

# VI

## Shakespeare

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This chapter has four sections: 1. Editions and Textual Matters; 2. Shakespeare in the Theatre; 3. Shakespeare on Screen; 4. Criticism. Section 1 is by Gabriel Egan; section 2 is by Peter J. Smith; section 3 is by Elinor Parsons; section 4(a) is by Chris Butler; section 4(b) is by Chloe Porter; section 4(c) is by Daniel Cadman; section 4(d) is by Richard Wood; section 4(e) is by Steve Longstaffe; section 4(f) is by Kate Wilkinson; section 4(g) is by Naomi McAreavey.

### 1. Editions and Textual Matters

One major critical edition of Shakespeare appeared in 2009: James R. Siemon edited *Richard III* for the third series of the Arden Shakespeare. An abortive Arden edition of *Romeo and Juliet* also appeared in the form of an appendix to a monograph, but it was so poorly executed that it needs little notice. There were also four major monographs directly on our topic and a further two with important contributions, the usual number of essays in book-format collections and more than the usual number of relevant journal articles.

Siemon's 123-page introduction has nothing to say on the complex textual situation of *Richard III*, because an extended appendix deals with the matter. At times Siemon's tone is rather too colloquial—'cue victim number one' (p. 6), 'goofy' (p. 7), and 'Sound familiar?' (p. 8)—and is unhelpfully aimed at readers who already know the play. He makes the valuable point that Richard's character, and his stichomythic wooing, are somewhat dependent upon the character of Dissimulation in Robert Wilson's play *Three Ladies of London*, before exploring more familiar analogues in Thomas Kyd's *The Spanish Tragedy* and Christopher Marlowe's *The Jew of Malta*. Siemon points out that the character of Richard in this play is rather different from the one seen in the preceding history plays, where he was vengeful but not a loner, not

anti-family. Not until his soliloquy in Act III of *3 Henry VI* does he become the 'theatrical, scheming, wicked, ironic' (p. 40) figure we see in *Richard III*.

Composition of the play could not have preceded the publication in 1587 of the second edition of Raphael Holinshed's *Chronicles*, a major source, and must have been complete before the play's entry in the Stationers' Register on 20 October 1597. Siemon considers it highly likely that *Richard III* postdates *3 Henry VI*, on which it builds, so that makes it after the spring of 1592 once allusions by Robert Greene and Thomas Nashe are factored in, and Siemon goes for the summer of 1592. He adopts John Jowett's nomenclature for the lost manuscripts underlying early editions, but calls them QMS and FMS rather than Jowett's MSQ and MSF. If *Richard III* was in performance before the closure in June 1592—which lasted until December 1593 except for five weeks in December 1592 and January 1593—then it ought to be mentioned in Philip Henslowe's records, but it is not, and we would expect Nashe to mention it when praising the depiction of Talbot in *1 Henry VI* and when lauding Lord Strange (whose ancestors the play depicts positively), and he does not. So, *Richard III* was probably first performed by the new Chamberlain's men company when they began in June 1594 at the Theatre, with Richard Burbage in the lead. Siemon offers a useful summary of the sources (pp. 51–67), indicating the crucial importance of Thomas More's biography as well as how Holinshed, Edward Hall, and *The Mirror for Magistrates* tell the play's stories.

A digression (pp. 69–74) on a family-tree pageant for Elizabeth I on the way to her coronation in 1559 contains an odd mix of colloquialism ('Sound familiar?' again) and obscure words such as *scapular* (twice) for 'pertaining to shoulders' and *nuntius* for 'messenger'. A play on the same topic, Thomas Legge's *Richardus Tertius*, introduced the wooing scenes absent in other sources, and as Siemon notes, the anonymous Queen's men's play *The True Tragedy of Richard III* contains the line 'A horse, a horse, a fresh horse'. Siemon's introduction ends, conventionally, with a brief stage history (pp. 79–123), which contains the familiar story common to Shakespeare's plays of an adaptation (here, Colly Cibber's) holding the stage from 1700 to the early nineteenth century. Quoting Henry James's account of Henry Irving as Richard from the essential compilation *Eyewitnesses to Shakespeare*, Siemon fails to give its author Gāmini Salgādo the accents in his name (p. 101, n.1). This section (and hence the introduction) ends abruptly with a description of Jonathan Slinger's remarkable performance as Richard for the Royal Shakespeare Company in 2007–8.

Before turning to the text, it will be useful to survey Siemon's Appendix I on the early editions (pp. 417–60). He gives the hypothetical stemma, and necessarily it is complicated; in compensation Siemon quotes Barbara Mowat and Paul Werstine's excellent summation that 'the first printed version, almost all scholars agree, provides a second state of the play, and later printings of this second state, in turn, influenced the printing of the play in the first state' (p. 418). For III.i.1–III.i.166 and V.iii.49–end, about one-sixth of the play, the Folio simply reprints Q3 (a reprint of Q2, which reprinted Q1) and hence Q1 of 1597 is Siemon's copy text for these parts. F is his copy text for the rest of the play. Siemon charts the general twentieth-century preference for F, with

Q1 rising in editorial popularity towards the end of the century, culminating in Jowett's edition for the Oxford Shakespeare, which preferred Q1 as more theatricalized. (This edition was reviewed in *YWES* 81[2002], covering work published in 2000.) Siemon makes the case that the inextricable linking of Q1 and F means that an editor has to use both—that is, conflate them—but his policy is to prefer F overall and bring in from Q1 what he needs, using the symbols  $Q \dots Q$  to mark it off, except where F merely reprints Q3, for which parts Q1 is basic, and  $F \dots F$  is used to mark what is taken from there. These symbols have not been seen in the Arden3 series since R.A. Foakes introduced them for his *King Lear*. Because of F's dependence on Q6, Siemon has to collate all six pre-Folio quartos.

In a subsection on Q1 (pp. 422–31), Siemon repeats Peter W.M. Blayney's claim that plays were not particularly attractive to publishers, without acknowledging Alan B. Farmer and Zachary Lesser's counter-claim (reviewed in *YWES* 86[2007], covering work published in 2005), which did not appear too late to be noticed. At 3,480 lines, Q1 is much longer than other plays and Siemon awkwardly describes how the play must have been cut for performance: 'Sometime between some version of what Shakespeare wrote and what found its way into print as Q1, someone—perhaps Shakespeare, the acting company, a theatrical scribe, the printers or their employees—shortened the text' (pp. 424–5). Here there is slippage between the terms 'time' and 'text': Siemon ought to have written 'Some time between the writing of some version ... and the writing of what found its way ...'. The words 'Some time' are required because 'Sometime' means formerly or occasionally.

Valentine Simmes printed sheets A–G and Peter Short printed sheets H–M of Q1, both for Andrew Wise. The copy was cast off and, to judge from the results, fairly accurately, although there is in places severe cramping that may well have necessitated shortening of stage directions in a way that altered the action. Siemon traces the dispute between MacDonald P. Jackson and Susan Zimmerman about the number of compositors who set Short's sheets H–M, but he omits the final blow in the exchange, an article by Jackson published in 2001. (This reviewer missed it at the time too, because it appeared in the fairly obscure *Bibliographical Society of Australia and New Zealand Bulletin*; it ought to have been reviewed in *YWES* 82[2003].) Siemon also uses only two of Alan E. Craven's four relevant articles on Simmes's compositor A, whose habits Craven rather over-confidently detected across a number of jobs over the years. The over-confidence resided in Craven's assumptions that one man's habits would be relatively stable over time, that compositors did not share typesets, and that from the variants in an edition the extent of its proof-reading can be inferred; all three assumptions are unreliable. Siemon mentions challenges to Craven's work on Simmes's compositor A (p. 429, n. 1), without going into the details.

Having acknowledged, albeit under-represented, the extent of, the Jackson/Zimmerman dispute about Short's sheets of Q1, Siemon writes that 'less has been asserted about personnel' (p. 429) in Short's shop than has been asserted about Simmes's compositor A; in fact the debates are about equally extensive. Then comes a howler '... printing by formes requires setting all pages for one side of a sheet and printing them before setting the pages for the other side of

the sheet' (p. 429). In fact, the setting of the second side can be done simultaneously with the setting of the first, or during the printing of the first. Indeed, being able to set both sides simultaneously is one of the reasons for bothering with casting off and setting by formes, as the procedure allows more flexible and rapid reallocation of labour in the printshop.

A single variant introduced during stop-press correction of Q1 is of considerable importance. Where the uncorrected state of L2r reads 'greatest number' the corrected state reads 'vtmost number', and at this point F reads 'vtmost power'. Siemon reckons that F's agreement with Q1c in having *vtmost* (a reading not found in Q2-6) shows an F/Q1 relationship independent of F's derivation from Q2-6, since it is most unlikely that F got this reading, and nothing else, from Q1c. If so, QMS and FMS were closely related. However, as Siemon points out, the next word shows the opposite, since it is hardly likely that Q1's compositor would make the correction of *greatest* > *vtmost* (which is so unmotivated that it must have followed from consultation of copy) without also fixing *number* if his copy showed that *power* was the correct word. Thus it cannot be the case that QMS and FMS agreed on the second word, so we have evidence for and against these manuscripts being closely related. Siemon reckons that Q1/F's exclusive agreements (that is, against Q2-6) show the closeness of QMS/FMS but their differences show that QMS derived from FMS. To prove this, he turns to Q2-8.

Siemon's subsection (pp. 431-41) on these derivative quartos characterizes the Q2-8 line as essentially monogenous, although Q5 drew on Q3 as well as Q4. Q3 supplied one-sixth of the copy for F, and for the other five-sixths Q3 and/or Q6 was printed copy for F that was first marked up by reference to FMS. F frequently agrees with one or more of Q2-6 against Q1, which could be coincidence but could also happen because F got those readings from Q2-6. Thus the need to collate Q1-6 and F. Q2 supplies two lines absent from Q1 and an important question is how they got there. One possibility is that these lines were in a corrected exemplar of Q1, now lost, which was copy for Q2. The alternative, supported by Jowett, is that for these two lines the copy for Q1 was consulted in the making of Q2; Andrew Wise would still possess that copy, being the publisher of both. Siemon finds evidence for this alternative in the crowding of the preceding page of Q2: the compositors would not need to crowd if they were just reprinting an exemplar of Q1 that had these lines, but would need to crowd if they discovered during setting that the Q1 they were reprinting had two lines missing. (Actually, this is not quite true, since the corrected exemplar of Q1 they were reprinting might itself have the crowding, created during press correction when it was realized that two omitted lines had to be squeezed in, and since Q2 is a page-for-page reprint of Q1 it would reproduce this crowding.) As Siemon points out, the signs of crowding (a turn-up and a catch-word sharing a line with dialogue) appear elsewhere in Q1 and Q2 so they do not tell us much. Weighing it all up, Siemon plumps for the idea that Q2 was reprinted for an exemplar of Q1 (now lost) that contains these two lines, added during press correction. Thus nowhere in Q2-6 was QMS consulted: they are pure reprints. Jowett argued that Q3's small improvements over its copy Q2 betray consultation of QMS, but Siemon

thinks that a clever printshop worker could have made them and he itemizes the evidence (pp. 436–7).

Q1 gets right ('ix' months), Q2 gets wrong ('xi' months), and Q3 gets right ('ix' months) the age of Henry VI at his coronation, but this did not require consultation of QMS since the fact was well known. A crucial case is Q3's reordering of the speaking of the ghosts, which Jowett thinks Q1 and Q2 get right—in the sense of showing what got performed, since its order is more efficient in casting—and Q3 gets wrong by putting the ghosts in the order in which they died. Jowett came up with possible explanations for Q3's ordering of the ghosts' appearance, such as unclear transposition marked in QMS, a change in the staging that Wise knew about, or Shakespeare's insistence that the ghosts appear in the book in the order they died rather than in the order they appeared on stage for purely practical reasons; Siemon finds them all unconvincing. He adds his own possibility: whereas Q2 reprints Q1 page for page, Q3 reprints Q2 without preserving its pagination, and Siemon reckons that 'It could not have been easy to mark up so many repaginations' (p. 438). But in fact there would have been no need to mark them up if Q3 was set seriatim, and Siemon offers no evidence that Q3 was instead cast off to be set by formes. Siemon notices that Q3 corrects a speech-prefix error in Q1 and Q2 regarding one of the ghosts: Rivers' ghost's condemnation is attributed to 'King' in Q1 and Q2 (both on L4r) and correctly to 'Riu[ers]' in Q3 (L3r). Thus someone in the printshop making Q3 was paying close attention to Q2, and such a person might easily read Buckingham's 'The last was I that felt thy tyrannie' (Q2, L4r) and decide that all the ghosts' speeches ought to appear in order of death, and altered the text to make them do so. On this supposition, Siemon rejects Q3's ordering of the speeches (which is F's, but then F just reprints Q3 at this point) and goes back to Q1's.

