

The Shakespeare Clinic and the Oxfordians

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Help Gods, help saints, help sprites and powers that in the heaven do dwell,
Help ye that to wail aye wont, ye howling hounds of hell;
Help man, help beasts, help birds and worms, that on the earth do toil;
Help fish, help fowl, that flocks and feeds upon the salt sea soil,
Help echo that in air doth flee, shrill voices to resound,
To wail this loss of my good name, as of these griefs the ground.

Author A

O God, Horatio, what a wounded name,
Things standing thus unknown, shall I leave behind me!
If thou didst ever hold me in thy heart,
Absent from felicity a while
And in this harsh world draw thy breath in pain
To tell my story.

Author B

Author A is Oxford, “Fram’d in the Front of forlorn hope,” published in 1576 (May, 1980, 27-28), hereafter, “Help fish, help fowl.” Author B is Shakespeare, *Hamlet*, 1601 (V.ii.344-49) Hamlet’s death scene. Does it matter who wrote which? It does to us. Does it matter whether Oxford and Shakespeare were the same person? It does to us, too, and we would guess that it also matters to our critics, John Shahan and Richard Whalen, and to most readers of *The Oxfordian*. Do they sound like the same person? We think not, and so, in a sense, do nine out of ten of the top scorers on our Shakespeare Golden Ear test. Are they a stylometric match with each other? Anything but. Is there anything in the documents to show that A=B? The leading documents experts say no, there’s not a scrap of direct evidence that A=B, quite a bit of evidence that A≠B, and also plenty of evidence that B=B. The evidence we have seen overwhelmingly supports the experts, not our Oxford-believer, Shakespeare-denier critics.

Eliminating Oxford

Do any or all of these problems eliminate Oxford as a credible Shakespeare claimant? We certainly think so privately, and we have not hesitated to use the e-word, *eliminate*, in public to describe our Shakespeare-Clinic students’ wholesale removal of 37 testable claimants from the “credible” list. Their sponsor, the Shakespeare Authorship Roundtable, had asked them to use new-optics stylometry to shorten the list of credible, testable claimants, Oxford included, by comparing their known poems or plays to Shakespeare’s. This they did, decisively, and with enough overkill to make statistical refutation very difficult. By their rules, they cut the list to zero. None of the tested claimants came within a mile of matching Shakespeare. Nothing in the 20 years since, which included the “Shakespeare Wars” over the *Funeral Elegy* and several Oxfordian attempts to distinguish or refute their findings, has weakened them. Several subsequent developments, such as new tests, new texts, and a succession of failed refutations, have strengthened them, as we see it, to the point where we felt safe in offering a £1,000 bet that no one could find an untested play not by Shakespeare that would fit within our Shakespeare profiles (Our 2004, 363-368). There are more than 200 such plays out there eligible for testing. All our takers

need to find to win the bet is one. And we're willing to pretest, or let them pretest with our software, with no penalty, if they will just furnish the plays modernized and edited for proper testing. In other words, with a bit of editing effort, they could get all 200-plus plays pretested before risking any money. We would get them edited for no more than £1,000, or, more likely, if we are right, for nothing but the cost of our time spent testing them. And maybe, just maybe, one of these would turn out to be the Lost Shakespeare, for which £1,000 would be an absurdly low price to pay. What's not to like? Shahan, Whalen, and TOX editor Michael Egan, have all denounced our bet as "phony," and offered their own counterbets reflecting their own priorities. But we can't help noticing that, after all the bluster, none of them has taken us up on ours, nor has anyone else. Does that tell us something?

Doubts and Declarations

Under the circumstances, we consider the e-word not such a bad description of what our students did. On present evidence, they settled the Authorship Question conclusively, at least for the claimants they tested, and ruled out all but Shakespeare. To us, the big AQ is now much more settled, and therefore less interesting, than the smaller, less settled AQ, "What did Shakespeare write?" If we had our druthers, we would be studying *Double Falsehood* and *Arden of Feversham* for Shakespeare's hand right now, rather than doing another slog through Shahan-Whalen's latest, longest, and most strident bill of complaints (their 2009). We have not seen much good evidence to support their Declaration of Reasonable Doubt, and are in no hurry to sign it.

On the other hand, especially in a field which has long been underserved by professional scholars, critics do have value. You can learn things from them that you can't learn from your fans. Other eyes can see things that yours miss, especially if they are looking harder in different corners than you are, and from different angles. And, *faute de mieux*, the authorship groups have often been the only game in town. Hence, we have kept up our contacts with the Shakespeare Authorship Roundtable and the Shakespeare Oxford Society, and we try to keep half an eye on denier literature. We have welcomed good-faith efforts to disprove or distinguish our findings, including Shahan-Whalen's earlier efforts, and have gone far out of our way to respond to them. To facilitate such contacts, when discussing the case of individual claimants such as Oxford, we have politely tried to minimize our use of the e-word. After all, our methods were new, hadn't been closely scrutinized by people with different perspectives, and could therefore be considered provisional. What looks true in gross at first glance may not always be so in particular at second glance, and that what rings true for one generation may not ring so true for the next. We've always warned our readers that ours will not be the last word, and we've tried to keep our work open for fruitful discussion.

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You don't have to be a political scientist to realize that, for the groups most aggressively seeking a conclusive answer to the AQ, actually finding one, as we think our students did, could be catastrophic. Who would join the SOS or the SAR if everyone knew their favorite claimant had been conclusively ruled out—or ruled in, for that matter? The March of Dimes almost collapsed when it finally got polio conquered. It had to scramble to find some other disease to conquer to keep itself in business, and it took care to pick one, birth defects, that nobody expects to be con-

quered anytime soon. Thus, when it asked the Shakespeare Clinic to rule out implausible claimants, the relatively low-budget, open-minded Shakespeare Authorship Roundtable was taking a real institutional risk. What if the students ruled them all out, as it now appears they have? What would the group do then? For larger, wealthier, less open-minded groups like the Shakespeare Oxford Society, more is at stake, it's a greater risk to the group, and it's understandable that their leadership was not, and is not, eager to have research like ours on the agenda—and, if it gets on somehow, it's not surprising that they want whatever damage control they can find.

Politeness and Respect

We think we can profit from informed, civilized discussion and debate of some authorship questions with people who do not share our views. But we don't expect much profit from endless loops of recreational wrangling and name-calling, and we have tried to avoid those. Shahan-Whalen's earliest efforts seemed to fall into the first category, and we have long tried to treat them with politeness and respect and not describe their arguments and evidence as "garbage" or "phony" (see their 2009 248-49, 260, 264). Except where they, or the *Tennessee Law Review*,

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have prodded us into debate mode, we have generally soft-pedaled the e-word in discussing individual candidates, while continuing with it for the whole claimant group. We have accommodatingly used those off-beat denier terms like *Shaksper*,

Stratford Man, and *grain dealer* to help the discussion along, and we have repeatedly downplayed or sidelined our skepticism about those long chains of inference from which some key links are obviously missing.

Twenty years later, after a ton of intense adversary scrutiny, we wonder whether we didn't worry too much about being tentative and polite. Our findings are remarkably unscathed, and the counterarguments in tatters. Our attackers in the Shakespeare Wars have long since surrendered and abandoned the field. Until lately, even Shahan-Whalen have not seriously contested the gross mismatch we found between observable Shakespeare and observable Oxford. After surviving 20 years of assault essentially unscathed, we and others consider our findings stronger and less tentative than before, strong enough to bet on and not get called. Moreover, Shakespeare deniers have turned out to be remarkably—though not totally—oblivious to our and others' strong contrary evidence, and denier groups manage to fly on unaware or unconcerned that there is nothing but fumes in their gas tanks. Most of them, already oblivious to the bad news we brought about their claimant's viability, have been even more oblivious to the softening language we have used to try to make it more tentative, bearable, and discussable.

Shahan and Whalen are exceptions to this. They're well aware of our bad news but insist that we should have soft-pedaled harder in the 2004 Tennessee debates, shouldn't have used the e-word at all, didn't do nearly enough recanting and foreswearing of it in 2007, and still owe them an exorcism—not for anything we said in our 2007 "Other Car" article, but for what we said in 2004 to a different audience, with different rules of engagement, and less need for cosmetic reticence about the problems of the Oxford claim (Shahan and Whalen, 2009, 237-38). We're not persuaded.

New-Optics Stylometrics

The fullest discussion of our new-optics stylometric analysis is still our 140-page, web-accessible “Oxford by the Numbers” (*Tennessee Law Review*, 2004). We invite TOX readers to consider it, and, if they wish, try to refute it, but we don’t propose to reproduce it here. The short of it is this: our stylometric analysis of both authors’ known works says it’s wildly improbable that Oxford and Shakespeare are the same; their styles are too different. By our tests Shakespeare’s style during his writing lifetime was remarkably consistent—and so, to all appearances, was Oxford’s. In the few cases where Shakespeare’s changed, such as line endings and speech endings, it changed in a consistent way, generally from less frequent to more. This consistency permitted our Shakespeare Clinic students to calculate stylometric profiles for both early and late Shakespeare and to show that none of the 37 testable claimants, early or late, came anywhere near being a plausible Shakespeare match. Subsequent validation testing covering all 40,000 words of Shakespeare’s known poems, and 210,000 words of his play verse, in 1,500-word blocks, indicated that, with sufficiently long text samples, the students’ tests were 100% reliable in saying “could-be” to Shakespeare’s known poems and at least 95% reliable in saying “could-be” to his plays—and 96-100% reliable in saying “couldn’t-be” to 40,000 words of poems by other authors (our 2004, 357). In Oxford’s case, “the odds that Shakespeare could have produced Oxford’s test patterns by chance are between 400,000 and 1.5 quadrillion times worse than the odds for Shakespeare’s own most discrepant block. These odds are also worse than the odds of getting hit by lightning (our 2004, 370-71).” We believe that this strong, well-validated stylometric evidence, unless somehow refuted or explained away, would be sufficient to eliminate most suspects in a criminal investigation.

Shakespeare Not Oxford

Stylometric evidence, of course, is not the only evidence worth considering. Before using the e-word, we might also want to look at conventional documentary evidence, and perhaps at usable intuitive evidence, if available, to see if there is a serious conflict with our stylometric evidence. We have found none. Writing in the same *Tennessee Law Review* issue, the two leading authorities on Oxford’s documents, Steven May (2004) and Alan Nelson (2004), were, if anything, more emphatic than we were in rejecting Oxford as a plausible claimant. Nelson says this: “A legal community with respect for documentary evidence must conclude that William Shakespeare wrote the plays and that Oxford did not” (Nelson, 2004, 168).

May says this:

Since nothing in Oxford’s canonical verse in any way hints at an affinity with the poetry of William Shakespeare, we must believe that Oxford made the leap from his mid-century poetic style to the late Elizabethan style without leaving a trace of transitional writing. We must next believe that, after publishing both verse and prose under his own name, the Earl was suddenly afflicted with a manic compulsion for anonymity. This compulsion did not lead him, however, to protest published references to him as a playwright or the ongoing publication of verse under his name or initials. He then enlisted, not someone among his own players, but the obscure actor from Stratford to set forth his own creative writing as Shakespeare’s work and under his name. We must next believe that De Vere and William took the secret to their graves, that they fooled everyone... What a tale of clandestine intrigue, bizarre passion, plus the wholesale outwitting of friends, family, colleagues, and acquaintances for over twenty years. Can you believe it? ... I cannot (May, 2004, 242)...

