieur du Plessis's book against atheism, "that mercenary book" by Arthur Golding, the translator of Ovid's *Metamorphoses* and the uncle of the Earl of Oxford. Greville proposes that a version of *Arcadia* in his possession should instead be carefully printed along with Sidney's translation of the book against atheism and other works—"Bartas his *Semaine*, forty of the Psalms translated into meter." Greville's motive in writing Walsingham is clear: he wants to use print to fix Sidney's image as a writer, that is, he desires "that Sir Philip might have all those religious honours which are worthily due to his life and death."

Greville temporarily got his wish. He, with the help of two others, prepared the manuscript of the new *Arcadia* and saw it through the press. The script of the "old Arcadia" did not resurface for about 300 years. Sidney's name was added to the title page of Golding's version of Plessis' book.

But things did not rest there. Gradually, a very different view of Sidney as a writer—and especially as a poet—reached the mass audience through print. His sonnet sequence on an affair with Lady Penelope Rich—*Astrophil and Stella*—was issued in a quarto in 1591, the year after Walsingham's death, with an introduction by Thomas Nashe. And in 1598, a folio that purported to contain Sidney's *Complete Works* was published by Sidney's sister, the Countess of Pembroke, a patron and brilliant literary light of the time, with the help of others. This Folio criticized and corrected the *Arcadia* Greville had issued, added Sidney's *Certain Songs and Sonnetts*, expanded the *Astrophil and Stella* sequence, and included other previously unpublished work. It did not, however, print Sidney's "A letter to Queen Elizabeth," virtually the only writing for which Sidney could have been widely known at the time of his death.

Sidney, in 1598, emerged not as a courtier and soldier, a Protestant martyr, but as a Petrarchan poet of real ability and of lasting interest—his sister's version of him, not Walsingham's or Greville's. And it is no doubt as a poet—not as a politician and soldier, the Protestant knight, that Sidney will continue to be remembered. Sidney's own practice, his limiting adherence to "scribal communities," left him virtually unknown and potentially unknowable as a poet at the time of his death. It was his loyal sister's literary interests

and love of his poetry—a love that meant more to her than the "stigma of print"—that allowed her to crack if not topple the state-sponsored image of Sidney that had been erected at the time of his funeral so that he now resides among the English poets:

> Leave me, O love which reachest but to dust,
> And thou my mind, aspire to higher things.
> Grow rich in that which never taketh rust:
> Whatever fades but fading pleasure brings.
> Draw in thy beams, and humble all thy might
> To that sweet yoke where lasting freedoms be;
> Which breaks the clouds and opens forth the light
> That doth both shine and give us sight to see.
> O take fast hold; let that light be thy guide
> In this small course which birth draws out to death,
> And think how evil becometh him to slide
> Who seeketh heaven and comes of heavenly breath.
> Then farewell, world! thy uttermost I see:
> Eternal Love, maintain thy life in me.

The Thirty-Eighth Play

Shakespeare's Edward the Third: An Early Play Restored to the Canon
Ed. Eric Sams (Yale University Press, 1996)

Reviewed by Daniel L. Wright, Ph.D. Professor Wright is Chair of the English Department at Concordia University in Portland, Oregon, and is the Director of the Edward de Vere Studies Conference.

Scholars have vigorously debated the question of *Edward the Third's* authorship at least since Edward Capell proposed the likelihood of Shakespearean authorship of the work in 1760. Recently, however, a consensus among scholars regarding the authorship seems to have emerged which suggests that, while *Edward the Third* probably is not entirely a product of Shakespeare's hand, it at least is substantially enough to be his to be considered canonical and worthy of inclusion among a body of thirty-seven (now thirty-eight) plays (inclusive of such enigmatic works as *Pericles, Prince of Tyre* and *The Two Noble Kinsmen*—romances which have achieved Shakespearean attribution that, nonetheless, continue to be disputed as authentically or even pre-eminently Shakespearean by many readers of the Bard).

Eric Sams, editor of Yale University Press's new edition of *Edward the Third,* contends that Establishment Academia's recent, grudging concession of the bulk of *Edward the Third* to Shakespeare's hand is still reflective of a too timid, too conservative (and finally erroneous) judgement—one typical, though, he argues, of the "elitist attitudes of 1920s Oxbridge that still dominate orthodox scholarship world-wide." It is Sams's conviction, contrary to orthodox opinion, that *Edward the Third* is less likely the consequence of a collaborative effort of playwrights than a work fully Shakespearean—its "deficiencies" attributable only to the probability that it is an early Shakespeare play. Specifically, as an immature composition, Sams argues, *Edward the Third* naturally lacks some qualities that typify the more mature, familiar and indisputably recognizable plays of the Bard. Sams submits that its occasional distinctiveness and marked differentiation from other works in the canon, therefore, are evidence not of deformity by collaboration (or worse—plagiarism—as some have contended); they rather more likely are simply stylistic anomalies reflective of Shakespeare's yet-unripened talent in the rendering of historical drama (an observation much in character with our common-sense recognition of the incontestable inferiority of Shakespeare's Yorkist Tetrology when those works are contrasted with the more seasoned achievements of the Lancaster plays).