Siemon then turns to the evidence for the other five-sixths of F not printed from Q3 being printed from Q6. There are a dozen F/Q6 agreements against Q1–5. Importantly, where Q1–5 have 'Is colder tidings, yet they must be told', Q6 has 'Is colder news, yet they must be told', and F has 'Is colder Newes, but yet they must be told'. For F to be independent of Q6 requires either that FMS had *news* and that Q6 got its *news* by accidentally picking it up from two lines earlier, or that F and Q6 both independently picked it up from there. However, F has *but* which would not metrically fit with *tidings*, and this *but* is most easily explained as an attempt in the making of F to bring the erroneous reading in Q6 back to good metre. Since F is at this point simply reprinting Q6, Siemon returns to Q1's reading.

Siemon discusses the creation of F by compositors A and B, noting that no press variants implying consultation of copy have been found. Unfortunately, he takes over from Antony Hammond's Arden2 edition of the play the quite meaningless claim that page q6r is 'the last of the inner forme of gathering q' (p. 443, n. 2). Gathering q, made of three sheets, has three inner formes and three outer formes, and Siemon means that pages q6r and q1v make up the last inner forme of gathering q to be set and printed. There are signs of stretching of copy on this page, towards the end of gathering q, and Siemon (following Hammond) sees in this compositor B attempting to meet an agreed end-of-gathering break predetermined by casting off. This makes no sense

unless the next gathering had already been cast off too, and neither Siemon nor Hammond gives a reason for thinking that happened here. The standard work they follow is Charlton Hinman's *The Printing and the Proof-Reading of the First Folio of Shakespeare*, which describes the normal method as setting seriatim in the second half of the quire and letting the next gathering start where it will. (Hinman gives exceptions to this—occasions where multi-gathering casting off was done—but gatherings q and r are not among them.) Werstine has shown that the second half of a gathering might be cast off if its setting was to be shared by compositors, but that is not the case here.

Over 200 lines in F do not come from a quarto, so there was a manuscript involved too. Conversely, Q1 has nearly forty lines not in F, so we need to consider the QMS/FMS relationship. There are Q1/F agreements against Q2–5 that seem to come from QMS/FMS agreement rather than consultation of Q1 when printing F. Siemon does not go into the detail of how we know that these are not cases of Q1 itself influencing F, which is that around these Q1/F agreements against Q2–5 the copy for F is clearly Q3 or Q6, so unless the compositors were flitting furiously between different forms of copy—that is, if they were doing the sensible thing and just alternating between exemplars of Q3 and Q6 marked up from an authoritative manuscript—the Q1/F agreements must come from that manuscript. The line 'Harpe not on that string Madam, that is past' is in Q1, missing in Q2–5 (making nonsense of a dialogue exchange), and present in F, but in the wrong place. The obvious inference is that it was written in the margin of F's quarto copy (when it was noticed that this line in FMS was absent from that quarto copy) with an indication of where it should be inserted, but the indication was badly made or badly followed when setting F. The fact that QMS was derived from FMS (and not the other way around) is shown by Q1's garbling of things correct in F. For example, in F Richard is sarcastically advised to woo princess Elizabeth by sending her, to wipe her eyes, a handkerchief dipped in her brother's blood, whereas in Q1 the advice is to send a handkerchief dipped in Rutland's blood; the latter would mean nothing to her.

Siemon surveys claims that QMS was made by collective, legitimate memorial reconstruction by the company, ending with Jowett's proof (from variation in speech prefixes) that QMS and FMS are related by transcription, not memory. (Jowett's article was reviewed in *YWES* 81[2002], covering work published in 2000.) Siemon accepts that QMS must derive from FMS since Q1 is more theatricalized than F, although he notices that the theatricalization sometimes hurt the meaning of dialogue, as when streamlined casting gives Lovell and Ratcliffe's tasks to Catesby alone, but the references to those actions retain plural pronouns. Likewise, in Q1 Ratcliffe takes over the Folio Sheriff's role as Buckingham's executioner, yet in Q1 Buckingham treats his executioner as someone he does not know rather than as a former ally. In Q1 Catesby takes over from Lovell and Ratcliffe (in F) as Hastings's executioner, yet in Q1 Hastings fails to reproach Catesby, his former friend and confidant. (Hastings has no such close relationship with Ratcliffe and Lovell, so it is plausible that he would not reproach them.) There is a similar wrinkle with the streamlining that makes Dorset, in Q1, rather than a messenger (as in F) bring to his mother in II.iv the news of the imprisonment of members of their

family: she and he express no concern for one another, and she talks as if he was not there, as indeed he was not in F.

How should an editor respond to this textual situation (pp. 456–60)? If we accept FMS > QMS revision, then mostly it was a matter of cutting lines, although Q1 has the jack-of-the-clock episode absent from F. Was it added in? Was it censored when F was printed? Siemon does not know. In some verbal variants Q1 is closer to the sources than F is, but in others it is further. F has certain geographical errors that Q1 fixes, such as getting from Stony Stratford to London via Northampton (II.iv.1–2), but Siemon thinks this a miscorrection in the sense that F reflects the sources. That is, F's route, which indicates turning back and heading away from London, is indeed what the party historically did after Richard intervened at Stony Stratford to arrest Rivers, Grey, and Vaughan. The trouble with this argument is that it leaves the Archbishop trying to reassure the others on stage that the royal party is on its way to London, yet naming a sequence of places that indicates, to the geographically knowledgeable at least, that they are heading in the opposite direction.

Siemon is not convinced that authorial correction explains the difference between F's treatment of Woodville, Rivers, and Scales as three men (apparently arising from ambiguous phrasing in the source Hall) and Q1's historically correct reduction of these names to one man. It could, he thinks, just be theatrical economy. He decides to let the route-to-London geographical error stand (that is, he follows F) since an unauthorized change by a geographically knowledgeable printshop worker might account for Q1's correction. On balance he decides that the reduction of Woodville/Rivers/Scales to one man is not something that could have happened in the printshop, so it was a fix authorized by someone in the know and Siemon's retains this fix (that is, he follows Q1). The famous error of Richard saying that Richmond was raised at 'our mother's cost' (V.iii.324) instead of 'our brother's cost' is not fixed by Siemon because it seems to be what Shakespeare really thought happened, having been misled by a misprint in Holinshed. Overall and in general this edition 'sides with F' (p. 460).

Let us see how these ideas affect the words chosen for the text of the play. Siemon uses one collation band for everything, and where there is a choice of Q1 or F wording, he goes for one or other, usually favouring F. Where one edition has something the other lacks he imports it inside  $Q \dots Q$  and  $F \dots F$  symbols. Modern editions are collated very occasionally, and although he refers to the corrected and uncorrected states of formes of Q1 Siemon does not indicate where the exemplars containing them are located nor give a list of all their press variants. Siemon uses F for 'Plots have I laid, inductions dangerous' (I.i.1.32), rejecting Q1's reading (defended and adopted by Jowett for the Oxford Shakespeare series) of *inductious* as an adjective. F is also followed for Clarence's 'but I protest | As yet I do not. But, as I can learn' (I.i.52–3), although Q1 makes the first *but* into *for* (which makes smoother sense), and one might argue that the F compositor picked up *but* from the next line; Siemon thinks the *but ... but* phrasing might be intended to show Clarence's inarticulate excitement. Siemon has Richard say that the queen 'tempers him [the king] to this extremity' (I.i.65) using *tempers* from Q1 because it fits the

sense of women moulding men, whereas F's *tempts* does not. The limitation of the edition's typographical conventions is clear in the first of Siemon's importations for Q1: 'Heard you not what an humble suppliant | Lord Hastings was <sup>Q</sup>to her<sup>Q</sup> for his delivery?' (I.i.74–5). Aside from *to her* there is another Q1/F variant in this line—*her/his* as the penultimate word—but it is buried in the collation. This mixture of conventions makes it hard for the reader to appreciate that Q1 reads 'Lord Hastings was to her for his deliuey' and F reads 'Lord Hastings was, for her deliuey?' Siemon has Richard swear by 'Saint John' (I.i.138), the Folio reading, rejecting the quartos' variant *Saint Paul* although the sources attest to his use of it and he says it elsewhere in the play.

Siemon offers a textual note on the three-way press variant in Q1 'set downe your honourable l' versus '... honourable lo' versus '... honourable lo:' (I.ii.0), pointing out that the last word might mean *lord* instead of the familiar *load*, but he does not adopt Q1 for his edition, and he does not go into the two-stage correction that this press variant is witness to. Indeed nowhere does he make systematic comments on press variants and their significance for the editor. For the complex set of Q1/F variants in Anne's speech 'Stabbed by the same hand ... cursed ... Cursed ... blood from hence' (I.ii.11–16) Siemon just follows F, noting but not being persuaded by the arguments in favour of emendation, such as the first *hand* being contradictory of 3 *Henry VI* where all three York brothers stabbed her son, nor by the argument for reordering the lines to make better poetic sense. Although following Q1 for 'Unmannered dog, stand thou when I command!' (I.ii.39) where F has '... stand'st thou ...', Siemon tries in a note to make sense of F's reading which 'could be a demand in the form of a question: i.e. are you going to stand still or not when I issue the order?'. There are twelve lines at I.ii.158–69 that appear in F and are absent from Qq, forming a speech about how he, Richard, has forborne weeping until now. Siemon surveys the arguments about these lines—an addition to FMS? a deletion from QMS?—and decides that 'Speed [of theatrical performance] seems the most likely motivation for omission', so clearly his edition is not trying to present the play as performed else he would remove these lines.

Despite the colloquialisms noted above, certain aspects of Siemon's edition are rather old-fashioned. There is a recurrent pattern of cross-referencing to other literary works without comment, as when the note for lines I.ii.177–81, where Richard offers his naked breast to the sword, begins 'Cf. Seneca, *Hercules Oetaeus*, 1000–1, 1015'. It is hard for a reader to know if she should take the trouble to find these lines in Seneca without first being given a hint about why it is worth doing. Likewise for 'Cf. Berowne's surprise and consternation at the absurdity of his falling in love (*Love's Labour's Lost* III.i.169–200)' (I.ii.230–40n.), which seems to betray an educator's concern for comparing the plays more than an editor's concern for explaining this one. Siemon follows F to give '[your hatred] Makes him [the king] to send, that he may learn the ground' (I.iii.68) where Q1 has 'Makes him to send that thereby he may gather | The ground of your ill will and to remoue it'. The thought is rather more completed in Q1, but Siemon is right that F makes sense on its own and needs no improvement.



Siemon combines F and Q1 to produce 'What? Threat you me with telling of the King? | <sup>Q</sup>Tell him and spare not. Look what I have said<sup>Q</sup> | I will avouch't in presence of the King' (I.iii.12-14). The trouble here is that *avouch't*, from F, seems necessary because F lacks the Q-only words. That is, the *it* of *avouch't* refers back to an earlier speech Richard made, while in Q1 the corresponding word is *avouch* because the antecedent 'what I have said' is present in the sentence. To conflate Q/F here is to change the meaning of 'Look what I have said', which appears only in Q1, where it is the subject of *avouch*. The conflation makes it into a separate thought roughly equivalent to 'think on what I'm saying', and that is rewriting Shakespeare. At I.iii.322 Siemon gives '*Exeunt all but* <sup>Q</sup>*Richard*<sup>Q</sup> [, Duke of] *Gloucester*', and it is not clear why Siemon bothers to mark that this first name comes from Q1 since ordinary regularization of character names would in any case warrant the intervention. Another odd use of the superscripted <sup>Q</sup>...<sup>Q</sup> markers occurs at the end of I.iii, the suborning of the two murderers. It is clearly the end of a scene as the location is about to change, and F has '*Scena Quarta*' as the next line, yet Siemon prints '<sup>Q</sup>*Exeunt*<sup>Q</sup>'. Using superscripted markers instead of confining variants to the collation band is usually justified as a way of highlighting plausible alternative readings, but here there is no alternative: the scene must end with a clearing of the stage.

The next scene, including Clarence's murder (I.iv.84-282), is where the quartos differ from F most extensively. Siemon follows F for almost all readings except where he thinks it reflects censorship of swearing or profane religious matter, for which he reverts to Q1. Just how he represents these interventions is not immediately clear. At I.iv.125 Q1 reads 'Zounds he dies, I had forgot the reward' while F has 'Come, he dies: I had forgot the Reward', and Siemon follows Q1 (with some minor repunctuation). There is no indication in the body text that he has done this: the information is buried in the collation. Yet at I.iv.143-4 Q1 reads 'Zounds it is euen now at my elbowe perswading me | Not to kill the Duke' and F has 'Tis euen now at my elbow, perswading me not to kill the Duke', and Siemon again departs from his usual authority, F, to here follow Q1. But this time he puts *Zounds* inside <sup>Q</sup>...<sup>Q</sup> and omits to mention it in the collation. The rule seems to be that where Siemon rejects a copy-text word and adopts a non-copy-text word, he need not draw attention to this in his body text—just mentioning it in the collation will do—but where he adopts a non-copy-text word for which his copy text has no word he draws attention to it with <sup>Q</sup>...<sup>Q</sup>. It is not clear why his copy text having no word at the point of Siemon's departure from it should cause him to mark the departure more heavily than he does when his copy text has a word he rejects: is not the rejection of an erroneous blank essentially the same editorial action as the rejection of an erroneous word?