Nelson and May found plenty of direct evidence associating Shakespeare himself with Shakespeare's poems and plays, but not a scrap for Oxford. Nelson found it hard to believe that the earl would never have spelled his own title *Oxford* in his poems and letters (Nelson, 2004, 152. *Oxford* was the default variant), while always spelling it *Oxford* in the plays—or that he would have chosen the deniers' despised country grain dealer as his front man—or that the Folio could conceivably identify someone of Oxford's rank as *Mr.*, *Maister*, or *Friend and Fellow* of commoners Heminges and Condell, or *servant* of the earls of Pembroke and Montgomery (Nelson, 2004, 155, 168). All these titles were appropriate for the "Stratford man," but not for an earl of Oxford's standing. By 2004, neither Nelson nor May put much stock in the "stigma of print" argument, that noble poets could not abide seeing their poems and names in print. Too many, including Oxford, obviously could (May, 2004, 234-35; Nelson, 2003, 386 (stigma argument is "thoroughly discredited"))

Contested Will

This summary barely scratches the surface of Nelson's and May's documentary research. We would encourage anyone who really cares who wrote Shakespeare to consult their articles directly. But, like our stylometric evidence, it is devastating to the case for Oxford. The same may be said for James Shapiro's *Contested Will: Who Wrote Shakespeare?* (2010), the first full book on the Authorship Question by a Lit Department professional in more than a century, maybe the first one ever. *Contested Will* was written to chronicle and explain the deniers, not to refute them, but Shapiro is anything but a Shakespeare denier. He adds significantly to the case for Shakespeare, and against Oxford, with a light, understanding tone like May's and Nelson's, which we consider more persuasive than, say, the polemics of Samuel Schoenbaum, who castigated denier tracts as "thousands of pages of rubbish, some of it lunatic rubbish (Schoenbaum, 1970, 530, 627)."

Shapiro pointed to subtle indicators in the plays of close involvement with the changing circumstances of Shakespeare's company, the King's Men—identifying by name his fellow actors, such as John Sinklo or Will Kemp, with the parts they played, or penciling himself in, in place of Kemp, to deliver a special epilogue for *Henry IV, Part II* in a court performance before the Queen.

It is inconceivable that any of the rival candidates for the authorship of the plays associated with the court—Francis Bacon, the earls of Oxford, Derby, Rutland, Mary Sidney, to name but a few—could possibly have stood on the stage at Whitehall Palace publicly assuming the socially inferior role of player, and spoken these lines. (Shapiro, 2010, 229-34, see 203).

Once again, a top-drawer documents person has found plentiful documentary evidence for Shakespeare, but none for Oxford. See *Contested Will* for more detail. We are more consumers than producers of documentary evidence, but the scholars' external documentary evidence looks entirely consistent with the internal stylometric evidence that we have produced: the Shakespeare links are strong and plausible, the Oxford links are weak and implausible, or "spectral," as Alan Nelson puts it.

Golden Ears

Finally, there is intuition. No doubt devout deniers would brush aside our comparison between Author A's "help fish! Help fowl!" passage and Author B's "Hamlet's death scene" passage, arguing, as Shahan-Whalen have, that it's a subjective question in any case and, even if the styles

are blatantly different, it's only because of the "fact" that "help fish!" must be a very juvenile work. We see the differences as more blatant than subtle; see our 2004, 393. But, of course, that no more makes a fact of our perception than Shahan-Whalen's perceptions that Oxford must have written his poems in his teens, or that every poet writes clumsy juvenilia, or that Oxford must have written *Woodstock* or *Hamlet*, make facts of these. None of these are facts, only perceptions or possibilities till confirmed or refuted. But we do have more than just our own—and Steven Mays' and Alan Nelson's—impressions that Oxford's poems don't sound much like Shakespeare. We asked the 20-odd highest scorers on our Shakespeare "Golden Ear" test whether they thought the last two stanzas from Oxford's "Wing'd With Desire" (May, 1980 No. 12, 1582, pp 34-35 (not "Help, Fish," which we believe would have been all too obvious) sounded to them like Shakespeare. These high-scorers, as a group, have been 95% accurate in identifying short passages by Shakespeare. 89% of them said the Oxford passage did not sound like Shakespeare (our 2008, 11). Neither the ear, the documents, nor the computer, in our view, leave much room for optimism for the Oxford cause.

Interactions with Oxfordians

We have had a long history of interaction and debate with Oxfordians, with many ups, downs, ons, and offs. Elliott's father was a prominent Oxfordian, Rhodes Scholar, poet, advisor to six Presidents, "Eagle among sparrows" in the Harvard Government Department, and co-founder of the Shakespeare Oxford Society. He is still a fixture of its honor roll of skeptics. There would have been no Shakespeare Clinic had not the younger Elliott, and the sponsoring Shakespeare Authorship Roundtable, been curious as to whether his father,

who died in 1979, was right. The Clinic also benefited from the presence (then) of the Francis Bacon Library in Claremont, with one of the three best Authorship Question collections in the world.

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So the starting players were open to the AQ, probably more Stratford-skeptical than not, but in no way bound by any kind of family or institutional loyalty oath.

As if on cue, our own Lit Department grandees denounced the Clinic as "madness" and "idiocy," not just for asking the forbidden question, but for trying to answer it with computers. They knew their lines. But it seemed odd at the time, and it still seems odd, having deplored with fervor the Authorship Question themselves, and wished with fervor that it would go away, that they should have been so officially put out at the thought of someone else actually trying a new way to get it settled and done with. But put out they were, and it was the wholesale abdication of the regulars like them which left, not just the deniers' side, but the defenders' side, if it came to that, wide open to irregulars like us and our students. If our students had had to wait for our Literature Department, or almost any other American Literature Department, to come up a Shakespeare Clinic, they would still be waiting. See <http://www.cmc.edu/pages/faculty/welliott/shakes.htm> for a mini-history of the Clinic.

In the earliest days of the Clinic, Oxford momentarily looked like a Shakespeare could-be, and Elliott got many invitations to Oxfordian gatherings to give progress reports. When the evidence turned against Oxford, the invitations stopped abruptly, or were rescinded, perhaps for the reasons of institutional self-preservation discussed above, perhaps simply out of reluctance to "provide a

platform” for bearers of unwelcome news. In this case, we got our platform anyway, thanks to the help of a few SOS dissenters, in another room of the same hotel. Many of the faithful attended, and their behavior was exemplary. It wasn’t the last such rescission. We’ve also had debates canceled, articles requested and then unrequested or else drastically cut and stripped of supporting evidence.

We count these, and the episodic outbursts of name-calling, as downs in the relationship. There was also a decade-long dry spell in the relationship, after the Clinic’s initial burst of media coverage in 1990, for tentatively ruling out all the poet claimants, Oxford included. After a brief, intense flurry of name-calling, the Shakespeare Oxford Society and all the other authorship advocacy groups lapsed into near-total silence: no discussion, no counterarguments, one debate invitation promptly rescinded, nothing more. Our last set of students went home unheralded in 1994, having achieved essentially the same results with playwrights that the 1990 group had with poets—that is, by their rules, eliminated everyone they tested but Shakespeare. They also eliminated every play of the Shakespeare Apocrypha, including *Thomas of Woodstock*, as plausible works by Shakespeare solo. None of these came within miles of matching Shakespeare. We developed and refined their findings a bit, wrote them up for publication in *Computers and the Humanities* (our 1996), and prepared to go back to our previous lives.

The Shakespeare Wars

But it was not to be. We were immediately engulfed in the Shakespeare Wars with Donald Foster (Rosenbaum, 2006, Ch. 5, Vickers, 2002, Epilogue: “the politics of attribution”). Foster, ingratiating and helpful at the outset, when there was no obvious conflict between our findings and his, became an implacable foe when our tests showed his precious *Funeral Elegy by W.S.*, which he billed as “indistinguishable from Canonical Shakespeare,” didn’t look like a Shakespeare match. He warned us that our methods were “not just doubtful, but certainly wrong,” and that any attempt on our part to publish or present our results would destroy our reputation. When we went ahead and published anyway, he did his best to deliver on his threat. He quietly arranged with the then-new editor of *Computers and the Humanities*, his Vassar colleague, to print his scorching, uninvited, hardball denunciation of our final report in the same issue, warning readers that we “cherry-picked,” “played with a stacked deck,” “exiled inconvenient data,” and used “worthless numbers,” and should never have been published unless to have our “methodological madness” exposed (Foster, 1996; Elliott and Valenza, 1998), the whole backed up by pages and pages of dense, rigorous-looking technical analysis. A subsequent rejoinder (his 1998) added “foul vapor,” “madness,” and “idiocy” to his inventory of opprobrious words, and yet more pages of rigorous-looking analysis.

Foster by then was no longer an authorship David, struggling to get his offbeat studies and methodology accepted by an indifferent-to-hostile old-guard professoriate suspicious of authorship studies and very suspicious of the computer methods he was willing to try. High-level, old-guard readers had rejected his *Elegy* manuscript at the Harvard and Oxford University Presses, and fear of their wrath had led him to soft-pedal his otherwise-electrifying Lost Shakespeare discovery when the Delaware University Press finally published it in 1989. Coming, as it did, from a midmarket press with an unorthodox methodology impenetrable to most lit-department technoskeptics, and heavily shrouded author’s conclusions, no one paid much attention to it.

But by 1996, when we and our students were not even Davids, Foster was looking more like Goliath. He had unshrouded his conclusions and announced that that he had at last gotten his Moby Dick of an *Elegy* vindication in the net. Moreover, he had just scored a smash hit in his

first outing as a forensic analyst. He had sensationally, and correctly, confirmed previous identifications by several others of Joe Klein as the true author of the best-selling *Primary Colors*. When Klein hotly denied it, a further researcher found smoking-gun evidence, forcing him to confess. Foster got the credit, and it helped sell his revamped *Elegy* claim.

For the uninitiated, it was much easier to understand and judge the case for Klein than the case for the *Elegy*, and Foster's triumph with Klein lent credibility to his ringing declaration that his new analysis conclusively showed the *Elegy* to be "indistinguishable from Canonical Shakespeare." He became a front-page media sensation, both as a celebrity forensic analyst and as the possible discoverer of a Lost Shakespeare. Scribes touted him as "the Sherlock Holmes of literary ascription." It is true that Foster's subsequent ventures as a forensic analyst were not nearly so successful as *Primary Colors*. He fingered two different suspects in the Jon-Benét Ramsey case and one in the 2001 Anthrax-attack case, all with resounding certitude, all later cleared. However, these "speed bumps on the learning curve," as he liked to call them, and his embarrassing *Elegy* retraction were years away when he was at war with us.

At one point Foster boasted in a letter to Patsy Ramsey, "In the 14 years I have done scholarly text analysis, I have never made a substantive error." Many were convinced that this was so.