As one who has come increasingly to mistrust the uncritically preservationist and self-interested orthodoxy which cripples more than it enables in contemporary Shakespearean scholarship, I find Sams to be a refreshing voice in Academia and regard his edition of *Edward the Third* as a contribution to Shakespearean studies that should be enthusiastically welcomed. Sams's critical posture with respect to this text is representative of the best work among those inquisitive modern scholars who aren't gloomily resigned to sing continually the fading hymns of a dying chorus which, more often than not, seems passionately intent only on mustering energies to drown out any new voice that challenges the tired uniformity of its repetitious and stale melodies. His study of this play, unprecedented in the breadth of the information it provides us in a handsomely-compressed and well-ordered format, is an estimable addition, complement and successor to some of the newer studies that lately have been published in this area of critical scholarship.

Among the more significant investigations of *Edward the Third* to have appeared in recent months, prior to the release of Eric Sams's study, is Jonathan Hope's *The Authorship of Shakespeare's Plays: A Socio-Linguistic Study* (Cambridge Univ. Press, 1994). Hope contends that, based on his socio-linguistic analysis of this oft-neglected play, no determination to exclude *Edward the Third* from the Shakespearean canon can reasonably be justified any longer. He proposes, moreover, that despite the merits of other contenders for canonical status, *Edward the Third,* among all of the apocryphal works of Shakespeare, is "the best suited candidate... for inclusion in the canon" (154).

Richard Proudfoot's examination of *Edward the Third* (*"The Reign of*

King Edward the Third [1596] and Shakespeare"), a particularly thoughtful article published in the recently-released edition of *British Academy Shakespeare Lectures, 1989-90* (Oxford Univ. Press, 1993), is another study that echoes many of the arguments of both Sams and Hope in its robust contention for the canonicity of the play. As Proudfoot declares, "Investigation of the play's language, particularly its exceptionally large vocabulary, and... those associative links described as 'image clusters,' is far on the way to demonstrating a kind and degree of connection between the early works of Shakespeare that amounts to a strong positive case for his authorship..." (162).

Familiarity with such commentary and the scholarship on which it is based is a vital constituent of any serious research that wishes to assist in determining the authorship of *Edward the Third.* One of the especially praiseworthy strengths, therefore, of Sams's edition of this text is his provision of a comprehensive and up-to-date bibliography of published commentary on *Edward the Third* (including the aforementioned pieces) that directs the reader to such essential studies and recent criticism as fully valuable as Hope's and Proudfoot's (although Sams also includes a fine survey of the scholarship and popular commentary on the authorship of *Edward the Third* in a summary account of critical works prior to 1760).

In addition to these valuable critical components, Sams supplies the reader with a thoughtful synopsis of the play, extensive notation of the text of the play, a summary chapter of the case for Shakespearean authorship, and appendices that address important considerations in the debate about the authorship of two other significant Renaissance manuscripts, *Edmund Ironside* and *Sir Thomas More*—each of which has been promoted for elevation to canonical status by those who regard these works as fully Shakespearean or at least marginally indebted to Shakespeare for some of their inspiration and versification.

In his study, Sams does not give any indication that he is anything other than Stratfordian in his authorial assumptions (he is silent on the matter of who the author of *Edward the Third* may be, apart from his insistence that it most likely is "Shakespeare," although inferences about "Shakespeare" that can be derived from his work suggest putative Stratfordian convictions). Regardless, his research should be embraced by Oxfordians because it significantly advances Shakespearean studies, and inasmuch as it does so, it contributes—however much it presently may seem only indirectly so—to the cause of conclusively demonstrating the Earl of Oxford's authorship of the Shakespeare plays and poems.

In particular, *Edward the Third*'s dramatic illustration of those conflicts which are begot when private desires become entangled with the inexorable demands of public duty, depicts with astonishing intensity many of the more poignant anxieties that we know preoccupied and almost obsessively troubled the Earl of Oxford. To see in the play the artistic sublimation of so much that defines Oxford's well-documented inward strife in these contentious

matters may suggest at least one route of endeavor for productive literary inquiry. Although, of course, profitable as such inquiry may prove to be, absent hard evidence, we remain mindful that interpretive constructs alone shall decisively establish but little that will secure a currency of decisive value in efforts to procure recognition of Edward de Vere's authorship of this and other works in the canon.