There are several such moments of apparent inconsistency in this scene. At I.iv.188-9 Q1 reads 'I charge you as you hope to haue redemption, | By Christs deare blood shed for our grieuous sinnes' and F has, in place of these, just the single line 'I charge you, as you hope for any goodnesse'. Siemon follows Q1 to print 'I charge you, as you hope to have redemption, | <sup>Q</sup>By Christ's dear blood, shed for our grievous sins,<sup>Q</sup>'. The logic of Siemon's intervention in adopting Q1's reading is that the alteration of *to haue redemption* to *for any goodnesse*

was part of the same act of censorship that struck out the entire second line present in Q1, and indeed he considers the whole matter in one textual note covering both lines' censorable religious content. But his rules about use of Q...Q markers means that he has to treat this single act of censorship in different ways in the two lines: *to haue redemption > for any goodness* is noted only in the collation, while the inclusion of Q1's 'By Christs deare bloud shed for our grieuous sinnes' gets the full Q...Q treatment and is not recorded in the collation. Siemon believes that a single intervention for a single reason produced this variant—and he rightly wants to undo it—so it is hard to see why he thinks different signals for parts of essentially one variant are the best way to make the modern reader aware of what happened and what has been done to reverse it.

A surprising choice is that Siemon sticks with F for 'I hope this passionate humour of mine will change' (I.iv.117–18) where Q1 calls it a *holy humor*. Since Siemon thinks that religious censorship has affected the whole of the murderers' scene and that the play's 'generally ironic treatment of religion... may have occasioned particularly close scrutiny' so that a hope for *redemption* was revised to a hope for *goodness* (I.iv.188–9n.), it is odd that he does not think *holy > passionate* part of that process. I mentioned that for almost all readings Siemon follows F, but at I.iv.236 F omits the quarto line 'And chargd vs from his souls, to loue each other'. Because Siemon thinks this necessary to the meaning of the speech it appears in, he concludes that it was most likely accidentally omitted by F's compositors and he reinstates it. In fact the passage makes just as good sense without the line, although it is less moving, so one could argue for F and Q1 offering equally viable alternatives. In a passage absent in Q1, F has Clarence ask 'Which of you... If two such murtherers... came to you, | Would not intreat for life, as you would begge | Were you in my distresse' (I.iv.256–60). The problem is 'as you would begge', which seems ungrammatical. *The Oxford Complete Works* fixed it by putting a dash after *distress* to show that Clarence is cut off, unable to complete his thought, but Siemon goes for the solution used by his Arden2 predecessor Hammond and emends *as* to *Ay*.

At II.i.5, F has the king reconcile his relatives so that 'more to peace my soule shall part to heauen', while the corrected state of Q1 has the more meaningful 'now in peace my soule shall part to heauen'; the uncorrected state of Q1 has the impossible reading '...depart from heauen'. Siemon thinks F wrong, but rather than adopt Q1c he goes for Nicholas Rowe's conflation of 'more in...', noting that 'editors often follow it' but without making a case for it. He does a similar thing at I.iv.236n., writing that 'Most editors include this Qq line [that F omits] as essential...' before specifying why he thinks they are right to do so. Likewise at II.ii.145 he writes that 'Most recent editors include this Qq line [that F omits]' and again at II.iv.21 ('Most editors assign...'). Siemon seems a tad too concerned with the editorial tradition, and although he generally gives the reasons for his decisions he repeatedly prefixes them with an observation that he is doing as others have done. Siemon adopts from Qq the king's requirement that Hastings and Rivers reconcile themselves, over F's line that has Dorset and Rivers do it (II.i.7), on the perfectly reasonable grounds that Dorset says nothing and there is no reason to suppose he is at

enmity with his uncle Rivers. Siemon adopts another Q1 reading of *God over F's heauen* (II.i.39) on the grounds of likely religious censorship. At II.i.57 Richard apologizes if he has offended anyone *unwillingly* (F) or *unwittingly* (Q1) and Siemon thinks the former possible but he nonetheless departs from his copy text F to follow Q1. At certain times it does not take much to make Siemon depart from his copy text.

Occasionally Siemon offers a speculation that appears not to have been entirely thought through. At II.ii.145 the quartos contain a line absent from F—*Ans. | With all our hearts*—which Siemon includes in his edition. Siemon speculates that its coincidence with a column break in F might have caused it to be overlooked. This could be the case only if the compositors stopped setting when they had completed exactly one column, and there is no reason to suppose that they did. From the reuse of rules Hinman concluded that ordinarily the centre rule was added as soon as the first column was in type, but since adjustments for balance would have to be made for every page this implies no more than pausing somewhere near (not exactly at) the column end. Where the columns were set by different men, a marker approximately dividing the copy might be useful, but this page was not shared.

At III.i.86 Siemon retains the spelling *valure* for 'His wit set down to make his valure live' despite noting that 'it has the same triple meaning as "valour"'. He appears to prefer the word's obsolete spelling because it 'reminds one of its polysemousness', but if the meanings are the same as the modern *valour* this decision seems to contradict his general principle on modernization. Siemon follows F in having a priest enter to Hastings and exchange a few words with him before Buckingham enters and comments on this conference (III.ii.105–10). Q1 has the same action except that the priest says nothing; Hastings simply acknowledges the priest and speaks in his ear. Thus Q1 saves a speaking part. Strangely, Siemon imports from Q1 the stage direction '*He whispers in his ear*' (putting it inside <sup>Q</sup>...<sup>Q</sup> markers), which surely is an action arising from the saving of a speaking part and ought not to be conflated with F's alternative version. That is, either talking openly with the priest (as in F) or whispering with him (as in Q) will do to motivate Buckingham's question 'talking with a priest?' and his comment on shriving, so there's no need to import Q1's stage direction.

Siemon follows F in having Ratcliffe enter at the start of Act III, scene iv, set in London despite the fact that he was in the previous scene set in Pomfret, West Yorkshire. Not only does this create a temporal/geographical problem for the reader and audience—when did he make the long journey down to London?—but it also violates the Law of Re-entry. Siemon decides that these inconsistencies are 'probably preferable to Qq's awkward and inconsistent substitutions of Catesby for Ratcliffe and Lovell' (III.iv.77n.). Siemon sticks to F's highly unmetrical 'Well, well, he was the covert'st sheltered traitor | That ever lived. | Would you imagine, or almost believe' (III.iii.33–5), where the problem is the short line in the middle, rather than patch it from Q1 where there is a clearly displaced half-line obtruding in the previous speech ('Looke ye my Lo: Maior'), which half-line fits perfectly the gap here. The logic of those who patch F from Q1 here is that 'Looke... Maior' was written in a

manuscript in such a way that it got displaced in Q1 and omitted altogether in F. Siemon, following recent editors, prefers F's unmetrical short line and plausibly suggests that Buckingham is here displaying his talent for histrionic pauses and breakings-off. Also based on Buckingham's character is the attribution to him of the lines 'I never looked for better at his hands | After he once fell in with Mistress Shore' (III.v.50-1), as in F, rather than the mayor who gets them in Q1. This attribution requires Buckingham to switch between *I* and *we* pronouns in one speech, which Siemon explains is not the intermittent intrusion of a royal plural but Buckingham distinguishing between his own opinions and those he shares with Richard (III.v.56n.).

Siemon is convinced that Shakespeare used *mine* before words beginning with a vowel when he did not want to emphasize the possessive and *my* when he did, so that in place of F's 'And when my Oratorie drew toward end' he has Buckingham say 'And when mine oratory drew toward end' (III.vii.20), using *mine* from Q1. As Siemon acknowledges when treating the same problem elsewhere in his edition, compositors appear to have imposed their own preferences regarding *my/mine* before a vowel and deciding whether the possessive is to be emphasized is subjective. Given these causes for doubt, it is surprising that the otherwise conservative Siemon should here depart from his F copy without giving a compelling reason for thinking it wrong. Q1 has Buckingham say 'Come Citizens, zounds ile intreat no more' to which Richard replies 'O do not sweare my Lord of Buckingham', whereas F has Buckingham say the much less objectionable and less forceful 'Come Citizens, we will entreat no more' and omits Richard's response (III.vii.218-19). As Siemon points out, censorship that removed Buckingham's swearing (*zounds ile > we will*) obviously entailed cutting of Richard's objection to it, so Siemon restores Q1's readings here. Having accepted the crown, Q1 has Richard pretend to return to his devotions with 'Farewel good coosine, farwel gentle friends' where F has the plural 'Farewell my Cousins...' (III.vii.246). Siemon adopts F's wording but with the singular *cousin* from Q1, on the grounds that 'It seems improbable that he would be so familiar with mere citizens, since he has expressly distinguished his own *degree* from their *condition* (p. 142)'. But Richard wants to appear to have relented over the course of this scene—no longer aloof and accepting the honour thrust on him—so an overly familiar term might be just what he thinks he should use at this point. Again, a departure from his copy when it makes reasonable sense is inconsistent with Siemon's conservatism elsewhere.

F has the Duchess of York notice the entrance of 'My Neece Plantagenet, | Led in the hand of her kind Aunt of Gloster?' whereas Q1 cuts the second line (IV.i.1-2). This affects the casting, as in F's reading the niece must be Clarence's daughter—to whom Anne, Richard's wife, is aunt—while Q1's reading allows the niece (a relationship used loosely) to be Anne herself. In accepting F's reading Siemon is obliged to follow Lewis Theobald's lead and emend the scene's opening stage direction to include Clarence's daughter, who is mentioned in neither Q1 nor F's direction. Following F, Siemon is obliged at IV.ii.81 to omit the Q-only exchange in which Richard asks about the murder of the princes in the Tower: 'Shal we heare from thee Tirrel ere we sleep?', and the murderer's reply 'Ye shall my lord', which is almost exactly the same as an

exchange between Richard and Catesby at III.i.188–9. The editors of the *Oxford Complete Works* argued that this exchange is connected to the ‘clock-passage’ near the end of the scene, where Richard is anxious about time in a way quite unnoticed and unappreciated by Buckingham, and they import it from Q1 for that reason. Editing the single-volume Oxford Shakespeare edition, Jowett decided that the echo was deliberate—a possibility that Gary Taylor did not consider for the *Oxford Complete Works*—and included the exchange in both scenes. By following F, Siemon has included the exchange in III.i where it does not make a lot of sense—Catesby does not go to sound out Hastings until the next morning—and omitted it in IV.ii where it makes a great deal of sense in relation to Richard’s insomnia and impatience, and where it connects with Richard’s appalling desire to enjoy hearing the full story of the princes’ murder as an ‘after-supper’ treat (IV.iii.31).

Siemon includes the whole ‘clock-passage’ exchange, present in Qq and absent in F, surveying the various explanations for its absence in F and noting that ‘most commentators agree that it is Shakespearean’ (IV.ii.97–114n.). Apart from this consensus, Siemon gives no reasons for his including it—against F’s authority—and this is all the more surprising since on F’s authority he omitted the Richmond/Tyrrel exchange (IV.ii.81) about hurrying back with news of the princes’ murder, which plausibly can be connected to this passage. Siemon chooses not to adopt Q1’s version of Buckingham’s response to Richard’s jack-of-the-clock insult, which is a clearly petulant ‘Whie then resolute me whether you wil or no?’, preferring instead F’s simple repetition of the polite question Buckingham has been pursuing all along: ‘May it please you to resolve me in my suit?’ (IV.ii.115). There is an argument to be made that Q1’s petulant question goes with Q1’s inclusion of the ‘clock-passage’, while F’s polite question goes with F’s omission of the ‘clock-passage’, in which case Siemon’s conflation of Q1 and F has produced a contradiction. That is, Siemon makes Richard deeply insulting in calling Buckingham a jack, but Buckingham appears not to notice. However, Siemon rightly comments that the seemingly polite line for Buckingham that he has adopted ‘could be inflected many different ways, expressing frustration, despair, incredulity or any combination of these emotions mixed with a desire not to offend’ (IV.ii.115n.).

At IV.iv.37–9 Q1 reads ‘And let my woes frowne on the vpper hand, | If sorrow can admitte societie, | Tell ouer your woes againe by vewing mine’, where F has almost the same (except *woes* > *griefes*) but omits the last line, which Siemon restores using his <sup>Q</sup>...<sup>Q</sup> notation. In fact, F makes good sense on its own because the ‘If...’ can just as well, or even better, refer back to its preceding line—to mean ‘if we can share these pains then mine is foremost’—as it can refer forward to the missing line to mean ‘if we can share these pains then count your sorrows again in hearing mine’. Oddly, Siemon gives no defence for importing F’s missing line from Q1. F has ‘That reignes in gauled eyes of weeping soules: | That excellent grand Tyrant of the earth’ (IV.iv.51–2) which, as Siemon says, makes better sense if one puts the second line first. But he also moves the preceding line of F, ‘That foule defacer of Gods handy worke’, to after this pair, admitting that it may stay where it is in F ‘without

spoiling the sense'. So why move it? At IV.iv.64 Siemon uses Q1's 'Thy other Edward dead . . .' in place of F's 'The other Edward dead . . .', but admits that F might be right because although it introduces added ambiguity into an already notoriously confusing exchange that might be intentional: 'might interchangeability have been the playwright's point?'