He had also become an overnight sensation within the profession, at least in the U.S. (scholars elsewhere knew less of Joe Klein and trusted their ears more than Foster's computer). U.S. professors, many of them confessing they could not fathom Foster's methods, seemed to be as enthralled by them in the 1990s as their predecessors had been scornful in the 1980s. Several hailed his *Elegy* ascription as irrefutable. "Over the next 10 or 20 years this will become part of the Shakespeare canon, and no one will question it." "[His] methodology is flawless. If anything is likely to get included in the canon, this is going to be it." "I'm vastly impressed by Foster's scholarship. No one has attempted to discredit it." Foster himself, having fought hard to silence us and other critics, made much of the supposed lack of nay-sayers "at this late juncture" (for example, his Shaksper postings of 17 January and 10 March 1996), and people believed him.

A Streetfighter

At one point Foster boasted in a letter to Patsy Ramsey, "In the 14 years I have done scholarly text analysis, I have never made a substantive error." Many were convinced that this was so. He was the centerpiece of a UCLA conference on the *Elegy*, gave the plenary address at the International Shakespeare Association convention, and was honored when it was revealed that all three forthcoming American editions of Shakespeare's *Complete Works* would include the *Elegy* as "possible Shakespeare." Even the Oxfordians were scrambling, as ever, to show that the *Elegy*, though plainly written in 1612 to commemorate someone who had just died in 1612, must have actually been written much earlier, before Oxford's death in 1604, to commemorate someone else, but got hastily modified in 1612 to honor a new decedent. Or perhaps, they speculated, Oxford's death, like Marlowe's, had been faked (below), and he was still there in hiding to write the *Elegy*. How much more ascendant could you get?

No rational person would want to tangle with a person like Foster even before or since his ascendancy, let alone right in the middle of it. Despite his tentative talk in *Elegy* by W.S., and despite his *basso continuo* of outward appeals for contrary evidence, he was an Ahab in his pursuit of vindicating the *Elegy*, a Talleyrand in courting allies, a persuasive self-promoter, a skilled political infighter, and a terror to anyone he saw as an obstacle. His critics described him as a

streetfighter, much quicker, if you crossed him, to threaten you with a whacking or a lawsuit than to admit error (Love, 2003, Vickers, 2004). Who would want to take on such a person at the peak of his influence?

Certainly not us. We're not in Foster's league as tough guys, infighters, or superstars, and we try to steer clear of people looking for street fights, especially black-belts like Foster. Nobody likes to take punches. In retrospect, we were naive to suppose that Foster's pleas for counter-evidence were meant for our ears or our counter-evidence. On the other hand, we had what looked like a strong *prima facie* case, and we did think that our students, in six years of work, had done something remarkable, showing every claimant, and every apocrypha poem and play they could test, to be gross mismatches with Shakespeare. What Lit Department could say such a thing? That seemed to us, and to *Computers and the Humanities'* former editor, Glyn Holmes, and his scholarly outside readers, something worth bringing to scholarly attention. Foster was asking a lot of us to throw it all away, purely on his say-so, and cancel publication or else have our reputations destroyed. Some might say he was not asking us in a very conciliatory way. He himself supplied no more than a scrap of counter-evidence, only the warning that we were risking our reputations; his own supposedly conclusive vindication of the *Elegy*, announced with fanfare in late 1995, was not published till months later, in the fall of 1996; and his case turned out to be weak.

Methodological Madness

So we spent the winter of 1996 vainly trying to mend fences with Foster, but did not ask *Computers and the Humanities* to stop the presses. Perhaps we should not have been surprised, when we finally got the issue, to find our report quietly repackaged as a "debate" with Foster which a casual Lit Department reader might well have thought a fulfillment of Foster's threat that our reputation would be destroyed by publication. The Sherlock Holmes of literary ascription, who had never made a mistake, had taken it upon himself to read and judge our work just in time to spare *CHum* readers a taxing encounter with our long, technical study, which, he said, should never have been published save as a bad example of "methodological madness." No need to read. And he had those pages and pages of impressive-looking technical analysis to back it up. Lit department people are no more at home with our kind of technical discussion, or Foster's, than we are at home with street fights. But Foster, unlike us, had a glowing reputation in the trade. He offered them a tempting way to finesse the technical stuff, along with our students' remarkable findings. Why bother to puzzle through our technical data, or even his, if the infallible Holmes had found it was nothing but Foul Vapor?

At that point, with our final report so quarantined, all hope of going back to our lasts was dashed. We were caught in what would later be called the Shakespeare Wars. Our students had no way of speaking for themselves. If anyone spoke up, it had to be us. Short of capitulation or simply slinking off the field, there was no easy way out.

But there were two harder ways, and we chose both. The first was to try to make a real debate of it, and take Foster on with symmetrical rules of engagement and rebuttal, and notice to both sides that a debate was in progress. We asked for that, got it, and had a six-year series of skirmishes with him which hardly anyone has bothered to read, but which at least permitted us to go over his points, tediously one by one, and to demonstrate that nine-tenths of his hardball accusations of our error were nowhere near the plate (our 1998, 2002).

A few were true but trivial. We corrected all of them, and, together, it changed our bottom-line results by a tenth of one percent, a reassuringly low rate of error for a group of amateur undergraduates of varying ability levels using novel methods never before tried or tested by anyone

else. The same may be said for their advisors. Where you have trials as far off the beaten track as ours, you expect some errors, but the attrition rate turned out to be surprisingly low, far too low to change any outcomes. Few Lit scholars knew or cared, but for us the effort was not wholly wasted. It was a powerful vindication to suffer such trivial attrition from such a massive, no-holds-barred attack by an authorship black-belt. If Foster couldn't find serious errors in two years of trying his hardest, who could? "If Foster's time is really worth \$250 an hour," we said, "we would hate to think what we must owe him for his services." It also got our long, detailed technical analysis onto the record, sparing us, and our readers, from having to go through it again in subsequent discussions like this one, and permitting us more freely to use compact, conclusory language.

Our second response was to take a closer look at the *Elegy*, first in a letter to Foster, and eventually, when he did not respond, in the *Shakespeare Quarterly* and *Literary and Linguistic Computing* (our 1997 and 2001), having been turned away sight-unseen by *CHum*'s war-weary editor. These were, respectively, the first stylometric critiques by anybody of Foster's ascription of the *Elegy* to Shakespeare, and the first stylometric confirmation that it could easily have been written by John Ford. The news was bad for Foster's ascription. The unique "thumbprints" that the *Elegy* supposedly shared with Shakespeare alone turned out to be plentiful in Ford's poems, and it had far too many countable features which fit Ford's profiles, but not Shakespeare's. Our primitive odds calculations showed it 3,000 times more likely to have come from Ford than from Shakespeare. Later, more sophisticated calculations showed the same kind of wild Shakespeare improbability levels that we found of Oxford's published poems (see "Elegy" and "Oxford poems" in our 2004, 423-24).

A Threatened Whacking

Unlike our *CHum* rejoinders, these did get attention in the trade, starting with Foster, who promised us a whacking, yet, ever the gentleman, offered Valenza a chance to bail out before he administered it. Valenza ignored him, and, more than a decade later, the promised whacking has still not been supplied. Fortunately for us, our articles also caught the eye of Brian Vickers, now Sir Brian Vickers, who was preparing two blockbuster books on authorship, (his 2002 and 2002a), one of which, '*Counterfeiting*' *Shakespeare*, included hundreds of pages of demolition, both stylometric and conventional, of Foster's ascription. It also included a mini-history of Foster's altercations with us and others, not flattering to Foster (Vickers, 2002, 422-65). Foster, getting wind of it, but without having seen it, threatened to sue the Cambridge University Press for libel (Vickers, 2004). The threat did hold up publication for several months while CUP's solicitor checked it for actionability. But, unlike us, Vickers was not someone Foster could silence or sideline simply by pulling rank, even at the pinnacle of his success, far less after the cracks in his "flawless" stylometric case for the *Elegy* had started to show. In terms of rank and scholarly firepower, Sir Brian Vickers was to the Sherlock Holmes of Literary Ascription as a battleship to a jet-ski, and the jet-ski's case for the *Elegy* was now looking far from flawless.

For Foster, humiliation loomed, and he did what he could to stave it off and blunt it. He managed, with his threatened lawsuit, to block Vickers' book for six months, and then, to the surprise of many, did the one thing he and his sidekick, Rick Abrams, could do to keep people from buying it and reading it—surrender. Not to Vickers or us, to be sure, but to Gilles Monsarrat, an old-optics French Ford scholar, for showing the *Elegy* was really Ford's (Monsarrat, 2002), and with an observation touchingly modest for one who had just threatened to sue Vickers' publisher: "No one who cannot rejoice in the discovery of his own mistakes deserves to be called a scholar (Foster, 2002)." We couldn't have said it better ourselves.

It didn't matter to whom he handed his sword. The Shakespeare Wars, and Foster's career as an infallible judge of Shakespeare ascription, were over; Monsarrat's side had won, Foster's lost; and it seemed that there was no longer any real need to buy, read, or think about Vickers' book or the *Elegy*. It wasn't the last of Foster's surprises. He abandoned Shakespeare studies altogether, including his old, Ahab-quest to vindicate the *Elegy*, and took up a new persona, that of Inspector Javert, and a new quest, to use his forensic detection skills to bring high-profile modern suspects to justice. Unfortunately, as we have seen, he was unable to replicate his success with Joe Klein. All three of the "perpetrators" he proudly and confidently fingered, Andrew Ramsey, Patsy Ramsey, and Steven Hatfill, were cleared.

W. Ron Hess

Foster's attacks were a big obstacle to our effort to explain our findings and methods to the profession, and they closed our main outlet, *Computers and the Humanities*, to us indefinitely. But they did force us to stay in the authorship game, to refine our old tests and adapt them to work on shorter text blocks, and to look for new tests and new ways to estimate composite discrepancy between one author and another. We count it a plus that he tried hard but failed to demolish our results. Our own reanalysis of the *Elegy* (and of *A Lover's Complaint*, also a Shakespeare mismatch by our rules) was far narrower and less significant than what our students had done, but it got much more attention from the profession than the students' groundwork had, perhaps because

It didn't matter to whom he handed his sword. The Shakespeare Wars, and Foster's career as an infallible judge of Shakespeare ascription, were over.

our topics were more manageable and respectable than the big AQ (Our 1997, 2001). Foster could no longer block us, or even whack us effectively, and we were starting to contribute episodically to the Global

Electronic Shakespeare Conference (Shaksper), a listserv of professional and amateur Shakespeare buffs forbidden to ask who was Shakespeare, but very interested in what Shakespeare wrote. By the end of the Twentieth Century we were starting, at last, to get on the radar of people who do Shakespeare for a living.