Love's Labor's Won

Love's Labor's Lost: Critical Essays
Ed. Felicia Hardison Londré
(Garland Publishing, 1997)

Reviewed by Gary Goldstein.

This handsomely produced hardcover of 476 pages assembles a brilliant selection of critical essays, theater reviews, poems and letters spanning four centuries and three continents. By so "merging" the contributions of the scholar, the critic and the theater professional, Professor Londré has provided generalists and specialists alike with that most rare of pleasures: a fully rounded perspective on one of Shakespeare's most misunderstood plays.

Contributors include two contemporaries of Shakespeare, Robert Tofte and Sir Walter Scope, classic essays by Samuel Johnson, von Schlegel, and Coleridge, Hazlitt and Pater, plus modern contriibutions from scholars, reviewers, directors and actors from Japan, France, England and the U.S.

In her introduction, Londré discusses the Shakespeare authorship question, presents the Oxfordian case, her position (that Edward de Vere, 17th Earl of Oxford wrote the plays and poems under the pseudonym "Shakespeare"), and expresses the hope that, in the future, scholarly research will be conducted on both sides of the issue and within the Academy.

Shakespeare's Scribe

The Texts of Othello and Shakespearean Revision
by E.A.J. Honigman
(Routledge, 1996)

Professor Honigman offers us several explanations for several long-standing problems regarding *Othello* and the First Folio. He starts off by declaring that "Shakespeare (like other dramatists of the period)

wrote a first draft or 'foul papers' and also a fair copy, and that these two authorial versions were both copied by professional scribes, the scribal transcripts serving as printer's copy for Q[uarto] and F[folio]."

He then poses the question: was F *Othello* printed from a corrected copy of the Quarto or from a manuscript? His answer is the latter. As to who penned the manuscript of *Othello* for the First Folio, his answer, or discovery, is the scribe Ralph Crane. Without pausing for breath, the professor proceeds to ask, "Who edited the First Folio?" and then provides the following scenario.

> Howard-Hill thinks that detailed supervision of the texts included in the [First] Folio would have been impossible for busy men like Heminges, Condell and the book-keeper of the King's men.
>
>> On the other hand, there does exist indisputable evidence of an editorial presence in the Folio over some stretch of time, exerted by one who had a documented close connection with the King's Men—
>
> in short, Ralph Crane. By adding *Othello* to the five comedies assigned to Crane we stretch the 'stretch of time' to embrace just about the whole of the First Folio, and the implied question becomes even more interesting: what kind of editorial presence?

Honigman wonders whether Crane may have "corrected what less trusted scribes had written and who copied out single pages or scenes that were deemed too untidy or illegible for the printer?" He answers his own question:

> We can say, then, that Crane's role in the preparation of the First Folio appears to have been a significant one, more so than hitherto suspected. At the very least he transcribed five comedies—this is generally agreed—and it may be that he transcribed eight plays in all and even replaced pages in other texts that were illegible or otherwise unsuitable for the printer.

Honigman here proposes that Crane transcribed *Othello* and *2 Henry IV* for the First Folio, and urges more detailed study of Crane's scribal habits, in particular his transcripts of the play, *A Game at Chess.* Doing so "may yet identify other plays, or parts of plays, in which he had a hand."

Honigman also declares that "The arguments of this book drive me to a conclusion that I did not anticipate, namely that the reliability of F Othello has been overrated and that Q's has been underrated—", leading him to offer this advice to himself and, obviously, future editors of the plays: "...I may want to re-edit *Othello* with Q as parent text."

Those interested in weighing the detailed evidence assembled by Professor Honigman are advised to comb through his short, 181 page text. Honigman summarizes his methodology as follows:

> ...the evidence identifying Crane as the scribe responsible for five Folio comedies consists mainly of accidentals—'marks of elision, parentheses, hyphens and the like.' My list of 'Crane' spellings supports this identification of Crane as a Folio scribe and also depends on accidentals... Howard-Hill concentrated on Crane's usual or favored spellings, whereas most of my 'Crane' spellings, etc., are best described as occasional, rather than usual, in his work.

Oxford Redux

Alias Shakespeare
by Joseph Sobran
(The Free Press, 1997)

Reviewed by John Mucci. Mr. Mucci is associate editor of The Elizabethan Review.