Siemon has Queen Elizabeth call words of complaint 'Airy succeders of intestate joys' (IV.iv.128), which draws on Q1's *intestate* rather than F's *intestine*. As he admits, this choice of metaphor—words as empty-handed inheritors of joy that died leaving nothing behind—suppresses a much ruder possibility of words as farts. I would have thought F's reading particularly attractive since Queen Elizabeth goes on to say of them 'yet do they ease' (IV.iii.131). Of the means to woo Queen Elizabeth's daughter, Folio Richard says to her 'That I would learne of you' (IV.iv.268), but Siemon follows Q1 to read 'That would I . . .', giving no more reason than 'Most recent editors prefer' it. Siemon follows Q1 to have Queen Elizabeth refer to the children left fatherless by Richard who will wail it 'in their age' (IV.iv.392) rather than F's 'with their age', but he sounds scarcely convinced, pointing out that F's reading provides a parallel with the next line but one in which Q1 and F agree that the parents left childless by Richard will also wail it 'with their age'. However, Siemon might have defended his choice by saying that 'in their age' means when the children grow up while the parents wailing 'with their age' means now, in their old age.

As this long scene moves to its final phase, Siemon notes that in F Queen Elizabeth exits before Richard has told her to bear his kiss to her daughter, and he thinks this is due to there being not enough room for the stage direction to take its correct place near the bottom of page s5v, so it got displaced upwards. This may be, but the reader is left wondering why matter could not be carried over from the bottom of s5v to the top of s6r, where there is room. Siemon's explanation would make sense if the compositors were particularly lazy or in a hurry, or if s6r were already printed or about to be and they did not want to disturb it. But the usual practice was for the pages in the second half of a Folio quire, here s4r to s6v, to be set in reading order by one compositor (for this quire, compositor B) while the pages in the first half of the quire were set in reverse reading order by the other compositor (here, A). It so happens that this order was slightly departed from in that compositor A jumped in and set page s6r in the second half of the quire (compositor B's half). It could be argued that compositor A did this because compositor B was falling behind and that in response they agreed the s5v/s6r page boundary to enable compositor A to start on s6r while compositor B completed s5v. But unless forme s1v:6r actually went to press before what would normally be its predecessor, s2r:5v—and we have no evidence that it did—the simplest expedient to solve crowding at the bottom of page s5v would be to move a line or two to the top of s6r.

It might be argued that moving a line to the top of the next page—which is all that would be needed to make room for Queen Elizabeth's exit direction to appear in its correct place—would create an unattractive page because s5v's column b would end on the centred words *Exit Queene*. However, several columns in this quire and elsewhere end with such a lonely stage direction, for

example r6vb, s1ra, s3rb, and s4ra. The truth is we do not know why at the bottom of s5v compositor B squeezed a stage direction in at the end of a line and seemingly too early, and we cannot say that he was forced to by the exigencies of printing. For all of Siemon's edition from V.iii.49 to the end, Q1 is his copy text because F is clearly a simple reprint of Q3. Yet, within this, Siemon departs from Q1's perfectly meaningful line 'Richard loues Richard, that is I and I' (V.iii.186) to favour F's '... I am I', on purely poetical grounds. After the text there are three appendices. The first, on textual matters, is discussed above. The second is a doubling chart showing that thirteen men and seven boys are needed for the play as it appears in this edition; that is a rather a lot of boys for a playing company of the period. The third appendix gives genealogical tables for the aristocratic families depicted in the play.

Had things gone according to previous planning, there would be another Arden Shakespeare edition to review this year: Lynette Hunter and Peter Lichtenfels's *Romeo and Juliet*. For reasons not disclosed in the book, it has instead appeared as part of a monograph called *Negotiating Shakespeare's Language in Romeo and Juliet: Reading Strategies from Criticism, Editing and the Theatre*. The edition itself appears as an electronic text included with a DVD tucked into the back cover of the monograph, which latter will be reviewed first. A blurb page before the title page says that this book includes 'on CD-Rom the first modern edition of the text of *Romeo & Juliet*', and it is hard to know what is meant by this; presumably it depends on what one understands by 'modern'. The oddness of the entire project is apparent from the introduction (pp. 1–5), which includes personal material that normally is found in an acknowledgements section, such as the recollection that its authors, who are married, spent Christmas holidays walking in Yorkshire. The first two chapters, 'The Reader and the Text' (pp. 9–31) and 'The Actor and the Stage' (pp. 33–58), are not relevant to this review. The third chapter is called 'The Editor and the Book' (pp. 61–82), and much of it is repetition of work already published.

Because John Danter printed sheets A–D of Q1 *Romeo and Juliet* and Edward Alde printed sheets E–K, Hunter and Lichtenfels think in terms of 'Danter beginning the project and Alde finishing it' (p. 65), but of course we do not know that: they could have worked simultaneously. Unfortunately their consideration of the padding in Alde's section is ignorant of Jowett's argument that Henry Chettle extended the stage directions, perhaps from memory of performance. They refer to the 'current consensus' on the matter (p. 65), but support their account of it with references to R.B. McKerrow writing in 1933 and Harry R. Hoppe writing in 1948. Not knowing the argument that the padded stage directions probably come from Chettle, Hunter and Lichtenfels incorporate them into their edition under the misapprehension that they provide 'a wealth of information about stage props and stage actions, as well as some indication of what theatre practitioners thought about the movement of the play, pace and timing' (p. 66). Far from exemplifying the explanatory power of what they call the 'transdisciplinarity' of their approach—that is, theatrical and editorial knowledge coming together—Hunter and Lichtenfels unintentionally

exemplify the danger that a lack of knowledge in one field may create a vacuum that is filled by irrelevant knowledge from the other.

Hunter and Lichtenfels's knowledge of key works in the bibliographical tradition is scant to the point where it would, if presented in a Ph.D. dissertation, imperil the awarding of the degree. They think that in his classic *The Stability of Shakespeare's Text* [1965] E.A.J. Honigmann argued that 'there could be no "definitive" text for Shakespeare's plays partly because the full detail of their historical production is lost' (p. 69). They have no idea that he was primarily concerned with authorial tweaking when copying out fairly. Hunter and Lichtenfels think that Taylor's essay 'Swounds Revisited' explains that *zounds* was 'a word apparently so strong that the Folio editors 20-odd years later would not set it' (p. 73). They appear to believe that the Folio's 'editors' (John Heminges and Henry Condell?) set type. In two paragraphs Hunter and Lichtenfels gallop through McKerrow's best-text principle of editing, W.W. Greg's response to it in his essay 'The Rationale of Copy-Text', and recent work by David C. Greetham, Peter L. Shillingsburg, and G. Thomas Tanselle. All this careful scholarship is thoroughly garbled in their accounts of it. A taste of this can be had from a sentence that follows a confusion of McKerrow's emphasis on recension with Greg's on emendation: 'We have to have better reasons for using Q2 than copy text theory, to provide more appropriate guidelines for reading the text today' (p. 79). Hunter and Lichtenfels do not use English words the way the rest of us do, objecting that 'traditional editorial practices can be evasive and implicitly authoritative' (p. 80) when they surely mean by that last word authoritarian. Or perhaps they think editions ought not to be authoritative, which would help account for theirs.

Chapter 4 (pp. 85–131) is called 'The Family: Behaviour, Convention, Social Agreement and their Breakdown', so it was a surprise that here Hunter and Lichtenfels place their summary of how early books were made. They call the spaces between words 'slugs' (p. 103), which is in fact the name for a line of type created as a single piece of metal by a Linotype machine. They seem to think that wooden printing presses of Shakespeare's time were fundamentally different from the 'compact steel printing presses of the nineteenth and twentieth centuries' (p. 103), but in fact little changed over this period, and of course the metal used to make presses was iron, not steel. They date the replacement of the compositor by the computer to exactly '1987' (p. 103), whereas in fact stereotyping and mechanized compositing (cold- and hot-metal) had been putting compositors out of work since the nineteenth century, and mid-twentieth-century phototypesetting and offset lithographic printing were the bridge to the nearly complete abandonment of setting type by hand in the 1980s. Hunter and Lichtenfels imagine a compositor at work 'setting the line from right to left' (p. 104), which a moment's reflection should have told them would be impossible for prose—unless the copy were written to be read from right to left, as in Hebrew and Arabic—since each line ends when there is no room for another word, and one cannot know in advance when that will be. Right-to-left setting is not impossible for verse, where the line endings are set by the poet, but it is awkward and quite unnecessary. Hunter and Lichtenfels make bizarre references to compositors 'measuring out' (p. 104)



type when setting seriatim, where I think they must mean 'set type continuously without regard for line breaks'. They think that casting off manuscript copy means 'mentally estimating' (p. 105) how much of it will occupy each printed page, but of course Joseph Moxon's seventeenth-century manual of printing gives detailed descriptions of how it is worked out on paper. All this garbled stuff about printing is included only so that Hunter and Lichtenfels can explain (poorly) G. Blakemore Evans's speculation about why the Queen Mab speech is mislined as prose in Q2.

To explain a transition from setting speeches as prose to setting speeches as verse in Q1—and without mentioning which they think the speeches should be—Hunter and Lichtenfels suggest that perhaps a second compositor began setting the first page of sheet D before his colleague had finished setting the last page of sheet C, and hence this second compositor forced the first to compress his speeches, to set them as prose, on the last page of sheet C (pp. 120–1). It is clear that Hunter and Lichtenfels assume that page C4v was the last page of sheet C set in type and that page D1r was the first page of sheet D set in type. Even if this were the case, the compression at the bottom of C4v could be relieved by one compositor simply passing a few lines to the other. But in fact C4v was not the last page set on sheet C. Type-recurrence evidence uncovered by Frank E. Haggard in 1977 shows that C(outer), comprising C1r, C2v, C3r, and C4v, was set before C(inner), comprising C1v, C2r, C3v, and C4r. Thus pressure on C4v, noticed when it was set to a predetermined sheet-break, could have been relieved by transferring lines to its predecessors C4r and C3v that were not yet set. Also, there is no reason to think that sheet D was begun before sheet C was complete, so lines could also have been transferred forward from a tight ending of C to the beginning of D. All this does not make it impossible that C4v is crowded because the casting off was misjudged, but it takes away from the power of the mechanical explanation that Hunter and Lichtenfels offer for the setting of prose and verse on C4v and on D1r.

The remainder of the book—chapters 5 ('The Humours: Anarchy and Doubtfulness', pp. 133–78) and 6 ('Governance: The Law, Medicine and the Recuperation of the Social' pp. 179–212)—is outside the scope of this review. The electronic edition of the play, provided as an e-text on a disk, has a copyright date of 2007 while the printed book's is 2009. The first line of the play text is its title '*Romeo and Juliet*', for which Hunter and Lichtenfels provide the startling collation note 'TITLE] *this edn.*'. A check of their list of abbreviations confirms that they mean by '*this edn.*' what we would expect: 'a reading adopted for the first time in this edition'. But of course they are not the first editors to call the play *Romeo and Juliet* and it is not clear why they think they are. The opening stage direction is '*Enter SAMPSON and GREGORY, of the house of Capulet, with swords and bucklers*' and at the back of the edition this is glossed with a Longer Note beginning 'Heavy swords and shields were the ordinary weapons of servants; gentlemen wore rapier and dagger'. These words are a direct quotation from G.L. Kittredge's 1946 edition (in the collection *Sixteen Plays*), but Hunter and Lichtenfels do not put it in quotation marks nor attribute it to him. An undergraduate who did this in an essay could escape a charge of plagiarism only by pleading guilty to the lesser

charge of incompetent referencing. Hunter and Lichtenfels miss the interesting point that Charles Edelman drew attention to: wearing a sword was fairly unremarkable but nobody normally went around the streets of early modern London carrying a shield, so the stage direction indicates that the characters are either anticipating trouble or are heading for Smithfield (or rather Verona's equivalent) where fencing was practised on Sunday mornings. At l.i.74 Hunter and Lichtenfels offer the collation note 'crutch, a crutch] *F*; crowch, a crowch *Q2-4*'. Since *OED* attests that *crowch* was an ordinary sixteenth-century spelling of *crutch* there is no need for a collation note. Hunter and Lichtenfels appear to be unaware of the basics of modernization and their edition falls below the threshold for further consideration here.