We also were back on Oxfordian radar. They had a new journal, TOX, and W. Ron Hess took us on in the second issue (Hess, 1999), attempting to make a more convincing Oxfordian Shakespeare chronology than previous ones by Eva Turner Clark and Dorothy and Charlton Ogburn, and to argue that his new backdating dating made some of our analysis beside the point. At the invitation of TOX editor Stephanie Hopkins Hughes, we responded, concluding that Hess's new dates were better aligned than Clark's and the Ogburns' with Shakespeare's stylistic trends obvious in conventional dating, but noting that, by his own admission, many of his new dates were pulled out of a hat. We questioned his across-the-board, undocumented backdating of Shakespeare's plays to twelve years before wherever we and the *Riverside Shakespeare* had put them, and didn't think it matched how plays and films are marketed today, with much hoopla over the latest, and very little over whatever was the latest thing twelve years ago.

Hess had too little to say about Shakespeare's last three collaborations with Fletcher, clearly datable to 1612-13, and his relentless backdating, which, if believed, might solve the back-end problem of Shakespeare's plays continuing to evolve long after Oxford's death, gave Oxford problems at the front-end which conventional dating did not. Some of Oxford's apparent mismatches with Shakespeare, otherwise explainable-away by his being much earlier than Shakespeare, become much harder to explain when he and Shakespeare are reclassified as contempo-

raries. We concluded that Oxfordians had not yet come to consensus on Shakespeare dates, and that attempts to solve back-end (late) problems by backdating Shakespeare produced problems at the front end (early). Hess's new dating seemed to us an improvement over earlier Clark/Ogburn dating, but was still much more speculative and less documented than orthodox Stratfordian dating and fell short of what was needed for an Oxfordian chronology to match both the Shakespeare documents and the observable trends in Shakespeare's plays (our 2000).

Shahan and Whalen

There followed a nine-year exchange of articles between us, John Shahan, and Richard Whalen in *The Oxfordian* and the *Tennessee Law Review* (Shahan, 2001, Whalen, 2004, Shahan and Whalen, 2006 and 2009; our 2000, 2001, 2007, and the present article), totalling over 200 pages of argument and evidence. There was also a Shahan-Elliott debate for the Shakespeare Authorship Roundtable in 2007. The fullest and most important of these on our side was our 2004, describing our methods, assessing their strengths and weaknesses, and applying them to the case for Oxford. It had 131 pages of analysis, including 57 pages of tables and appendices, and was by far our longest publication on the authorship question. We considered not just the glaring stylometric mismatches between Oxford's poems and Shakespeare's, but also the various arguments that Shahan-Whalen have made for getting around them—heroic attempts to backdate Shakespeare's plays to the Stratford Man's teens, backdate Oxford's poems to Oxford's teens, distinguish some of them as songs, not poems, and suppose that the surviving Oxford poems we have are not the only ones he wrote, that the others, if we had them, might be more like Shakespeare, and that, therefore, we couldn't claim to have eliminated Oxford because we haven't seen or tested everything he wrote.

Grains of Truth

We thought at the outset that some of these points might have a grain of truth, and have said so many times. We were certainly not eager to open another front in the Shakespeare wars, but Shahan-Whalen seemed less threatening than Foster, less bent on giving us a whacking, especially at the outset, and more inclined to concede some of our points. Could there be some common ground? It sounded like they weren't ordering us to shut up and go away, just to be a bit more cautious in our use of the e-word. It also seemed to us that they were gamely making the best possible case for Oxford that our evidence might still permit—in contrast to the Baconians, Marlovians, and such, who had no journal and have shown no interest whatever in finding whether our evidence has any bearing on theirs. Shahan-Whalen also seemed a refreshing change from those old, invitation-retracting Oxfordian bulls and from those other big-hearted Oxfordians who couldn't let well-enough alone and had to make Oxford not only the True Shakespeare, but also the true Gascoigne, Kyd, Greene, Lyly, Marlowe, and author of much of the Shakespeare Apocrypha. Unfortunately for them, most of these are poems and plays that we have tested and found to be gross mismatches with Shakespeare, but not subject to any of Shahan-Whalen's qualifiers. They're not songs, they're not juvenilia, but they are still on a different statistical planet from Shakespeare.

So we saluted Shahan-Whalen for taking up our evidence at all, for making the best case we thought they could make (though still less than convincing), and for not (for the moment at least) picking a street fight with us. We took them seriously, lapsed into denier patois, toned down, but did not forswear, our use of the e-word for purposes of discussion, looked in detail at every way they or we could think of to get around it. We also paid more attention to the documents, which

is easier to do when comparing just two authors than when comparing 38. We said yes to the Tennessee Law School symposium, sat through more discussion of the AQ, on both sides, than most Lit Department professors would tolerate, and learned from it, especially from May and Nelson's discussions of the relevant documents.

In our view, none of the evidence so developed could salvage the Oxford claim, and much of it was very damaging. Oxford's available poetry, once considered an enhancement to his claim, had been so heartily disavowed as unrepresentative of his work in full, that he seemed to have been moved from the category of "testable claimant," to whom we can reasonably apply the e-word, to "untestable," who cannot be eliminated by our methods (our 2004, 395). If so, and Whalen has said so in so many words (his 2004: "Oxford is untestable"), it would be a terrible demotion for his candidacy, from a small class of prime suspects with no more than a score or two of possibilities to a very large one of not-so-prime suspects with millions of possibilities (our 2004, 395). As we put it in 2006:

[R]emoving Oxford from the category of "testable claimants" makes him, well, untestable, and does in fact put him in the company of the Rosicrucians and the Earls of Rutland and Derby, and millions of other people from whom no poems and plays have survived, and whose claims rest not on what they have written, but on what they *might* have written (our 2004a, 395). If believed, it might rescue Oxford from elimination, but it also makes his candidacy much more speculative, less credible, and certainly less distinguishable from those of anyone else whose writing is said to be Most Excellent. Again, Oxfordians can't have it both ways. He is either a first-class, eligible, testable claimant who was on the boat but walked the plank, or he is one of many, many possible third-class claimants who can never walk the plank because they have nothing comparable and testable to show that they were even on the boat (our 2006, 19).

Unfortunately, we had to leave this and another 4,000 words of our original 10,700-word response to Shahan-Whalen (2006) on the cutting-room floor, by command of TOX's then-editor. She had asked for the response, not specified a length, and given us two weeks to write it. We dropped everything and got it done, at what seemed to us a reasonable length. But she had privately expected no more than 1,500 words; and she wanted us to cut it to that retroactively or else to postpone publication for a year. We wanted fuller documentation, not just our conclusions, didn't want the cuts, and chose the postponement. When the next year came, she still pushed hard for big cuts and a further postponement, so that her successor could decide, *de novo*, whether and how to publish it. To us, that sounded too vague and contingent, and too reminiscent of prior Oxfordian backtracking on invitations. She had requested the article; we had taken the request at face value and had gone to some trouble to deliver it; and we thought it should be published by her, and at a reasonable length, not held up for two years and then kicked down the road to her successor. In the end, after much travail and negotiation, both sides compromised. 6,400 words of it appeared in her final issue, a bit shorter than Shahan-Whalen's 7,000-word original, but in the same ballpark, and, to us, better than nothing.

In it we responded to Shahan-Whalen's central complaints, that Oxford's surviving poems are probably not all that he wrote, that some or all of them could be very early, or songs, not poems, and therefore not representative of his mature work, especially when you consider the glowing commentaries about Oxford as a poet or playwright that his contemporaries have left us. Why were we still using the e-word? We reiterated our polite formula, to use the e-word for wholesale rulings-out by our students, and to try for something more tentative when discussing a specific candidate. We cut out Oxford's arguable songs and juvenilia, retested what was left, and found it still a gross mismatch with Shakespeare (our 2007, 144). At the suggestion of Oxfordian

Nina Green we compared Shakespeare's songs to Oxford's supposed songs, and found that they didn't match either. All of Shakespeare's were from plays, but none of Oxford's. Three-quarters of Oxford's "songs" are in meters that Shakespeare almost never used for songs. Most of Shakespeare's songs are sweet, sunny, and entertaining, but none of Oxford's. Many of Shakespeare's have been reset to music and are still sung, but none of Oxford's accepted poems and just one of his "possible" poems (our 2007, 145-46). We found a dozen examples of great poets who published before age 21, none of them as clumsy as "Help Fish, Help Fowl" (our 2007, 147). We noted Oxford's own disinclination to "murder" a work in the "waste bottoms of my chests." We commented on the problems of the supposed Oxford Apocrypha, dozens of poems and plays nominally written by others, but claimed for Oxford by his admirers. Every one of these we have tested is a gross mismatch with Shakespeare, yet can't be explained away as songs or juvenilia. "Oxfordians," we said, can have either a big, indefensible Oxford Canon like Brame and Popova's, or a much smaller, more defensible one like what we take to be Shahan's and Whalen's—but it can't be both. If Shahan and Whalen are serious about their deflation of the Oxford Canon, they need to put some daylight between it and the inflated, irredentist, hopelessly indefensible Oxford canons still popular with other Oxfordians" (our 2007, 143-44).

We also spoke of a "yawning creativity gap between Oxford and Shakespeare, hitherto unmentioned:"

Judging from their surviving writing, Shakespeare was not just 100 times better than Oxford, he was also 80 times more productive. Shakespeare wrote about 3,500 lines of verse a year for twenty years, most of them immortal; Oxford, in the Shahan-Whalen scenario, wrote about 40 lines of woebegone juvenilia a year for ten years, then, for fifteen years, wrote nothing at all that he or anyone else could be bothered to save—but then, at forty-three, supposedly burst from his cocoon to become a literary supernova overnight. He suddenly moved from producing a teaspoon of vinegary Chateau Malheureux ordinaire a year for his first twenty-five years to producing a cup a year of incomparable Chateau Lafite Rothschild for his last twenty, a bigger, less credible jump for Oxford at age forty-three than Barry Bonds' fabulous hitting surge at age thirty-six, and before steroids, too (our 2007, 147-48).

The Oxford Apocrypha, if substantiated, might fill some or all of this gap, but only at the cost of an irreparably greater burden of stylometric discrepancy with Shakespeare.

We acknowledged that Oxford did have extravagantly admiring reviewers, but found that they described scores of others with equally flattering language, often proportionally to their rank and attractiveness as possible patrons. We couldn't exclude the possibility that some of the praise could be flattery and hype (our 2007, 148-49). Even if the flattery was sincere, and did hint that Oxford must have written something much, much better than "Help Fish, Help Fowl," we didn't think it showed that Oxford was Shakespeare. That idea seemed to us akin to those bumper stickers that say "my other car is a Porsche" (our 2007, 148-152). "Porsches of the mind," we said, "are hard to disprove. Nevertheless, we suspect that most people share our notions, and Bacon's, that what you see is more likely to be what you get than what you can't see. What we see, we fear, is still not very encouraging for the Oxford claim" (our 2007, 152).

As noted, we had to cut our "Other Car" article drastically (and the cut published version itself seems to have been purged from TOX's archives, though it's still up on our webpage. (See Bibliography, below). When you have to make cuts, conclusions and simplifications are generally what stay, and documentation, explanation, qualification, detail, and nuance are generally what get discarded or compacted for arguments on both sides.