In the commonality among the mass of material available on Shakespeare's authorship, there is a necessity to cover the same ground to introduce readers to the contention. After reading dozens of such books, one comes to regard them as a kind of familiar tapestry, some with one design brought forward, and others with items subdued or omitted. As the threads are drawn out one by one, the reader may with some pleasure appreciate the skill which the author has selected his patterns and arranged his loom. In his long promised book, *Alias Shakespeare*, Joseph Sobran has succeeded in creating a most attractive arras, through which we are invited to run our rapier and skewer the persistent man from Stratford whom traditionalist conflate with William Shakespeare.

On the author's own terms the book is persuasive: those who read this as their first introduction to the authorship question are likely to find it absorbing and thorough. As a mainstream book brought out by a major publisher, it begs to be taken seriously, and will doubtless be mightily pounced upon by academia for that presumption.

Although Sobran himself regards traditional Shakespearean biographies to be "comically formulaic," his case for Oxford follows many others in the attempt to first compromise the position of the Stratford Man (or, "Mr. Shakspere," as Sobran so disingenuously insists on putting it), then build up Oxford through parallels in his life with the Shakespeare works.

The new twist is that so many of the obstacles with which Oxfordians have grappled—one might almost say been bloodied over—Sobran ignores, or casts aside, leaving himself a very clear path of polished touchstones which he uses to smoothly present his case. His introduction is coy: "I have not tried to

dispute every point about the authorship question; some interesting problems have been left hanging because I believe they are, for the time being anyway, unanswerable." But this cavalier method will sound the alarum for many Oxfordians, as Sobran continues: "In many cases, I have not tried to refute orthodox criticisms of common anti-Stratfordian arguments for the simple reason... in some cases I think the orthodox are quite right." He then lists items which are dear to every dissenter's heart: the Ashbourne portrait, the hyphenation of the name Shake-Spear, the Stratford grammar school, supposed cryptograms, the shaky signatures, the monument and its changes... he has decided to address none of these. It is a dose of cold water thrown on those who are expecting a full-blooded attack on Stratford. "The key issues are sufficiently demanding and, happily, soluble," he concludes.

Once the path is straightened by this bold set of assumptions Sobran's thesis becomes clear. His new material focuses on the three long poems (*Venus and Adonis*, *The Rape of Lucrece*, and the 1609 cycle of *Shake-Speare's Sonnets*) and the relationship to Shakespeare's patron, Henry Wriothsley, Earl of Southampton. Hyphenated pseudonyms and Tudor Rose theory be damned, Sobran has weeded out a clear spot to hammer in his groundstake, and this is it. Edward de Vere, the sophisticated rake and man of letters, fell into a passiontate relationship with a young earl, and these three long poems express it openly. Rather than have it be an enormous embarrassment to the Oxford and Southampton family names, attention was focused on the plays with the publication of the First Folio, in an attempt to obscure these tattle-tale lyrics, which were not printed or even mentioned in the Folio. This despite the apparent popularity of the two narrative poems to have up to ten editions published by 1623. It is an intriguing theory, and one which does not strain credulity—*if* one is convinced of de Vere's authorship.

His method of distilling the salient facts and interpretation of facts to conclude Oxford's authorship is compelling, even though his sources seem to be less than complete. Although Ogburn, Looney and Fowler are mentioned, they are all but glossed over, and only Ogburn's *The Mysterious William Shakespeare* is to be found in the Works Cited appendix. For the opposition, although he acknowledges the input of Alan Nelson, Samuel Schoenbaum, and Irvin Matus, their works referred to (when cited at all), are their lesser works, books which do not really address the points at hand, but which need to be addressed. Sobran has mentioned that his editors pruned much of what he had originally written (and we all are thankful he did not keep the working title of *Outing Shakespeare*), and it is too true that many anti-Stratfordians are prolix to the point of asphyxiation. Yet there are times when he misses making a connection in this great swarm of material. He will repeat himself over several chapters for the sake of emphasis, at the expense of more complete material. For example, the analysis of *Hamlet* is given a near-royal treatment in this book, yet Sobran misses the connections with both Horatios in Oxford's life (his cousin Sir Horace Vere, a brilliant English general, and the choir-boy Orazio Cogno–

both of whom are mentioned in other contexts), and the rest of the Peregrine Bertie report from Denmark which refers to "Danskers" and the arras in the hall, and the royal guest list which included a certain Guildenstern, and two members of the family Rosencrantz. As long as the road is clear, why not send the whole battalion down it?

Although there are many questions lurking in the shadows (exactly how did they bring off this imposture with William Shakespeare? Why were there no private correspondence mentioning Oxford as the author?), it is interesting to watch Mr. Sobran weave his tale of Oxford. Since his conclusion is one which can stand independently of them, one can only hope that he will continue, in another volume, to address the remaining questions.