Four monographs wholly relevant to this review were published in 2009, but only two will be noticed. It is regrettable that University of Delaware Press was unable to provide review copies of Adele Davidson's *Shakespeare in Shorthand: The Textual Mystery of King Lear* and Paul Menzer's *The Hamlets: Cues, Qs, and Remembered Texts*; they will be noticed next year. The third monograph is Hugh Craig and Arthur Kinney's *Shakespeare, Computers, and the Mystery of Authorship*, which offers compelling arguments for the attributions of certain works. The book's only significant flaw is that it misses an opportunity to explain to a Shakespearian audience the mathematics used in stylometry, such as Principal Component Analysis, and instead points readers to existing textbooks that few of them will understand. In the Preface and Acknowledgements (pp. xv-xix), Craig and Kinney assert that there are Shakespearian things we can measure that underlie the variations in the speeches of 'Hal, Falstaff, and Hotspur' and that can distinguish them from the characters of other dramatists (p. xvi). Brian Vickers having done the groundwork for five of Shakespeare's collaborations—presumably *Titus Andronicus*, *Timon of Athens*, *Pericles*, *Henry VIII*, and *The Two Noble Kinsmen*—this book will concentrate on *Edward III*, *Arden of Faversham*, additions to *The Spanish Tragedy*, Hand D of *Sir Thomas More*, *Edmond Ironside*, Folio *King Lear*, *1 Henry VI*, and *2 Henry VI*. The electronic texts used are transcriptions of early editions and software did the work of bundling the various spellings (such *folly/follie/folie*) under one headword (*folly*). The book is based on a corpus of 165 play e-texts created by the authors rather than on texts drawn from Literature Online. The book is here described as having 'four authors' (p. xix) and it is noticeable that the title page does not report that it is 'edited by' anyone, so although it looks like a collection of essays it is really a collaboratively authored monograph for which two of the authors are simply not mentioned on the title page.

The introduction (pp. 1-14) makes a weak start by giving a rather imprecise summary of where neuroscience stands on the individuality of language, which marvels at the combinatorial potential of millions of neurons' connections but does not indicate how they produce idiosyncratic language or personality. There is rather a lot of irrelevant writing about DNA, RNA, and protein biosynthesis. Things pick up when Craig and Kinney describe the phenomenon of the characteristic collocations of two-, three-, and four-word groups and observe that even when they are deliberately impersonating another's

style, writers betray their authorship to 'tests of common words, rare words, and word pairings' (p. 9). One test alone is seldom reliable so investigators need a battery of them. Function words, that is those having a syntactical function rather than a semantic one, are most commonly counted, but they tend to fluctuate simply according to genre. Collocations—what Craig and Kinney call word-combinations—are particularly good for working up an authorial signature. Stylometrics, they observe, does not have to confine itself to authorship attribution: we can also date texts and group them according to various kinds of likeness.

In chapter 2 (pp. 15–39) Craig and Kinney describe the methods that will be used in the book's case studies. Shakespeare uses the word *gentle* nearly twice as frequently as do other dramatists of his time, in all the genres. To turn this knowledge into a test, one must divide Shakespeare's work and others' into segments of a fixed length (say 2,000 words) and compare how often a Shakespeare segment contains *gentle* with how often a non-Shakespearian segment contains *gentle*. (This technique tends to discount clusters of *gentle* since a segment is counted as a container of *gentle* whether it has one or ten uses of the word.) Likewise, Shakespeare's avoidance of *yes*, *brave*, *sure*, and *hopes*, and his liking for *answer* and *beseech* make him stand out from his fellow dramatists. Add a few hundred more marker words to this batch—some he used a lot, some he avoided—and one has a reasonable test: does the unknown passage lack the words he avoids and feature the words he likes? If so, it is by Shakespeare. If it features the words he avoids and lacks the words he likes, it is non-Shakespearian. Craig and Kinney admit that in doing their work with this kind of test they used strings rather than linguistic words, so that for example *hope* and *hopes* are counted as different things not as variant forms of one word. They explain that this is done to avoid introducing arbitrariness and inconsistency, and neglect to mention that it is also a lot of work to lemmatize a text. In any case, it is wrong to imply that lemmatizing is arbitrary or inconsistent. Fortunately, unlemmatized texts are perfectly valid for their tests so long as all the dramatists are treated equally.

An important test of a stylometrician's method is to ask whether it properly distinguishes all of Shakespeare's work from everyone else's. Craig and Kinney use 2,000-word segments from twenty-seven of his core sole-authored plays, and the question to be asked is whether the Shakespearian segment with the least number of words he favoured nonetheless has more of those words than has the non-Shakespearian segment with the greatest number of them. The question also should be asked of the words he avoided, with a view to determining the overlap between Shakespeare's usage and everyone else's. Craig and Kinney give a detailed account of how they validated their test. They took *Coriolanus* out of their core set of twenty-seven Shakespeare plays and found 500 words that appear in many of the segments from the remaining twenty-six plays and appear infrequently in the segments from the non-Shakespearian plays. Specifically, for each word they counted how many segments by Shakespeare it appeared in and divided that by how many Shakespeare segments there are, thus producing a score between 0, for words that appear in no segments, and 1 for words that appear in all segments. To this they added a score derived by counting how many non-Shakespearian

segments lack the word and dividing that by how many non-Shakespearian segments there are. Thus a word with an ideal score of 2 is in every Shakespeare segment and no non-Shakespearian segment. In the event, the highest score (for *gentle*) was 1.24 and the lowest was 1.03, and Craig and Kinney simply took the words with the top 500 scores. The whole procedure was repeated in reverse to find non-Shakespearian markers.

At this point Craig and Kinney mention that they excluded function words from the segments before they started; for these they have a different procedure. For each segment of play text, Craig and Kinney counted how many Shakespeare marker words it has and divided that by total number of different words in that segment—or rather the number of strings, since they did not lemmatize—in order to show how frequently that segment uses his favourite words. They did the same for the words he avoids, and, plotting for each segment its place on a graph whose axes are ‘words Shakespeare uses more than others do’ and ‘words others use more than Shakespeare does’, the segments visibly cluster into two populations. The Shakespeare segments are all high on the ‘Shakespeare likes’ axis and low on the ‘Shakespeare avoids’ axis, and the non-Shakespearian segments are all low on the ‘Shakespeare likes’ axis and high on the ‘Shakespeare avoids’ axis. The graph shows two variables at once, and if only one had been used—either the x-axis, frequency of appearance of words Shakespeare likes, or the y-axis, frequency of appearance of words Shakespeare dislikes—the overlap would be considerable: it is the two-at-once procedure that makes the populations largely non-overlapping. Putting into the graph the *Coriolanus* segments, which played no part in setting up the test, they all appear comfortably in the Shakespeare zone so we could have assigned them to him with confidence using just this test. Doing the whole thing again for a non-Shakespearian play abstracted from the non-Shakespearian set—Thomas Middleton’s *Hengist, King of Kent*—showed that all but one of its segments comfortably sit in the non-Shakespeare zone of the graph.

For a new test to combine with the existing one, Craig and Kinney take the very common function words and count their frequencies, using *Titus Andronicus*, *Timon of Athens*, *Henry VIII*, and *The Two Noble Kinsmen* as their texts because the boundaries of collaboration in them (with George Peele, Middleton, John Fletcher, and John Fletcher respectively) are well established, as are the collaborators’ other plays, whereas for *Pericles* we have only one other George Wilkins play. If successful, this ought to be a rigorous test because in collaboration writers generally try to produce something self-consistent using the same materials and genre, so the ability to tell their shares apart is impressive. Craig and Kinney work with whole scenes of at least 1,500 words, of which *Titus Andronicus* has five: I.i (Peele) and II.iii, III.i, V.ii, and V.iii (all Shakespeare). Rather than use all the function words, Craig and Kinney set out to discover the ones that most distinguish Peele from Shakespeare in *Titus Andronicus* using what is known as Student’s t-test. This procedure governs all the function-word tests in the book: only those words for which the authors being tested have a significant preference or dislike are used, and this set of words will obviously be different for each test; Craig and Kinney ought to have spelt that out at this point. They define the statistical

concepts of mean, variance, and standard deviation, and point out that for a normal distribution (a concept they do not gloss) around two-thirds of the values will be above or below the mean by no more than the standard deviation value; that is, they will be within the first standard deviation.

Here the mathematics gets tricky. The *t*-value is the difference between the means for Shakespeare counts and Peele counts—how often they use a particular word in each of their segments—divided by the standard deviations for all the counts. So, a high *t*-value happens when the two means are far apart and the standard deviation is low, which happens when the means—the rates of usage of a particular word—for Shakespeare and Peele are markedly different but the readings for both men considered together are not terribly widely spread. If the readings were widely spread, of course, then the differences in the two men's means could be generated by chance alone. Craig and Kinney calculate the *t*-values for 200 function words in twenty-seven Shakespeare plays and four Peele ones, and fifty-five of them turned out to be good discriminators of the authors. In particular, *and* and *thy* are words that Peele uses a lot more than Shakespeare, and *it* and *very* ones that Shakespeare uses a lot more than Peele. At this point Craig and Kinney introduce Principal Component Analysis (PCA), but rather than explain it they point the reader to standard textbooks. However, they offer a useful analogy borrowed from a textbook: if one had a set of data that showed the height of several people and another set that showed their weight, one could derive from them a Principal Component called 'size' that combines these data for each person. This value would not capture all the detail, since some people are tall but light, others short but heavy, but it would account for most of the correlation between height and weight.

Craig and Kinney use as their PCA variables the frequencies within each play segment—the 2,000-word segments from twenty-seven Shakespeare plays, the 2,000-word segments from four Peele plays, and the five 1,500-word or more scenes from *Titus Andronicus*—of the words known to be the best discriminators of Shakespeare and Peele. On a graph where each segment's position along the *x*-axis is its first Principal Component score and its position on the *y*-axis is its second Principal Component score, the Peele and Shakespeare writings occupy fairly distinct zones, and scene I.i belongs with the other Peele writing. Craig and Kinney repeat the process—finding the most discriminating function words, then graphing the PCA results—for the bits of *Timon of Athens* by Shakespeare and Middleton. Interestingly, among the markers from Middleton is '*that* as a demonstrative' (p. 34), but Craig and Kinney do not tell the reader how the demonstrative use is distinguished from other uses, having earlier indicated that words are treated as merely strings of characters without lemmatization. Again there appear distinct zones for each author on the graph and the investigators find that the bits of *Timon of Athens* attributed to each author by other means mostly sit in their respective zones. Likewise for *Henry VIII* and *The Two Noble Kinsmen* with Fletcher. So, thus validated, Craig and Kinney have at their disposal two tests—one excluding function words and one using them—that give good but not perfect results when used with bits of plays where we know the authors. In the rest of the

book the authors and their co-investigators apply these two tests to segments of plays of unknown authorship.

Chapter 3 (pp. 40–78) is by Craig alone, and concerns ‘The Three Parts of *Henry VI*. Craig acknowledges the problem that if collaborators worked together on a small unit such as a scene, or revised one another’s work, the chances are they would erase the evidence of individual authorship. Craig divides the three plays into 2,000-word segments, which—because not following natural boundaries such as scene division—are likely each to contain mixed authorship. Since he does not yet know the boundaries of the collaboration, that is all he can do. Using the lexical and function-word tests described above, these segments were compared with the known Shakespeare plays, providing for each segment two measures of likeness-to-Shakespeare. Taking first just some early Shakespeare plays—*Richard III*, *Richard II*, *King John*, *The Two Gentlemen of Verona*, *The Comedy of Errors*, and *Love’s Labour’s Lost*—Craig plots their likeness to the rest of the securely attributed Shakespeare set, that is the twenty-seven known Shakespeare plays minus, for each test, the play being tested. This produces a graph showing where each segment from each of these six plays sits in its likeness to Shakespeare, with the score from the function-word test along the x-axis and the score from the lexical-word test along the y-axis. This graph shows that most 2,000-word segments from these plays are more like than unlike Shakespeare. The worst outlier is one segment from *Richard II*, that fails both tests (function-word and lexical-word) and there are thirteen other segments (out of fifty-eight segments in all for the six plays) that fail one or other of the tests and are falsely declared non-Shakespearian. Craig concludes that since only one segment in fifty-eight is misclassified by both tests, ‘the methods are more reliably used together than separately’ (p. 47).

With the tests now calibrated, Craig repeats the operation for the 2,000-word segments from *1 Henry VI*, and they turn out to be mostly—in eight of out the ten segments—to be unlike other Shakespeare writing of his early period. This suggests mixed authorship. Likewise for *2 Henry VI*, but not quite so much unlike Shakespeare; this also suggests mixed authorship. But most of *3 Henry VI* turns out to be much like Shakespeare. The part of *1 Henry VI* most like Shakespeare on these tests is IV.ii.56–IV.vii.40, including Talbot and his son dying at the siege of Bordeaux, which Edmond Malone subjectively judged to be the only Shakespearian part of the play. John Dover Wilson and Taylor also gave this part to Shakespeare, and it is the part that Nashe celebrates in *Pierce Penniless*. Other parts that score highly on these ‘like Shakespeare’ tests are ones that many critics have thought distinctly Shakespearian, including the Temple Garden rose-picking scene.