Critics like Shahan-Whalen then notice and take us to task for condensing and paraphrasing

their arguments, along with our own, and for ignoring their demands for more documentation and detail. To their credit, they usually quote us accurately before going on to interpret us often not so accurately, but they were allowed plenty of time and space to do it. We were not. We thought a reasonable length for a response should be at least equal to that of the original article, longer if it's a minefield of complaints about ignoring or mischaracterizing their arguments and assertions that we, not they, are the ones who should bear the burden of proof. That's how Shahan-Whalen's "Apples to Oranges" (2006) looked and still looks to us (see Table 1, below). It takes much more time and effort to clear such a minefield than to lay it, especially if you are trying to be polite about it. We took the time last time but weren't allowed to publish all the results.

Auditing the Stylometricians

It's also how Shahan-Whalen's "Auditing the Stylometricians" (2009) seemed to us, only longer, more strident, and more reminiscent of those tedious old set-to's of the Shakespeare Wars. Here are some examples:

Mischaracterization is a pattern with Elliott and Valenza, as we have noted before ('Apples,' 119-20). They routinely distort our views and those of other Oxfordians. Note that we quote them extensively, while they rarely quote us directly. Instead they paraphrase, telling the reader what we say, or think, usually incorrectly. It's easier to knock down straw-man arguments than to rebut what was actually said (their 2009, 263).

Elliott and Valenza have a habit of attributing to others things that they never said (their 2009, 252).

Elliott and Valenza mislead by putting words in peoples' mouths (their 2009, 256).

This ... is the self-congratulatory statement of one claiming to have triumphed over opponents who are held in slight regard. Rather than a neutral, objective scholar, Elliott comes off as a partisan in an adversarial relationship with 'the Oxfordians (their 2009, 261).

We hereby put all who care about this issue on notice that it has still not been fully adjudicated as far as we are concerned. Elliott and Valenza have failed utterly in their attempt to pass the audit that we have conducted over the last many years (their 2009, 262).

Elliott and Valenza present their opinions of the quality of Oxford's verse without telling readers that they are totally subjective and unscientific. As stylometricians, they know, or should know, that readers will assume their opinions are based on science. They fail to disclose that their opinions differ from those of certain well-known authorities, including their own. They repeatedly mischaracterize Shahan's and Whalen's positions, to an extent that is hard to imagine is not deliberate. The extent to which Elliott and Valenza mischaracterize their statistical results is shocking. It calls all of their work into question. Their study appears scientific, but they exploit this to reach unwarranted conclusions, and make invalid claims. We have not scrutinized their findings and conclusions for other candidates, but those for Oxford inspire no confidence. Rather than objective, unbiased analysts, they appear to have a strong bias against the 17th Earl of Oxford and Oxfordians (their 2009, 262-63).

Elliott's and Valenza's claim to have eliminated Oxford as a Shakespeare candidate is unwarranted. We hereby challenge them to prove their claim before a panel of neutral experts knowledgeable in quantitative methods and critical analysis, without restrictions on evidence or arguments. The burden should be on them to prove their claim, the standard being 'beyond a reasonable doubt. We do not normally view wagers as an appropriate way to resolve such disputes; but Elliott and Valenza use their phony bet to claim bragging rights, continuing to say that no one has accepted it, even though the editor of this journal says he did. We therefore offer to bet £1,000 on the outcome, if they agree to the above terms. Michael Egan adds that he is prepared to meet Elliott's challenge any time, given fair

terms such as those proposed above. One panel should suffice for both issues (their 2009, 263-64).

Our Response

Help Fish! Help fowl! If we were the Earl of Oxford, we would be very upset by this unflattering talk, which strikes us as both disparaging and false, and call upon the hounds of hell to rescue our good name. If we were Shakespeare, we might say it more elegantly. But we are neither of these. Our first impression was that Shahan-Whalen were using more strident language than usual, trying to make a street fight out of the dialogue, burning their bridges with one last big burst of disparagement, and hoping to end the dialogue and permit them to claim victory.

But who are we to rely just on our impressions? Could stylometry help confirm or refute? Suppose some anti-Shahanian authorship group announced that we, and not Shahan-Whalen, were the true authors of their “Apples to Oranges” and “Auditing the Stylometricians” articles, but didn’t want anyone to know and formed a conspiracy to present the otherwise-improbable Shahan and Whalen as front men.

If we were trying to explore such a question with stylometry, we might well start with profiles of their favored words, or “badges,” such as *ignore*, *mischaracterize*, *bias*. Intuitively, we associate these with John Shahan and consider them, word for word, more dainty and polite in their disparagement than, say *madness*, *idiocy*, *lunatic rubbish*, *foul vapor*, or *misrepresent*, *mislead*, or *phony*, all of which suggest that the wild pitch is intentional, not inadvertent, or, at the very least that the speaker has gotten hot under the collar and is blowing off steam. Even the polite words, with enough repetition, do come across as heavy-handed. As Shahan-Whalen put it, in a different context, “They [Elliott and Valenza] repeatedly mischaracterize Shahan’s and Whalen’s positions, to an extent that is hard to imagine is not deliberate (their 2009, 262-63).”

Table 1 counts both the polite words of disparagement, *mischaracterize*, *ignore*, and *bias*, and three of the less polite words, *misrepresent*, *mislead*, and *phony*, the latter combined under “other.”

Table 1. Disparaging words targeted on other side in four recent TOX Articles

Articles	Words	<i>Mischaracterize</i>	<i>ignore</i>	<i>bias</i>	other	Total	Rate/10k
<i>Apples & Oranges</i>	7032	4	8	12	0	24	34
<i>Auditing</i>	14017	18	25	17	8	68	49
<i>Ox by Numbers</i>	23128	0	0	0	0	0	0
<i>Other Car</i>	6409	0	0	0	0	0	0

Table 1. Shahan-Whalen (top pair) offer many more targeted disparaging words than Elliott and Valenza (bottom pair), 34 to 49 per thousand, most of them dainty. Elliott and Valenza offer none at all. Shahan-Whalen’s “Auditing the Stylometricians” (2009) has 60 dainty disparagements (43 per ten thousand) and eight not-so-dainty “other” words, 4 *misleads*, one *misrepresent*, and three *phonies*. We haven’t included “garbage” in this count (their 2009, 260), though it does not seem very dainty to us, nor have we yet used the term “mudslinging” to describe their disparagements. It’s not very polite, but we’ve seen worse. None of these not-so-dainty disparagements are found in Shahan-Whalen’s earlier “Apples and Oranges,” 2006. Note that the version of

“Apples and Oranges” we counted is a next-to-final digital draft from John Shahan which differs slightly from the published one. It’s close enough for our purposes.

If this is a valid comparison, we would draw three conclusions from it. The first is that Shahan-Whalen are much more fond of these targeted disparagements than we are. They have 92, we have none. By this test, we would not be considered likely True Authors of Shahan-Whalen’s work, unless, of course, our samples (or theirs) are unrepresentative of our normal work. Our second conclusion is that their “Auditing” has noticeably higher disparagement rates than their “Apples,” 43 per ten thousand, rather than 34, more non-dainty put-downs, eight, rather than none, and more total put-down words, 49 per ten thousand, rather than 34. Has another hand been added, or is it just Shahan-Whalen in a heightened state of dudgeon? It doesn’t much matter. All of these totals are consistent with our first impression that Shahan-Whalen had not-so-subtly turned up the heat to bridge-burning level, hoping to force us out.

We were ready to go. We try to avoid street fights, and this one, unlike the Shakespeare Wars, was one we could and can skip without sealing our work off from the broader community of Shakespeare scholars. But John Shahan e-mailed us and turned out to be eager for a response, and so was TOX’s new editor, Michael Egan, so we have stayed, for now. Here’s what we wrote to John:

[A]s I told you earlier, the case for an early response in TOX, on our part, was not very compelling. It seemed to call for more fruitless wrangling over whether and what we have mischaracterized, misrepresented, etc.; more repetitive and inconclusive wrangling over how much weight should be attached to Meres, Puttenham, etc.; a new round of wrangling over the unedifying controversy with Michael Egan over *Woodstock*, this time with him as both party and judge; and dealing charitably with your embarrassing new Oxford Apocrypha. And there was all that smell of burnt bridges, all that accusatory language, which looked very much like a signal that you, Dick, and Michael wanted to hear no more of us. None of these seemed to me something we would or should want to rush into.

Perhaps I was mistaken on the last point. Since then, both you and Michael have expressly invited a response, and I told him I would see if I couldn’t come up with something ... If I can, I’ll start with another look at your article. Maybe there’s more to it than met my eye the first time. But for now I would not expect to spend much time discussing our supposed mischaracterizations, or on a long rehash of whether “eliminated” is the right word for Oxford’s claim, or even on a longer discussion than last time on what is to be inferred from Meres and Puttenham. Instead, I suppose I would summarize the controversy as I see it and try to address whatever new substantive points you and Dick raised in your Auditing article. The biggest of these is your proposed new Oxford Apocrypha.

And so we shall: summarize the controversy, state where we think we are now, shorten or skip the squabbling over our supposed misrepresentations and the e-word, let a lot of Shahan-Whalen’s accusations pass without comment, and certainly without endorsement, and deal briefly with just a few of the issues we consider important.

Negative Evidence

The first of these is negative “silver-bullet” evidence. We do pay much more attention to it than positive, and we should. It’s not hard to pile up scores or hundreds of resemblances between Oxford and Shakespeare, and it looks impressive till you find it’s also easy to do the same for Marlowe, Bacon, Mary Sidney, and many others. They can’t all be Shakespeare. Nobody we know of has found a single perfect Shakespeare-identifier test like fingerprints or DNA with no false negatives or positives. We use something more like the Bertillon system with multiple profiles and tests. No matter how many resemblances you turn up in a comparison, height, weight, hair,

skin, or eye color, hat size, shoe size, blood type, all it takes is one or two good mismatches, let's say eye color or blood type, to show it's not Cinderella. Some features, such as height, weight, and foot size, do change, but in predictable, measurable ways. Tiny feet grow, but always larger, not smaller, and they eventually stop growing. Broadly speaking, Shakespeare's open lines and midline speech endings, by conventional dating, went nothing but up. By some speculative Oxfordian datings they go every which way, and it's not a good sign that the Oxfordians have gotten it right.

Shahan-Whalen, who seemed in earlier discussion to concede that our tests were valid where the samples were properly matched, now may be backing away.

Elliott and Valenza ignore the many similarities between Oxford's writing style and Shakespeare's. They focus instead on so-called 'silver bullet' differences (said to disprove common authorship), as opposed to 'smoking gun' similarities (supporting common authorship). This greatly exacerbates the problem of non-comparable inputs, biasing the outcome toward rejection of Oxford. Of course there will be stylistic differences between works written decades apart, but why so many similarities? (their 2009, 236)

Shakespeare called this kind of reasoning "Salmons in both," lampooning Fluellen's attempt to equate Henry V with Alexander the Great by the fact that they both came from countries with rivers, and both the rivers had salmon. Shakespeare and Oxford had many common features—male, English, 16th-century, wrote poems and plays with passion, and had two eyes, and, probably, two ears, ten fingers and ten toes. They innovated and were praised by contemporaries, and they seem to have cared about their good name. As Shahan-Whalen put it, "They [Elliott and Valenza] find it unremarkable that the young Oxford wrote passionately about an issue that later concerned Shakespeare" (their 2009, 246). It's only remarkable if you think that proves they were one and the same, even though their styles and the documents say it's wildly improbable. We don't.