Instead of arbitrary 2,000-word segments, Craig decides to use the division of the play proposed by Taylor but expressed in 4,000-word segments. The outcome confirms Taylor’s claim: II.iv and IV.ii–IV.vii.31 are like Shakespeare, the rest, especially the first act, are not. Taylor, like others, thought that Nashe wrote the first act of *1 Henry VI*, but unfortunately the only certain Nashe works we have to compare it with are in prose. Craig uses 4,000-word segments from Nashe’s *Pierce Penniless*, *Strange News*, and *The Unfortunate Traveller* and equally sized segments from forty-nine

single-authored pre-1600 plays, and applies the lexical-word test described above using the top 500 Nashe marker words and the top 500 not-Nashe marker words. The resultant graph shows two zones, and the first act of *1 Henry VI* falls in with the plays by others and far from Nashe's prose works. But it is closer to the Nashe prose works than any other bit of the play is, and tweaking the test so that the comparison is just with Shakespeare (rather than forty-nine plays by others) also shows that it is a bit nearer to Nashe than anything else Shakespeare did. 'The Nashe hypothesis survives, then' (p. 55). Contrary to Vickers's 2008 article (reviewed in *YWES* 89[2010]), Craig's test shows 'no affinities between Kyd and *1 Henry VI*' (p. 56) since there is clear separation on both dimensions of Craig's graph, although he admits that the sample of known Kyd drama is so small that the results may not be reliable. Tweaking the test to bring in *Soliman and Perseda*—accepting for the nonce the claim that it is his—and limiting the others' drama to pre-1600 work does not help: *1 Henry VI* stays firmly with the non-Kyd material.

As Craig explains, Vickers's tests worked by finding collocations that appear in the suspect text and author X's work, but not in the works by other authors, yet Vickers is not scrupulous on this last step. Craig points out that Thomas Merriam has found phrases common to *1 Henry VI* and plays in the Marlowe canon and nowhere else, but this does not necessarily prove anything: we need to know how often, in general, a phrase appears in writer X's work and in writer Y's work and in no one else's work. If that is fairly common—say there are phrases that only Shakespeare and Marlowe use—then Vickers's methodology is invalid. (The same point about Vickers's methodology is made in an article by Jackson considered below.) Merriam has shown that rates of function-word usage and collocations-in-common give reasons to suppose that Marlowe wrote the Joan of Arc parts of *1 Henry VI*. To test this, Craig pulls out the 1,803 words of *1 Henry VI* in which she appears and uses his lexical-word analysis to see where they fall in a test that separates the six reliably Marlovian plays from 130 plays by other writers. Comparing the word usage in the early, middle, and late Joan passages, Craig finds the early to be non-Marlovian and the middle and late to be Marlovian. Craig surveys the various objections that have in the past been raised against Marlowe's contributing to *1 Henry VI*, and finds them all weak. Imitation of Marlowe's style, a popular explanation, would not give false positives on the function-word tests used in this book because words used by a writer imitating Marlowe would drop out of the calculations here, since the method is to find words that Marlowe uses that others do not and vice versa. Craig does not admit it, but his method would be fooled by a writer able to imitate perfectly Marlowe's word preferences (for and against) across hundreds of words, but that is a tall order.

Craig returns to the analysis of *2 Henry VI* in 2,000-word segments using lexical words and function words in order to explore the possibility that Act III is Shakespearian but the rest of the play is not. Two of the 2,000-word segments—numbers 10 and 11 covering lines IV.iii.160 to V.i.13—turn out to be markedly Marlovian, and Craig notes that these contain Jack Cade's rebellion, which is notably detachable and out of keeping with York's description of Cade when revealing in III.i the plan to incite him. Running a

battery of tests, Craig repeatedly finds that segments 10 and 11 look like Marlowe. Thus Cade and the devil-dealing Joan of Arc are Marlovian characters, both characteristically claiming high birth and dying defiantly. *2 Henry VI* also has recurrent decapitation, a characteristically Marlovian device. Comparing the Cade rebellion with the popular uprisings in *Julius Caesar*, *Coriolanus*, and *Sir Thomas More*, it is notable that the last three all are nearer to Shakespeare's norm than Marlowe's in lexical and function-word tests, whereas the Cade rebellion comes out nearer to Marlowe than Shakespeare by the same tests. It does not seem that subject matter is displacing the results. The conclusion of this chapter, then, is that *1 Henry VI* and *2 Henry VI* are collaborations.

Chapter 4 (pp. 78–99), by Kinney alone, is about *Arden of Faversham*, and after an extended discussion of past attributions, including to Shakespeare, he begins his analysis using scene boundaries to generate segments. Each he subjects to a variant of the lexical-word test of 500 words common in Shakespeare and rare outside Shakespeare in plays from 1580 to 1619, and vice versa for words others use and Shakespeare avoids. (No reason for the date limits is given.) The Shakespearian and non-Shakespearian segments form clear zones on a graph of Shakespeare-uses (x-axis) against Shakespeare-avoids (y-axis). This puts scenes 4, 5, 6, 7, 9, and 16 on the Shakespeare side and scenes 1, 2, 3, 8, 10, 11, 12, 13, 14, 15, 17, 18, and Epilogue on the non-Shakespearian side. Kinney is rightly cautious about this result as the scenes concerned are rather short. From this hint about possible division of labour, he constructs larger segments to test: 1–3 (putatively non-Shakespearian), 4–7 (putatively Shakespearian), 8–9 (a bit of both), and 10–18 (putatively non-Shakespearian). The ones that he seems to assign to the wrong side are 8 and 9, which were in any case borderline, and 16, which is short. Repeating his lexical-word test for these larger segments, 1–3 are confirmed as non-Shakespearian, 4–7 are confirmed as Shakespearian, and 8–9 and 10–18 are confirmed as non-Shakespearian. The function-word test produces the same discrimination in that scenes 4–7 look like Shakespeare, but scenes 8–9 now also look like Shakespeare.

Instead of creating a test of Shakespearian versus a non-Shakespearian group of plays to compare bits of *Arden of Faversham* with, Kinney reruns the lexical-word tests using Shakespeare-versus-Kyd—first just *The Spanish Tragedy* and *Cornelia*, then *The Spanish Tragedy*, *Cornelia*, and *Soliman and Perseda*—and then Shakespeare-versus-Marlowe. These tests put all four *Arden of Faversham* segments on the Shakespeare side of the graph, which shows not that Shakespeare wrote them but that neither Marlowe nor Kyd wrote them. Kinney repeats this procedure using function-word frequencies, which for Marlowe-versus-Shakespeare gives rather a lot of overlap because their function-word habits are similar, and into this area of overlap most of the play falls. The bit that does not, scenes 10–18, falls closer to Shakespeare than Marlowe. On function words, Kyd versus Shakespeare also has a lot of overlap, but here the segments of *Arden of Faversham* are even more distinctly not Kydian. That is, they fall into the Shakespearian zone, not because he wrote them but because they are unlike the rival candidate Kyd. Kinney does not explain why he tests each segment of *Arden of Faversham* on a



Marlowe-versus-Shakespeare spectrum and then a Kyd-versus-Shakespeare spectrum rather than testing them on a Marlowe-versus-everyone-else spectrum and then a Kyd-versus-everyone-else-spectrum. There are quite a few such questions that this book raises in the mind of the reader and does not answer. Kinney's conclusion is that, in collaboration with someone who was not Kyd or Marlowe, Shakespeare co-wrote *Arden of Feversham*, concentrating on the middle, around scenes 4–7 and maybe as far as scenes 8–9.

In chapter 5, about *Edmond Ironside* (pp. 110–15), Philip Palmer begins with a history of the play's reception, including E.B. Everitt's 1950s and Eric Sams's 1980s attributions of it to Shakespeare. Palmer tests *Edmond Ironside* against Shakespeare's plays and eighty-five other single-authored plays, using two new procedures described in chapter 7, reviewed below. (It would have been better if the tests and the methods of processing the results had all been laid out in advance, since to bring in new techniques halfway through the book, and without fully explaining them, enhances the non-specialist's sense that this work is incomprehensible computer 'magic' and raises the suspicion that the procedures were changed in the light of the intermediate outcomes.) The first new procedure is Discriminant Analysis of the frequencies of 200 function words in each 2,000-word segment under test. Palmer validates his test by taking out of the sample a play of known authorship and seeing whether the test assigns each of its 2,000-word segments to the correct author. His test was right 84 per cent of the time, which Palmer considers rather good, although it means that one time in six this test will be wrong. All seven 2,000-word segments of *Edmond Ironside* were deemed non-Shakespearian by this test. Turning to the lexical-word test that the book has already made much use of—in which the distinct Shakespearian and non-Shakespearian zones on graphs are by now familiar—all the *Edmond Ironside* segments fall on the non-Shakespearian side. So, by both tests Shakespeare is not a likely candidate.

Palmer takes the candidates Greene, John Lyly, Marlowe, and Peele, for whom substantial sole-authored play canons are already known, and for each he makes a candidate-versus-others graph—so, starting with words Greene favours more than others and words Greene neglects more than others—and plots where the *Edmond Ironside* segments fall on it. For each, *Edmond Ironside* is either firmly in the 'others' zone or in the overlap area where the zones are not distinct, so none of these four men is the author. To look beyond these four, into the authors whose known canons are small, Palmer switches methods and simply counts how many words in *Edmond Ironside* occur in other dramatists' plays, common words and proper nouns excluded. The dramatists are Lyly, Peele, Marlowe, Greene, Anthony Munday, Chettle, Thomas Lodge, Kyd, George Chapman, Nashe, and Shakespeare. Shakespeare's word usage comes out as particularly unlike that of the author of *Edmond Ironside*, but by Palmer's admission this test is not particularly convincing since Lyly comes out on top here even though the lexical-word test showed *Edmond Ironside* to be most unlike his writing. Palmer reports but does not graph his attempt at lexical-word tests for, in turn, Chapman, Thomas Heywood, Ben Jonson, and Kyd being the author of *Edmond Ironside* and in each case the play's segments fell into the 'others'

category. Thus all the candidates put forward by Sams, plus some more, are eliminated as possible authors of *Edmond Ironside*. Shakespeare did not write it and we do not know who did.

Chapter 6 (pp. 116–33) is by Thomas Irish Watt and is concerned with *Edward III*. Its opening remarks are somewhat confused, beginning correctly with the Stationers' Register entry of the play on 1 December 1595 but then going on 'In 1599, Burby entered a second quarto in the Register' (p. 116). Of course manuscripts, not printed books, were entered in the Stationers' Register and in any case there is no such entry in 1599. (It is bad form to give only a year for a Stationers' Register entry as the reader has to trawl the register to find it, or, as here, not find it.). This error is not a simple slip but a thorough confusion since Watt goes on to discuss these 'two entries in the Register' being 'the only evidence of performance on record' (p. 116). The Stationers' Register entry makes no reference to performance: it is the play's title page that tells us about performance 'about the Citie of London'. Further inaccuracy creeps into Watt's history of the play's reception, such as the claim that Shakespeare's fellow actors 'Heminge and Condell published the 1623 Folio' (p. 117). Something goes wrong with the referencing on page 120 as a quotation from Edmund King is supported by footnote 13 that reads '*Ibid.*, p. 9', pointing the reader back to a book by Edward Armstrong from 1946 cited in footnote 12, when in fact Watt means to point the reader back to footnote 5 where King's Master's degree dissertation is cited. Watt usefully points out that Eliot Slater's stylometric analysis of the play published in 1988 (and reissued by Cambridge University Press in 2009) uses a hopelessly flawed methodology.

Sections of the play that stand out as fairly unconnected to the rest are I.ii–II.ii, which shows Edward's failed attempt to woo the Countess of Salisbury, and III.i–IV.iii, which shows his campaign against the French; each contains around 6,500 words. Watt divides the twenty-seven single-authored Shakespeare plays into 6,000-word segments (why not 6,500?) and does the same for the eighty-five single-authored non-Shakespearean plays from 1580 to 1619. First he tries the function-word test, which does not produce highly distinguished zones on the Principal Component Analysis graph but nonetheless the I.ii–II.ii segment falls close to the Shakespearean zone and the III.i–IV.iii segment falls within the non-Shakespearean zone. Then comes this book's usual lexical-word test based on 500 words commoner in Shakespeare's segments than in the others' segments and vice versa, which for these 6,000-word segments produces a clear separation of the zones on the graph. Pleasingly, the I.ii–II.ii segment falls (just) inside the Shakespearean zone and the III.i–IV.iii segment falls (just) inside the non-Shakespearean zone. To validate this last test, Watt reruns it several times with one play removed from first the Shakespearean set and then the non-Shakespearean set, producing the new zones based on this slightly smaller dataset, and then tests the extracted play to see where the procedure would place it. Graphs show that the test consistently puts Shakespeare's *King John*, *1 Henry IV*, and *Henry V* in the Shakespearean zone and the non-Shakespearean plays *James IV*, *Edward I*, and *Edward II* in the non-Shakespearean zone.