Oxford's Juvenilia

The second issue is juvenilia. We thought it was a stock Oxfordian argument that lines like "Help Fish, Help Fowl," though they may not sound much like mature Shakespeare, can be explained away by Oxford's presumed youth. Shahan-Whalen seem to think we have excluded this possibility, along with the possibility that Oxford may have written some of his poems very young. They are wrong on both scores

They quote us correctly as follows: "The idea that juvenile work must be sour and clumsy—or, more precisely, the idea that sour, clumsy work must therefore be juvenile—is an old Oxfordian standby, much urged in all the Ogburns' books (Ogburns, 1952, Ch. 6; Ogburn, 1984, 390-97), and in Joseph Sobran's book (1997, Appendix 2), no less than in Shahan-Whalen's article' (quoted, their 2009, 254, from our 2007, 146).

They claim this: "We find no such statement in the works of the Ogburns, or Sobran, nor have we made any such statement ourselves. We have raised this issue before, and we hereby challenge Elliott and Valenza to back it up with the specific quotes" (their 2009, 254).

Here are a few. Shahan-Whalen said this: "Why should we expect a thirteen-year-old to write like a man in his fifties, or to write in a style that would not be in fashion for another thirty years?" (their 2006, 119).

The senior Ogburn, less inclined than the others to concede Oxford his immaturity, and more inclined to date most of his poems in his 20s to tie them to his supposed romance with Queen

Elizabeth (their 1952, 50, 59-60) said this of “Help Fish, Help Fowl”: “This early poem, however, gave small hint, unless through its fervor, of the power to come” (their 1952, 12).

Charlton Ogburn, Jr. said this:

[I]t would be surprising if a sonnet a man wrote in his twenties were not markedly inferior to those he wrote in his forties... What the professors are saying is that de Vere had not, in the poems accepted as his, probably all written by the time he was twenty-six, shown the capacity to write Shakespeare's works. That is a defensible position to take. But it is not one allowed Stratfordians, whose own “Shakespeare” had not by age twenty-six shown the capacity to write verse of any kind.... Had de Vere, after writing poetry and prose of such skill and evincing such an attachment to literature as he had by twenty-six suddenly ceased writing anything at all except letters and written nothing but letters during the latter half of his life, he would be incomprehensible and, as far as I know, unique (1984, 390-91).

Joseph Sobran said this:

[S]ome critics rank them [Oxford's poems] as brilliant and accomplished, but C.S. Lewis comments Edward de Vere, Earl of Oxford, shows, here and there, a faint talent, but is for the most part undistinguished and verbose.' These are, after all, youthful poems. One of them was published in 1573, when Oxford was twenty-three; in another, Oxford refers to himself as a 'young man.' Nobody knows exactly when any of the others were written; Professor Steven May puts the latest possible date for any of them at 1593, and they were probably written long before that. Some suspect that most of them were written before 1573. Few would call them works of genius. How, then, can they be Shakespeare's? Perhaps because they are early poems (Sobran, 1997, 231).

So much for Shahan-Whalen's claim to find no such statement as ours that Oxfordians like to blame the shortcomings of Oxford's surviving poems on his youth. They do. We think it's their strongest defense against evidence like ours, though still not very convincing. It's a mystery to us why they are so insistent on disavowing it and reproving us for referring to it.

Obstinate Refusal

As for our supposed obstinate refusal to acknowledge that some of Oxford's poems could have been written young, they claim this: “Elliott and Valenza incorrectly assume that all of Oxford's known verse was written between the ages of 22 and 44, i.e., between 1572 and 1594” (2004, 323, 394) (Shahan-Whalen, 2009, 239). Wrong. No such language appears on either page cited, but we have many times said something like this: “By some estimates (other than Steven May's), Oxford could have been as young as fifteen when he wrote the eight poems eventually published in the 1576 work, *The Paradyse of Daynty Deuses*. Any or all of them could be song lyrics, not poems proper, and, hence, not suitable for comparison with poems (our 2004, 392).”

This is one of many instances where, despite their litany of complaints that we have put words in their mouth (see their 2009, 253-54, *passim*) that they have not hesitated in the least to put words in ours. Our actual position on juvenilia and songs is not all that far from their latest: “The evidence says he wrote half by age 16, and the rest probably in his 20s (their 2009, 236).” Ours, in essence, is this: there is evidence that some of the poems could be songs and juvenilia, but it's only some, and not everybody takes it literally or exclusively (May, 2004, 231-32; Ogburns, 1952, 50, 59-60). The difference is not so much evidence as inference, plus, in their case, an unwavering insistence on sticking us with the burden of proof (their 2009, 237, 238, 245, 251, and 254), preferably “beyond a reasonable doubt (their 2009, 254),” and then berating us for not accepting it (their 2009, 237-38). But you would never guess it from “Auditing the Stylometri-

cians.”

Until now, Shahan-Whalen have shrewdly kept silence on other Oxfordians’ penchant for ascribing, not just Shakespeare’s poems and plays, but almost everything else they could find, to their polymath, Oxford. Oxford is billed as the True Lyly, Gascoigne, Kyd, Green, Marlowe, etc. In their latest, they protest that their concern is to refute us and make us withdraw our use of the e-word (their 2009, 247), not to make an affirmative case for Oxford. “We make no such ‘leap of faith,’” they say, “nor do we offer a positive argument for Oxford as Shakespeare in our critique. Our position is that, [Elliott and Valenza] should have concluded that although they found little stylometric support for Oxford, he was not a fully testable claimant, and so he could not be eliminated” ([Shahan,] 2006, 124). (their 2009, 254). It might sound a bit odd, having denounced us for putting so much stock in negative evidence against Oxford, to try to do nothing with their own case but negate ours. But, just like their attempts to shift the burden of proof to us and make sure it’s heavy, it’s a standard, lawyerly trick—if they can get away with it—to try to keep their own side’s weaknesses off the agenda and the other side’s on.

A Leap to Remember

Can they get away with it? Maybe, if it’s just a matter of rallying the faithful in the Oxford camp while deriding us as “totally subjective and unscientific” (their 2009, 262). But we doubt that they can do it and still convince the outside world without a credible case of their own. The Oxford camp has a variety of Oxfordian views of varying merit, not all of them consistent with one another. Discrediting or disproving our case without getting their own sorted out in a persuasive way is not enough to carry the day. When we noted the creativity/productivity gap between Oxford and Shakespeare (our 2007, 147-48), we created another problem for the Oxford ascription.

To believe it, in the backdated Shahan-Whalen scenario, you had to believe that Oxford wrote 40 lines a year of less-than-Shakespearean verse for ten years, then nothing at all for 15 years,

How could you rework the story and make it more credible? We can think of five strategies, none of them very promising.

then made the Great Leap to 3,500 glorious, Shakespearean lines a year for 20 years—or should it be 7,000 glorious lines a year for ten years on the Oxfordian calendar because the records say he died nine years earlier than Shakespeare’s last plays, and therefore six to eight late plays have to be reassigned to time slots already filled with early plays? Or should it be more like 14,000 lines, at least half of them glorious, if you accepted Shahan-Whalen’s proposed addition of eight Oxford-Apocrypha plays to the early Canon?

These, combined, would amount to a 350-fold increase from Oxford’s baseline production rates, such that what took him a year to write in 1576 would have taken only a day in 1592. Had it actually happened, that would have been a leap to remember. Did anyone notice? None of these alternatives seem likely to us. Perhaps we dimly recalled Charlton Ogburn, Jr.’s quote, cited above in a different context: “Had de Vere, after writing poetry and prose of such skill and evincing such an attachment to literature as he had by twenty-six suddenly ceased writing anything at all except letters and written nothing but letters . . . [for many years after], he would be incomprehensible and, as far as I know, unique (1984, 390-91).”

How could you rework the story and make it more credible? We can think of five strategies, none of them very promising. The first would be a more conventional, less relentlessly backdated chronology of Oxford’s poems than Shahan-Whalen’s. It might at least fill in the fifteen-year silence, though still at no more than half of one percent of Shakespeare’s productivity level. It

would create havoc with Shahan-Whalen's juvenilia speculations, and it would compound, not solve, the mystery of Oxford's sudden Great Leap from 40 lines a year to 3,500. By restoring Oxford's subtracted years without increasing his output, it would cut his documented productivity from 40 lines a year for ten years to 16 lines a year for 25. How to explain the abrupt, now-200-fold creativity jump when he turned into Shakespeare in the 1590s?

Backdating Shakespeare

A second, superficially more attractive strategy is the wholesale backdating of Shakespeare's plays from orthodox dates. Oxfordians have tried to do it for decades, and it is what Shahan-Whalen rely on, but without specifying how they would do it. "Our position," they say, "has been that Oxford wrote the works in the Shakespeare canon, starting earlier than in the orthodox dating" (their 2009, 248). But there are problems. The old Oxfordian chronology of Eva Turner Clark and the senior Ogburns, was pulled from a hat and turned all of Shakespeare's stylometric trends, clear in orthodox dating, into a discordant muddle (our 2000, 81-87 and our 2004, 383-88). Shahan himself, in 2001, was ready to consign it to the dustbin (his 2001). There is still no alternative, worked-out, agreed-on Oxfordian chronology to replace it. The closest thing to it is that of W. Ron Hess's "Shakespeare's Dates," (1999), where the author attempted to backdate the plays in a way that would preserve the trends.

But Hess's dates were far from convincing, and we have seen no sign that other Oxfordians have adopted them. They still had the front-end/back-end stylometric problems mentioned above, and their documentation, where provided, was thin and speculative. For half the plays, no docu-

But Hess's dates were far from convincing, and we have seen no sign that other Oxfordians have adopted them.

mentation was provided at all, and Hess's backdating was performed by "simply subtract[ing] twelve years from the Elliott/Riverside date for that play" (Hess, 1999, 34). As

he admits, though not in quite those words, he pulled them out of a hat. We commended him for gathering all the old Oxfordian dates together and for gamely trying to reshuffle our *Riverside* dates as best he could to cram them into Oxford's life (our 2000, 2004), but we thought his redating needed a lot of further work to make it credible. We can see why Shahan-Whalen don't cite him, or anyone else, to support their claim that Shakespeare's plays were written earlier than what orthodox pros agree on. That work has not been done. Credible supporting evidence has not been produced. Till it is, their hope to fill the gap with credibly backdated plays from the Canon is little more than wishful thinking.

What if it weren't? What if they found enough new factual evidence to justify moving the entire Canon back from, say, 1591-1613, to, say, 1579-1604, prime years for Oxford among the faithful, with Shakespeare trendlines sufficiently preserved to keep Oxford's zero feminine endings from becoming a glaring mismatch with the backdated Canon? It would cut the silence gap to only three or four years, but still leave the productivity gap at 40 lines a year for Oxford/Oxford and 3,500 for Oxford/Shakespeare. Unfortunately, this would not solve the Great Leap problem without yet further manipulation. The miraculous 80-fold Supernova explosion from O/O to O/S would still have taken place, only in 1579, not 1591, yet, again, with no surviving sign that anyone noticed. Who would believe that? Oxfordians are no strangers to conjecture and use it like a ton of plaster to piece together the few factual bones of their case (our 2007, 149), but, in this case they need not just to come up with the missing bones, but with the missing plaster as well.

Maybe there is some way of doing it that can rescue the canon-backdating, but, again, it hasn't been done. Till it is, and till some supporting documentary evidence is produced, the pieces needed to provide credibility have neither been provided nor made to fit plausibly together by Oxfordians, who have had 90 years to think about it. Filling the gap by credibly backdating the Canon is still wishful thinking.

The Oxford Apocrypha

The third strategy is what we have called the Oxford Apocrypha, reassigning anonymous plays of the Shakespeare Apocrypha and poems and plays now ascribed to others, Gascoigne, Kyd, Greene, Marlowe, and so on to Oxford. It, too, has credibility problems. As with Shakespeare, there are no documents directly connecting Oxford to any of these. The flimsy supporting evidence is sometimes intuitive, using similarities to Oxford's—or Shakespeare's—work to show “unmistakable” evidence of authorship (for example, Ogburns, 1952, 52 (Oxford-Kyd), Ogburn, 1984, 628-29 (Shakespeare-Lyly), sometimes stylometric, again, as far as we know, relying on similarities (Brame and Popova, 2002). Some look to relationships to show Oxford's authorship. If Oxford was this or that other writer's employer, patron, student, or collaborator, then he must also have been the True Author of at least some of the other person's work, and not the reverse (Ogburn, 1984, 442-46 (Golding), 514-18 (Gascoigne), 628-29 (Lyly). Such evidence may once have looked impressive in quantity, but no longer in quality. It's too speculative, too reliant on intuition, inference, and non-unique “salmons in both” similarities to pass muster today. We would be more impressed with modern, silver-bullet methodologies with validated accuracy for exclusions, and it doesn't have to be ours.

Low Odds

Neither we, nor anyone else we know, has used such methods on these works to test the Oxford Apocrypha for common authorship with Oxford. We're not about to; Oxford's odds of common authorship with Shakespeare are far too low to justify our effort. But others could, with our software, if they wished, and if they were willing to relinquish their seemingly ironclad principle that the burden of proof must always fall on us. Darwin and Newton would not have gotten far with such a principle. Of course, just as with the Canon, most of the Oxford Apocrypha, to be credible as *Oxford* ascriptions, would have to be backdated to close or narrow the productivity gap, and somehow reconciled to the Great Leap problem. To our knowledge, none of this has been done. To the extent that it hasn't, the supposed Oxford Apocrypha, like the backdated Canon, is nothing more than speculation and wish fulfillment.

Much more important to us than whether these works are credible *Oxford* ascriptions is whether they are credible *Shakespeare* ascriptions. That question we have tested with most of the Oxford Apocrypha, and our unequivocal answer is no, not by a long shot. Every one we have tested is a gross Shakespeare mismatch which can't be explained away as juvenilia or songs. The more of these you claim as Oxford's the more wildly improbable he becomes as Shakespeare, and the more tightly you have to cross your fingers, close your eyes, and blush when you try to describe Oxford as untestable or untested, as Shahan-Whalen do, and therefore not subject to elimination by our methods. We have commented on this problem in previous papers and counseled Shahan-Whalen to put daylight between themselves and the Oxford augmenters. But, as we read them, they could no longer resist the temptation to fill the fatal gap and adorn the Oxford claim with an impossible burden of stylometric discrepancy around its neck. Here's what they say:

Several apocryphal plays probably belong in the canon, in addition to *The Tragedy of Richard II, Part One*. These include *Edward III*, increasingly accepted even by orthodox scholars, plus the five anonymous dramas attributed to Shakespeare by Ramon Jiménez: *The Famous Victories of Henry the Fifth*; *The True Tragedy of Richard the Third*; *The Troublesome Raigne of John, King of England*; *The True Chronicle History of King Leir*, and *The Taming of a Shrew*.

All seven of these plays appear to fall at or near the beginning of Shakespeare’s career. To the extent that the canon is pushed back in time, the case for Shakspere fades, while that for Oxford strengthens. It’s highly unlikely that Shakspere could have written so many plays so early in his career—eight by 1594 just according to the orthodox dates that Elliott and Valenza used (2001, 93), plus these additional seven. That’s a total of fifteen by age thirty. Oxford, on the other hand, was forty-four in 1594. That’s a much more realistic pace, even for a very great playwright [*sic!*] (their 2009, 249).

Table 2
Shahan-Whalen’s Oxford Apocrypha by Date and Shakespeare Discrepancy

Play	Short Title	Latest Supposed Date	Total Rejections	Discrete Composite Probability	Continuous Composite Probability
Shakespeare Core Profile				2.32E-01	3.69E-01
Lower			0		
Upper			3		
<i>Famous Victories of Henry V</i>	FVH5	1588	17	<1.000E-15	<1.0000E-15
<i>Taming of a Shrew</i>	TOAS	1589	14	8.438E-15	<1.0000E-15
<i>Leir</i>	LEIR	1594	8	3.252E-06	<1.0000E-15
<i>Richard III</i>	RCD3	1594	14	8.438E-15	<1.0000E-15
<i>Edward III</i>	EDW3	1595	14	4.355E-12	2.639E-12
<i>King John, Part 1</i>	KJN1	1595	14	2.092E-13	2.063E-11
<i>King John, Part 2</i>	KJN2	1595	16	<1.000E-15	1.504E-09
<i>Woodstock</i>	WOOD	1603	23	<1.000E-15	<1.0000E-15

Table 2 lists eight plays of the Shahan-Whalen *Oxford Apocrypha* by latest supposed date and three measures of composite Shakespeare probability, measured against 48 Shakespeare profiles. No core baseline Shakespeare play had more than two rejections in 48 tests. No “Oxford Apocrypha” play had fewer than eight rejections. All but one had 14 or more. All plays listed showed Shakespeare odds with many zeroes after the decimal point, lower odds than those of getting hit by lightning. Almost half had Shakespeare odds too low to compute, less than 1×10^{-15} . All the plays combined would have Shakespeare odds too low to compute. *Thomas of Woodstock* is another, more orthodox name for Shahan-Whalen’s *The Tragedy of Richard II, Part One*, and 1603 is the orthodox date. We would guess that Shahan-Whalen, having adopted Michael Egan’s name and ascription for *Woodstock*, would also adopt his date, of around 1595, which was once the or-

thodox one. See our 2004, 344-60 for a description and evaluation of our measures of composite Shakespeare odds.

To us, the most obvious thing about Table 2 is the vanishingly low authorship odds it accords to each of the plays of the Shahan-Whalen Oxford Apocrypha, even *Edward III*, part of which we believe could have been by Shakespeare. Our composite odds of its having been written by Shakespeare alone are vanishingly low, so low that you have to use scientific notation to keep them from running off the page. The odds that all eight plays (counting the two parts of *The Troublesome Reign of King John* separately) could be by Shakespeare are too low to compute with a standard, double-precision computer. On the numbers, anyone who tries to present them all as Shakespeare's is going out on a long, long limb. Putting them into Oxford's basket would drive his claim even deeper into the elimination zone than it already is.

The second interesting thing about Table 2 is its dates. They leave in place twelve of the embarrassing and unexplained fifteen years of silence between Shahan-Whalen's supposed last Oxford poem, around 1576, as we read them, and the first play of their Oxford Apocrypha, 1588. They also compound the Great Leap problem, both by cutting Oxford's nominal output to zero during the Silence, and by doubling Shakespeare's by adding eight more fancied "Shakespeare" plays between 1588 and 1595 to the eight or nine already there. Before these were added, Shahan-Whalen had to explain a sudden eightyfold Great Leap in productivity with an unexplained fifteen years of no output at all between the one and the eighty. Afterward, it became one-to-160, also with twelve years of presumed silence in between. And Oxford was 38 in 1588, old enough, as Shahan-Whalen note, to write lots of plays—if he suddenly became as fast a writer as their souped-up Shakespeare with their eight extra plays added to his Canon. All he had to do in this scenario was to increase his baseline productivity by a factor of 160, and polish off a year's worth of work in two days. Maybe that's not a problem if you are a believer, but it's hard to swallow if you are not. We would suppose that at 38 Oxford was also old enough, and his supposed plays playlike enough, to preclude Shahan-Whalen from arguing that the gross Shakespeare mismatches of the proposed new plays could be dismissed as songs or juvenilia. That, too, would be a problem for nonbelievers. We urged John Shahan to return the plays to the vendor before someone notices and draws the inescapable conclusions, and we urge it still. It's not too late.

The Oxford Ephemera

Neither the problems of the backdated Canon nor those of the Oxford Apocrypha have been solved, nor has the Great Leap in productivity been adequately explored or explained. That leaves a final category, the *Oxford Ephemera*, which we took last time to be the bedrock of Shahan-Whalen's case: the mysterious, wholly lost works which, by a process of elimination, must have been what won Oxford all those encomia from his contemporaries for being the best of the best. For these, not only Oxford's authorship, but the work itself is conjectured, and, hence, at last beyond the range of our testing. Shahan-Whalen have berated us at length for using the r-word, *rumor*, to describe such imagined work. How can we deny the fact that all those old critics thought Oxford was the greatest? We haven't. Or the possibility that our surviving Oxford archive is incomplete? We haven't. The r-word has to do with what, if anything, we can properly infer from these two "facts." As we have seen, they can't really mean that Oxford's gaps are fillable by backdating the Canon, or by adding the Oxford Apocrypha to his ascriptions. Oxfordians have produced no good evidence that either of these is viable. Could they mean that there must be a body of Lost Oxford/Shakespeare works out there, of mature works presumably matching Shakespeare in quality and quantity? If so, and if it's anything like what we have from

the Shakespeare of record, it would have to be dozens of plays' worth of material ranging in quality from, say, the level of *Two Gentlemen of Verona* to that of *Julius Caesar* or *Hamlet*. Could such a corpus have somehow come and gone without anyone noticing it, mentioning it, or getting some of it preserved, not even Oxford himself, who surely had the means to do so, if he cared to? Again, it would be hard for unbelievers to swallow.

Shahan-Whalen haven't said this outright, to be sure. They might well prefer to leave unspecified and unexplored just what else Oxford might have written, and when he might have written it, contenting themselves with berating us for using our terrible e-words and r-words. But let's face it: their Missing Oxford can only be one (or more) of three things—*earlier versions of Shakespeare plays*, for which they have produced no good evidence; *reassigned non-Shakespeare plays*, which we've already tested and which don't match Shakespeare, the Oxford Apocrypha; and *unknown, undatable, untestable, out-of-the-blue plays*, that, if they ever existed, have since disappeared without a trace: what we call the Oxford Ephemera. The Oxford Ephemera could well be the least implausible alternative left, and we looked into it and thought about it as we have all the others. It still seems to us far-fetched, more rumor-like than fact-like, and all too reminiscent of those bumper stickers that said "My other car is a Porsche." We thought it transformed their case for Oxford from one where his poems could be considered an asset to one where they were an embarrassment, yet, they suppose, not one that would eliminate Oxford as a credible claimant because you can still vaguely imagine other works that might be more like Shakespeare. For us, imagining something doesn't make it so. Some imaginings are less plausible than others, and we would like to see a lot more flesh on this one than Shahan-Whalen have provided before abandoning the e-word.