So far Watt's tests have used plays from across several decades, and since there is evidence that dramatic language changed around 1600 he reruns all the tests with his dataset confined to pre-1600 plays. This produces zones that are a little more clearly defined for the function-word test and puts the two *Edward III* segments nearer the centre of their respective zones: I.ii–II.ii is more clearly Shakespearian, III.i–IV.iii more clearly non-Shakespearian. Under this new date restriction the lexical-word test still provides a clear separation of the zones for Shakespeare's plays and the non-Shakespearian plays, but although III.i–IV.iii falls within the non-Shakespearian zone, I.ii–II.i falls between the two zones. Replicating what Palmer did for *Edmond Ironside*, Watt tries one more test and simply counts how often the words in the *Edward III* segments appear in the segments by Shakespeare and the segments by each of fifteen other dramatists for whom we have two or more plays, but excluding common words, meaning those that appear in more than 40 per cent of the segments. This test shows that the unusual words in *Edward III* I.ii–II.i are words that appear more often in Shakespeare's plays than in anybody else's—as we would expect if he wrote this segment—and that the unusual words in *Edward III*, III.i–IV.iii are ones not favoured by Shakespeare but favoured by Marlowe (most strongly of all), then Peele, then Kyd. This gives Watt three new candidates for authorship of *Edward III* III.i–IV.iii, so he reruns his lexical-word tests, but rather than sorting the segments into two heaps of Shakespeare-versus-the-rest he uses Marlowe-versus-the-rest, Peele-versus-the-rest, and then Kyd-versus-the-rest. In all the tests, I.ii–II.i and III.i–IV.iii of *Edward III* fall into the zones of 'the rest' rather than Marlowe, or Peele, or Kyd. Watt's conclusion, then, is that Shakespeare wrote section I.ii–II.i of *Edward III* and not section III.i–IV.iii, whose author is unknown but is not Marlowe, Peele, or Kyd. (Actually, Watt does not make that last point explicitly but it must be the reasonable conclusion from his work since if he trusts his lexical-word tests in one part of his argument he must trust them throughout.)

Watt also wrote chapter 7, on *Sir Thomas More* (pp. 134–61), of which the Hand D section plus Addition III—More's soliloquy beginning 'It is in heaven that I am thus and thus'—add up to 1,214 words. Watt performs the book's standard lexical-word test by dividing twenty-seven Shakespeare plays and eighty-five non-Shakespeare plays into 1,200-word segments and finding the top 500 words used more by Shakespeare than by the others and the 500 words used less by Shakespeare than by the others. For each segment he plots the frequency with which it uses each of the words Shakespeare favoured and those he disliked, and the graph shows two fairly clear zones formed by the Shakespeare segments and the non-Shakespeare segments, with a little overlap. 'Hand D + Addition III' sits centrally within the Shakespeare zone. To validate the test, Watt takes one play at a time out of the procedure—in turn, Thomas Dekker's *The Shoemaker's Holiday*, Heywood's *If You Know Not Me*, Jonson's *Volpone*, Middleton's *The Phoenix*, Shakespeare's *Hamlet*, and John Webster's *The Duchess of Malfi*—recalculates the zones, and then checks where the removed play's segments fall on the graph: 90 per cent of the segments are correctly identified as Shakespearian or non-Shakespearian.

Watt then reruns the lexical-word test using not 1,200-word segments but the whole of each play, and this gives much better separation of the zones because in bigger samples the local variations cancel one another out. Here 'Hand D + Addition III' is much nearer the centre of the Shakespeare zone than the centre of the non-Shakespeare zone. Watt changes the comparison so that rather than Shakespeare-versus-others it is Dekker-versus-others, and this time 'Hand D + Addition III' is much closer to the others than to Dekker, so he did not write it. Repeating this procedure for each of them in turn, Watt establishes that it was not written by Heywood, Jonson, Middleton, or Webster. In function-word tests the 1,200-word segments are just too small for meaningful Principal Component Analysis, so Watt switches to Linear Discriminant Analysis, for the classificatory power of which he gives a short and not terribly helpful explanation in discursive prose. His explanation uses no analogies and the description is highly abstract, as for example when he explains the danger of over-training the test so that 'it struggles with new instances from the same class' (p. 153). The reader is left wondering what it can mean for a test to struggle.

Using the validation method he previously used for the lexical-word test, Watt counts the frequencies of the top 100 most author-specific function words in the set of 1,200-word segments from Shakespeare's *Hamlet* (called Group One) and, in turn, the sets of 1,200-word segments from each of Dekker's *The Shoemaker's Holiday*, Heywood's *If You Know Not Me*, Jonson's *Volpone*, Middleton's *The Phoenix*, Shakespeare's *Hamlet*, and Webster's *The Duchess of Malfi* (each called, in turn, Group Two) and uses Linear Discriminant Analysis to say whether the 1,200-word segment from 'Hand D + Addition III' belongs in Group One or Group Two. In every case the test shows that 'Hand D + Addition III' belongs in Group One, meaning it is Shakespearian. However, this test also wrongly indicates that two-thirds of *A Shoemaker's Holiday* were not written by Dekker. Rather than abandon the test, Watt decides that the validation 'underestimates the power of the method' (p. 153), which seems an odd way to describe a failure.

As a final test, Watt discards proper nouns, function words, numbers, and 'imprecations' from 'Hand D + Addition III', and then searches among the remaining words for those that appear in no more than 40 percent of the project's collection of 136 single-authored plays. There is an ambiguity here: did Watt look for words that appear in no more than fifty-four of the plays (40 per cent of 136) or in no more than 40 per cent of the 1,200-word segments from all the plays? Watt looks for these relatively rare words in plays from 1580 to 1619, and finds the highest set of matches with *Othello*, even after adjusting for the differing lengths of different dramatists' plays, since a long play has a better chance of matching with the rare-word set than a short one. Moreover of the top ten plays when listed in order of how many times they use these rare words, seven are by Shakespeare. Watt rather long-windedly (and confusingly) explains how he adjusted for the different sizes of the canons: how he 'correlated' (an ambiguous word in this context) the scores with the number of plays by each dramatist. After this adjustment, Shakespeare was still the front-runner for rare-word links with 'Hand D + Addition III'. Watt then runs the test in reverse, looking for the relatively rare words absent from

'Hand D + Addition III'. Here we would expect Shakespeare to be the lowest scorer if he were the author of 'Hand D + Addition III', and in the event he is the second lowest, with Jonson as the lowest, which shows that Jonson generally avoids the words that 'Hand D + Addition III' avoids.

Thus we can be reasonably sure that 'Hand D + Addition III' is by Shakespeare. To date the writing, Watt divides the 1,200-word Shakespeare play segments into two classes: up to 1599, and 1600 onwards, and repeats his tests but treating these two sets as though they were the work of different dramatists. In the lexical-word tests the two classes form fairly distinct zones on the graph, and 'Hand D + Addition III' sits on the edge of the 1600 onwards zone. How come, Watt then asks, 'Hand D + Addition III' fails two of Ward E.Y. Elliott and Robert J. Valenza's tests for Shakespearian authorship? As Jackson pointed out, Elliott and Valenza's test using word and sentence length can be thrown off by a manuscript source, and their function-word test used samples of writing that were just too small: they compared only the words common in *Macbeth* and uncommon in Middleton's *The Witch*, and vice versa, to create their list of Shakespeare-favours and Shakespeare-neglects markers. Watt reruns Elliott and Valenza's function-word test with certain modifications whose effects are hard to predict, such as including prose as well as verse, and finds that 'Hand D + Addition III' no longer fails it. Watt's conclusion is that, as other studies have shown, Shakespeare composed the 'Hand D + Addition III' material after 1600, and since it seems not to be scribal he presumably did the handwriting of Hand D too.

Chapter 8 (pp. 162–80), by Craig, is about the additions to Kyd's *The Spanish Tragedy* that first appeared in the 1602 edition. Addition One is fifty-four lines, Addition Two is ten lines (replacing two lines), Addition Three is forty-eight lines, Addition Four is 169 lines, and Addition Five is forty-eight lines incorporating some existing lines. We do not know who wrote them, but Philip Henslowe's Diary records two payments to Jonson for making additions to the play, on 25 September 1601 and 22 June 1602. However, Addition Four is parodied in John Marston's *Antonio and Mellida*, written in 1599, so presumably it was already in performance then and hence cannot be what Jonson was paid for in 1601–2. Jonson's own *Cynthia's Revels* (first performed 1600) also implicitly alludes to revision in *The Spanish Tragedy* by referring to 'the old Hieronymo (as it was first acted)', again before Jonson was paid by Henslowe. *The Spanish Tragedy* is recorded by Henslowe as 'ne[w]' in 1597, so maybe the additions that appeared in the 1602 edition were already written by 1597 and Jonson's additions were something else now lost.

Craig decides to use Jackson's method of looking for collocations in Literature Online (LION), but for an unexplained reason he uses Chadwyck-Healey's Verse Drama CD-ROM, part of the original basis for LION, rather than LION itself. This is bound to skew the results by disregarding all prose drama. At page 170, n. 41, Craig admits to missing the word *unsquared* in Marston's *What You Will* because it is not in the Verse Drama CD-ROM, but he caught it because *OED* mentions it. How many others did he miss because *OED* does not mention them? This is poor methodology. Craig starts with the additions' phrases that Warren Stevenson

decided were rare when arguing for Shakespeare's authorship. Some of the phrases turn out to be so common as to be useless, but the collocation *things/called/whips* occurs only in *2 Henry VI* and Addition Three, *pry/crevice/wall* only in *Titus Andronicus* and Addition Four, *hand/lean/head* only in *The Rape of Lucrece* and Addition Four, and *brow/jutty* only in *Henry V* and Addition Four. Other collocations might, however, be conscious reworkings of famous lines from other plays (as with Pistol's speeches in *2 Henry IV*), and Craig mentions other of the additions' links with Shakespeare that are not unique but rather rare. Having dealt with the rare words/phrases that Stevenson found, Craig finds his own: 'as massy as' occurs only in *Much Ado About Nothing* and Addition Four, *[un]delve/mine* only in *Hamlet* and Addition Four, and there is one further rare but not unique link.

Then comes a fresh approach. Craig takes 136 confidently sole-authored plays from 1576 to 1642 by thirty-five dramatists, and to match the 2,663 words in the additions he divides these plays into 2,500-word segments. The first test is of frequencies of function words and Discriminant Analysis is able to assign segments to their correct authors for 98.9 per cent of the segments. The procedure is not clearly described here, and at one point it is implied that the classification into author groups was made by hand for the software to work on: 'Each author's segments were assigned to a group...' (p. 172). But this would render meaningless the claim that nearly all the segments 'were assigned to the correct author' by the software (p. 172). The Discriminant Analysis is then asked to assign the additions to an author group, and it chooses Shakespeare. To validate the test Craig takes all the segments for one play at a time out of the dataset, reruns the test, and then asks the Discriminant Analysis to assign these (known-author) play segments to one of the thirty-five authors: 93 per cent of the Shakespeare segments are correctly identified as being by Shakespeare (so 7 per cent of them are wrongly ascribed to someone else) and 86 per cent of the non-Shakespearean segments are correctly identified as not being by Shakespeare (so 14 per cent of them are wrongly ascribed to Shakespeare). Craig seems to think that these are good results, but it means that about one time in seven the test will say something is by Shakespeare when it is not.

Next comes the by now familiar turn to the lexical-word tests. Since critics have settled on four frontrunner candidates for the author of the additions—Jonson, Shakespeare, Dekker, and Webster—Craig puts them head to head, starting with Shakespeare-versus-Jonson and using 2,500-word segments. On a graph whose x-axis shows the segment's use of words Shakespeare favours and Jonson neglects and whose y-axis shows the segment's use of words Jonson favours and Shakespeare neglects, the segments fall into two distinct zones, one for each author. The additions are well within the Shakespeare zone. Doing the same test for Shakespeare-versus-Dekker also produces well-defined zones, and although this time the additions are not within the Shakespeare zone they are a lot nearer to its centre than they are to Dekker's zone's centre. Shakespeare-versus-Webster has well-defined zones, and the additions fall on the edge of the Shakespeare zone, a long way from the Webster zone. And so on for Lyly, Marlowe, Greene, Peele, Heywood, Fletcher, and Middleton: on this test (that is, each man versus Shakespeare) the additions always come out

nearer the Shakespeare centre than the rival's centre. Craig tries just testing Addition Four, comprising nearly 1,500 words, but when the test is validated—by taking away a segment, recalculating the zones, and then seeing where this isolated segment falls on the new graph—a lot of the segments are wrongly attributed, so he has little faith in this test. Craig admits that when he ran the kind of test used in previous chapters, a lexical-word test of Shakespeare-versus-all-the-others-at-once, the additions came out as being not by Shakespeare, but mentions that when he confined this test to plays first performed up to 1602 they came out as Shakespeare again. His conclusion is that the additions to *The Spanish Tragedy* are most probably by Shakespeare, and if not by him then Jonson is a particularly unlikely alternative.