Resurrecting Oxford

Speaking of believers and conjecture, there is one more way Oxfordians might get around some, but not all, of their current problems: resurrect him from his reported death in 1604. It's also contrary to what the surviving documents say, but so what? So is the whole Oxford claim. We have already been asked to imagine that Oxford would stop at nothing to promote the conspiracy, that William Cecil would stop at nothing to purge the records, and that both had somehow mustered the power to blot out every trace of a connection between Oxford and the Canon. The faked-death gambit almost worked for the Marlovians: great writer fears persecution or death from Elizabeth's police state, fakes death, stops writing under own name, writes the Shakespeare Canon from hiding. Why not imagine the same for Oxford? Some Oxfordians had already toyed with the idea to cope with the Funeral Elegy (our 2004, 377), and Stephanie Hopkins Hughes, founding editor of TOX, is seriously considering it today (Hughes, 2010, citing Paul, 2004).

It could solve a number of problems. The long, floundering battle to uproot and backdate Shakespeare's plays could be abandoned, the plays restored to about where most orthodox scholars, available documents, and stylometrics indicate they belong. Many of the indicators of developing style, simultaneous collaboration with Fletcher in 1612-13, adaptation to performance in the Blackfriars, and close, continuous contact with the theater after Oxford's reported death would become less of an embarrassment to explain. And, as we have seen, solving the back-end problems of Shakespeare/Oxford's posthumous plays could help solve many of the front-end problems, and documentation problems of having to subtract twelve years from their conventional dates.

However, they still would not solve the Great Leap problem or the silence-gap problem imposed by Shahan-Whalen's Oxford backdating. Using the most plausible available dating, Shake-

spere still wrote as much in two to five days as Oxford did in a year, and Oxfordians haven't faced up to the task of explaining it or trying to reshuffle the dates to provide some kind of credible transition. On the other hand, at least it doesn't flagrantly compound the problem by trying to cram six to eight of Shakespeare's late plays, plus another eight from Shahan-Whalen's new Oxford Apocrypha into the same already-crowded time slot as his earlier plays, thereby, in effect artificially doubling or tripling his seeming productivity rates. The trouble you don't get into you don't have to get out of. And, of course, they don't solve either the old documents problems discussed earlier or the new ones of explaining away the ample documents confirming Oxford's death, as summarized by Hughes herself:

There's plenty of official evidence of the [1604] date: two *Inquisitions post mortem* (legal examinations of the deceased's property, required where the Crown sees a possible interest), entries of his burial in the Hackney parish records, his name removed from the record of nobles at Parliament that year, a (possible) tomb at Hackney, requests by Elizabeth Trentham that her son, Oxford's heir, be instated in his father's offices, all testifying to his death on that date, or close to it (Hughes, 2010).

Nor do they cancel out the gross stylometric mismatches we found, nor the anomalies of an earl being described as *maister* (see Stylometrics and Documents, above). Again, it's the kind of far-fetched argument which might work with the faithful but seems to us unlikely to persuade the outside world.

Our 'Phony' Bet.

Finally, there is our bet. We offered it originally with no thought of the current controversy to rescue ourselves from one of those endless loops of recreational wrangling beloved by some intellectuals, but not by us. It's too much like listening to a long, recorded phone message. This one was with an insistent Canadian literature department numerophobe and hockey fan who insisted repeatedly and categorically to the Global Shakespeare Discussion Group (Shaksper) that our statistics, indeed all statistics, are circular and tell you nothing that you do not already know. Though we might have found some tests that could separate a few known Shakespeare plays from a few known plays by others, these results tell us precisely nothing about plays we have not tested. We didn't think so and, having offered him a hockey-statistics bet which we would surely have won—that the Kings and the Ducks, who had used it all year, would use the neutral-zone trap in their playoff game that night—were about to bet him another 25 dollars that he couldn't find an untested play by Shakespeare that wouldn't pass our tests, or an untested play by someone else that would pass as a Shakespeare could-be. Then we realized that it would take us several hundred dollars worth of time and effort to prepare and test such a play, and a penny-ante bet might encourage yet further wrangling. We raised the ante to \$1,000 and then to £1,000, to discourage nuisance offers; the wrangler wisely withdrew, and the wrangle was over for the time being.

Some years later, but before he became editor of TOX, Michael Egan was preparing to publish his four-volume study of *Richard II, Part One*, better known as *Woodstock*, announcing it as incontestably Shakespeare's (his 2006). But no one was paying much attention to it. He defiantly and dramatically bearded us on Shaksper, calling us out as "pale, trembling cowards," and demanded that we admit the play was incontestably Shakespeare's and fork over our £1,000 on the spot. We didn't. *Woodstock* was anything but an untested play. We had already tested it and found it a gross mismatch with Shakespeare (Table 2, above). And we don't know many people

who share Mr. Egan's opinion that it is by Shakespeare. Mr. Egan spurned our bet as "phony" and offered us a different one more to his liking, and better suited than ours to selling his then-new book. In many respects it was similar to the ones offered by Shahan-Whalen in their "Auditing the Stylometricians." A panel of distinguished, neutral judges would be recruited to examine his study meticulously (not ours) and deliver a learned judgment. Needless to say, known critics of his position, such as MacDonald Jackson, commonly considered the world's leading authority on *Woodstock's* authorship, would be excluded as biased.

We saw in the Egan counterbet an invitation to an extended wrangle over choosing and compensating the panel, arguing over who was distinguished and who was biased, negotiating the rules of engagement, and debating the proper burden of proof, with no prospect of having our own methods fruitfully discussed. We were not interested. But we did offer to put the matter to a simple vote of Shaksper's membership after an exchange of brief position papers, and no more than a month of discussion by us and Shaksper members. Both sides would be free, if they wished, to post longer statements on their websites. Mr. Egan wanted none of that, having just taken down his own vast webpage, possibly to keep from undercutting the sales of his book(s). He wanted much more protracted consideration of his books than we were interested in, no limit on position papers, plenty of time and occasion to buy his book(s), and discussion to continue for "as long as it takes." He declined both our original bet and our offer for a one-month resolution, but has since insisted that we are the ones who backed off and that he is he is "prepared to meet Elliott's challenge any time, given fair terms such as those proposed above," i.e., his terms for his bet, not ours (Shahan, 2009, 264). We are not tempted.

Nor are we tempted by what we take to be Shahan-Whalen's terms: wrestle up a panel, have a good, long wrangle over the membership, compensation, and rules, find some way of paying for it, make sure that none of the leading orthodox authorities, such as Steven May or Alan Nelson, are on it because they are biased, saddle us with a burden of proof suitable for a murder trial, and call it "fair terms," while continuing to duck our original bet and deride it as phony. No thanks. If any of these bets are phony, it's Shahan-Whalen's and Egan's, where the decision is subjective and the outcome depends mostly on who gets on the panel and who doesn't, not ours, where the tests are highly replicable and the computer doesn't know or care whether or not you're a believer. Ours is remarkable, not just in that no one has taken us up, but in that, if anyone did, it would not be hard to tell who won, who lost, and by how much (our 2004, 360-61). Ours is like weighing something. Shahan-Whalen's and Egan's are like setting up a beauty contest with full knowledge that the choice of judges will determine the outcome, and then wrangling over that till one side stomps out and the other cries "pale, trembling cowards" (see our Shaksper posting, 2005) and claims victory while making certain that the accurate, well-validated scales in plain view are never consulted. You might not guess it from our long debate with Shahan-Whalen, but we do try to avoid interminable, inconclusive wrangles where we can, and this seems to us a particularly good one to stay out of.

Conclusion

We have enjoyed much of our dialogue with Oxfordians, especially at the outset, when they were fresh and confident and quite a bit more polite and conciliatory than they have been of late. They were ready for outreach, and certainly more interested in dialogue and debate than any of the other author-advocacy groups. We have learned from the exchange and know more today about both Shakespeare and Oxford than we did at the start: their stylistic quirks, their meters, their

lives, their documents, the dates of their works, the number, tone, and metrical structure of their songs, their respective output levels. We also learned a lot about our own data analysis and how to calculate composite probability. We see little sign of the two sides having been brought together; if anything they have moved farther apart. But we hope that Shahan-Whalen have learned as much from their side of the controversy as we have from ours.

We do not regard their “Auditing the Stylometricians” as a high point. It’s longer, shriller, more vituperative, more repetitive, and more insistently and consistently off-target than their prior efforts, and it got more and more so as they warmed to their task. By the end it sounded like a clear signal that they wanted us to end the discussion and go away. But we have heeded, for now, Shahan’s and Egan’s insistence that it was not so intended and that they wanted a response. We’ve tried to summarize the discussion and highlight the points of disagreement on both sides, and to respond to at least a few of their arguments and challenges.

Time will tell whether this debate is worth continuing. To make it happen, both sides have to find it worth their time. The Shakespeare Oxford Society makes much these days of its journal being open to opposing viewpoints. From our perspective that has been true for most of this decade, though not the last decade, and, in this decade, it has been less true lately than earlier. Even when most officially closed, it has always had individual members who didn’t get the word, or, getting it, did not rush to slam the door and pull up the welcome mat. TOX readers, we won’t rat on you, but most of you know who you are. Thanks for everything. Yet other individuals have had their open moments and their closed moments. The open ones are the ones that matter. We have many, many times been helped and encouraged by both kinds of Oxfordians and think it has been a plus for both sides. Our thanks to both categories. We consider them the ones most faithful to SOS’s stated mission, of “exploring the Shakespeare authorship question and researching the evidence that Edward de Vere, the 17th Earl of Oxford (1550 – 1604) is the true author of the poems and plays of “William Shakespeare.”

Some such evidence may one day say he is. Ours says it’s very unlikely: too much discrepancy between both parties’ surviving poems, plays, and songs; too few supporting documents for Oxford; too many for Shakespeare; too many Oxfordian dates pulled from a hat; too many supposed Oxford Apocrypha plays which doesn’t match Shakespeare either; too big and too exalted an imagined Oxford/Shakespeare Ephemera to have gotten lost with no trace; and too big a Great Leap from Oxford’s low productivity levels to Shakespeare’s high ones, no matter how you slice the dates. All of these seem to us serious problems for the Oxfordian claim. If you care about Shakespeare authorship, and want the rest of the world to suppose that he was Oxford, it’s not such a bad thing to know about them and try to deal with them with something better than name-calling. One way to know about them is to keep TOX open in fact, as well as in name, to the few contrarians willing and able to consider them.

For all its flaws, TOX has been more open to Stratfordians than most orthodox journals have been to anti-Stratfordians, and we hope it stays that way. If it really wants to hear from the other side, it’s a good idea to put out the welcome mat with the right hand. It’s also a good idea not to snatch it away with the left. Easing up on the confrontations and put-downs would be a welcome step in that direction.

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