The final chapter, by Kinney, is concerned with the revision of Shakespeare's *King Lear* (pp. 181–201). Without explanation, Kinney gives the date of publication of Q1 *King Lear* as 1607–8. Blayney reckons printing started in the middle of December 1607 and was finished in the middle of January 1608, and all surviving exemplars are dated 1608, so it is not clear what Kinney means by his date of 1607–8. Kinney gives an extended account of the textual condition of Q1 and Folio *King Lear*, the former lacking 100 lines that are in the latter and the latter lacking 300 lines that are in the former, with 1,000 words variant, and he gives a history of the explanations for these differences. Kinney wrongly credits the collaboratively written *Textual Companion* to the *Oxford Complete Works* for explaining how F came to have press-variant errors from Q1 despite being set from Q2 in which those errors do not appear. The credit rightly belongs to Taylor alone, appearing in his contribution to the collection called *The Division of the Kingdoms* [1983]. Another curiosity is a garbled sentence about 'leaves written in the margins of pages of the original manuscript' (p. 185), which I can make no sense of. Kinney also makes the common error of referring to a 'heath' (p. 189) in *King Lear* although the word appears nowhere in the play.

Q and F show small differences that seem to reflect a consistent set of changes: *which* becomes *that*, *doth* becomes *does*, *these* becomes *this/those*, and *thine* becomes *thy*. While Kinney is right that random corruption cannot do this, he is wrong to imply that the only alternative is artistic revision since scribes also imposed their preferences to this extent. Kinney offers bar charts about uses of *which*, *doth*, *these*, and *thine*, but it is not at all clear how they relate to his claim of substitution of one word in Q with another in F, since the charts show only proportions and ratios of these words in each act. There are passages amounting to 902 words present in F and absent from Q, so using this book's function-word test Kinney pitches Shakespeare against Fletcher using 900-word segments from their reliably sole-authored works. The resulting graph shows reasonably distinct zones, with the F-only passages of *King Lear* falling squarely in the Shakespeare zone. Using the same segments, Kinney performs the lexical-word test and again produces distinct zones, but this time the F-only passages of *King Lear* fall on the border of the Shakespeare zone, just where it meets the Fletcher zone. According to Kinney these two tests rule out Fletcher as a candidate for composition of the F-only passages of *King Lear*.

Kinney repeats the test by pitching Shakespeare against, in turn, Chapman, Jonson, Philip Massinger, Middleton, and Webster. Rather than give the graphs, Kinney uses a table that summarizes the 'distance' that the F-only passages lie from the centres of the two zones. (There is a linguistic slip here: the final column of the table is supposed to show the 'difference between' numbers in two preceding columns, but this final column's numbers are themselves all negative; by definition a difference cannot be negative.) It is clear from this table that the author zones for these comparisons are not so clearly defined as in previous tests, yet in every case the F-only *King Lear* passages are nearer the centre of the Shakespeare zone than the centre of the other author's zone. The chapter's conclusion is that Shakespeare was the reviser of *King Lear*. The book ends with a general conclusion by Kinney: Shakespeare collaborated more than we used to think and we must add 2 *Henry VI*, *Arden of Faversham*, and *The Spanish Tragedy* to the previously known list of *1 Henry VI*, *Edward III*, *Sir Thomas More*, *Titus Andronicus*, *Timon of Athens*, *Henry VIII*, *The Two Noble Kinsmen*, *Pericles*, and (posthumously) *Macbeth* and *Measure for Measure*. One of his collaborators, on *1 Henry VI*, was Marlowe. Notwithstanding this reviewer's objections in passing, this book is a most impressive achievement of scrupulous scholarship whose conclusions represent the current state of knowledge.

The fourth and last of this year's monographs is Lukas Erne's *Shakespeare's Modern Collaborators*, which argues that the editing of Shakespeare is necessary and enabling. In his introduction (pp. 1–11), Erne explains why unediting is impossible and why we need to improve on previous editing, not abandon it. The uneditors' argument that we should return to the early editions overlooks the fact that we cannot apprehend them as their first readers did since 'What was modern spelling for Shakespeare's contemporaries seems unfamiliar to us' (p. 6). Erne does not mention it, but this is essentially the problem faced by investigators of original staging: we cannot become the early modern audience who saw doublet and hose as modern dress. The problems have the same solution: we can somewhat train ourselves to think like them. Chapter 1, on 'Establishing the Text' (pp. 13–42), is concerned with the necessity of modernizing spelling and punctuation, and the dangers of emendation, including of lineation. Margreta de Grazia and Peter Sallibrass pointed out that returning to the early texts' fruitfully ambiguous spellings does not really restore the ambiguities that early moderns enjoyed because modern readers have internalized the distinctions: we cannot hear both *human* and *humane* in the early modern spelling *humane*. (I am not sure this is true; surely Erne would be unable to make this point, would be incomprehensible, if we had entirely lost the capacity to hear both senses.) Erne makes the surprising assertion that 'none of Shakespeare's playbooks published during his lifetime contained any act or scene divisions' (p. 35). In his edition of *Romeo and Juliet* for the New Cambridge Shakespeare Early Quartos series, Erne pointed out that the 1597 quarto has decorative bars 'inserted between scenes or scenic movements' (p. 39), and it is not clear what has changed his mind about this.

Chapter 2, on 'Framing the Text' (pp. 42–58), discusses editorial provision of collations and introductions, but largely neglects the problems of providing



explanatory notes. Erne offers an interesting discussion of the rethinking of the plays' order of composition that must have occurred between publication of the *Oxford Complete Works*' first edition in 1986 and the second edition of 2005, the latter shuffling a handful of plays to new positions. In fact, the years of composition assigned by the *Oxford Complete Works* editors did not change much, but rather where a single year contained more than one play they rethought the order within that year. Discussion of chronology in the edition's *Textual Companion* indicates that they were seldom confident about precedence within a single year. Only once did they shift a play by more than one year, with *All's Well that Ends Well* moving from 1604–5 (in the 1986 first edition) to 1606–7 (in the 2005 second edition). In Chapter 3, on 'Editing Stage Action' (pp. 59–85), Erne gives examples of indeterminate and absent, yet necessary, stage directions in the early editions, and surveys the arguments—by, amongst others, Stanley Wells, M.J. Kidnie, A.R. Braunmuller, and John D. Cox—over the extent to which an editor should intervene to resolve ambiguities. He finds Wells mistaken in asserting that editors should undoubtedly help readers to imagine the original performances. Why not, Erne asks, help readers to imagine the action in its fictional locations, and so respect the differences between a book and a performance? He acknowledges that early editions' stage directions refer explicitly to doors even where there should be none (as in a forest) and to things happening on 'the stage'; that is, they refer to the theatre fabric rather than the fictional locations. But they also refer, he points out, to fictional places such as 'the walls' of a city, 'a window', 'the grave', 'the cave', and so on. It would be reasonable, he decides, for an editor expanding the stage directions of her play to include fictional rather than theatrical ones.

Almost all of the first half of Erne's final chapter, on 'Editing the Real *Lear*' (pp. 87–102), is about how the play ends differently in Q1 and F, and the second half is concerned with the ways in which editions have chosen to respond to the Q/F differences. He makes the valid point that the editors of the *Oxford Complete Works* of 1986 claimed, shortly after its publication, that they regretted not splitting *Hamlet* as they had split *King Lear*, and yet they did not do so when they had the chance in the second edition of 2005, which added *Edward III* to the canon and represented all of *Sir Thomas More* where formerly they gave only Shakespeare's contributions. In fairness, we should remember that their hands might have been tied, since the publisher could likely countenance the extra expense of adding two new plays to the edition because it enhanced the book's attractiveness to readers, while splitting *Hamlet* would likely be perceived as harming its appeal. In his conclusion (pp. 103–4) Erne looks forward to more editions produced along fresh editorial lines, and in particular the splitting of not only *King Lear* and *Hamlet* but also *Othello*, *Troilus and Cressida*, *2 Henry IV*, *Henry V*, and *Romeo and Juliet*.

Two monographs published in 2009 had individual chapters that fall within this review's purview. The first 140 pages of Margaret Jane Kidnie's *Shakespeare and the Problem of Adaptation* are about various theatre and screen adaptations of Shakespeare. Kidnie describes the invitation of the BBC (the United Kingdom's state-run television broadcaster) that viewers

'press the red button' on their handsets in order to enter into an interactive relationship with broadcasts such as adaptations of Shakespeare. As Kidnie rightly points out, 'For those with analogue television, the invitation to press a non-existent red button on their remote controls seemed to summon up the promise of another world as inaccessible to their eyes as the fairy world of *Dream* is to the eyes of the young lovers' (p. 128). In fact this was not the only disappointment the invitation generated. Thousands of complaints were received from viewers who reported that pressing the red button made their television sets stop working altogether: on older television handsets the power button is red.

The last of Kidnie's chapters is on 'Textual Origins' (pp. 140–64) and begins with a sketch of the present textual situation of Shakespeare, with some editors constructing elaborate hypotheses about what happened to a play before it got printed—Taylor and Jowett on *Measure for Measure* is her archetype—and others trying to avoid doing that. She describes the New Folger Library Shakespeare editions edited by Barbara Mowat and Paul Werstine as ones that chose 'not to write or otherwise rely on textual histories that seek to recover from early printed texts, manuscript authorities' (p. 155), and approves of 'Mowat's and Werstine's refusal to make editorial choices on the basis of what one believes can be said about the manuscript(s) that provided copy for an extant printed text' (p. 156). Yet, as Kidnie acknowledges, the choice to base a New Folger Library Shakespeare on a substantive early edition (rather than a derivative one) indicates that Mowat and Werstine must have some sense of the *work* as distinct from its manifestation in particular *documents*, since they treat the early documents as not all equal. What else could they be measuring them against except some notion of the disembodied *work*? Actually, Mowat and Werstine's position is even more incoherent than that, since by their definition a substantive edition is one printed from a manuscript rather than an existing book. Contrary to their protestations of having nothing to do with the editorial divination of underlying manuscript copy, they engage in it at least as far as the determination that the copy *was* manuscript rather than print.

Kidnie concludes with the presentist observation that all editions serve the market for which they are created and are products of their times. Editions 'are not authoritative in and of themselves, but have authority conferred on them through reference to certain culturally accepted criteria' (p. 162), and hence we are free to do what we like in editing. We can, if we choose, 'release editorial practice from imperatives to represent in new editions of the works reconstructions of the past' (p. 164) and instead promote 'culturally engaged editorial practices'. Let us not, Kidnie argues, be driven just by a historicist impulse to be faithful to the past. Her closing sentence sums up what she sees as the new possibilities. 'In short, to resist the dominant inclination to regard past histories as foundational to editorial labour would be to insist on the realization that textual, no less than theatrical, efforts to recover "what happened" can only be pursued alongside efforts to shape "what is happening" in terms of work recognition and the ever-shifting boundaries that separate work from adaptation' (p. 164). What has dropped out of the equation here is the editorial duty to represent the author's intentions, and

Kidnie's whole book is an argument that we should not worry too much about that. Such an argument cuts both ways, since any writer who feels free to ignore another writer's intentions can hardly complain if her own receive the same treatment. If I have misrepresented Kidnie's thinking, her own logic would deny her grounds to object.

The first 130 pages of Paul Eggert's *Securing the Past: Conservation in Art, Architecture and Literature* are about historic buildings and paintings and so not relevant here. Chapter 7 (pp. 131–53) is called 'Materialist, Performance or Literary Shakespeare?' and considers Erne's recent arguments (reviewed in *YWES* 83[2004] and 84[2005], covering work published in 2002 and 2003) that Shakespeare wrote for a market of readers as well as for the stage. We already knew from reader-response theory that reading is a kind of performance in the head—novels are not quite so unlike plays—so why, asks Eggert, should Erne's ideas so greatly surprise us? Eggert thinks that the *Oxford Complete Works*' two *King Lear*s came about partly through the success of post-structuralist thinking. (I would have said they came out of a purely empiricist approach and happened to arrive by a circuitous route at a destination to which post-structuralism took a short cut.) Eggert conflates the move to stage-centredness with the post-structuralist turn, and critiques the materialist Shakespeare movement. In it he perceives evasive uses of the word *text* to sometimes mean the mental object, sometimes the physical object, and sometimes both, and he decries the movement's futile attempts to magic away agency. 'Objects *point* [at someone]' (p. 146), he writes. Eggert's own solution to the quandaries arising from Erne's work is that we should edit separately for the performer and the reader. For the former, the editor's model of agency would include all those involved in the original performances, and all that they acted would be included and what they cut would be excluded. In editions for readers, on the other hand, all that got written by Shakespeare (but no one else) would be included.

At least two, and possibly three, essays from book-format collections were relevant this year, but only two will be noticed. The University of Virginia Press was unable to supply a review copy of Stephen Burt and Nick Halpern's collection *Something Understood: Essays and Poetry for Helen Vendler*, which contains William Fleisch's essay 'The Bounds of the Incidental: Shakespeare's View of Accuracy'; it will be noticed next year if it turns out to be relevant. In the first of the two essays that could be examined, Richard Dutton's '*The Famous Victories* and the 1600 Quarto of *Henry V*' (in Ostovich et al., eds., *Locating the Queen's Men, 1583–1603: Material Practices and Conditions of Playing* (pp. 133–144)), the author argues that the 1600 quarto of *Henry V* is not a cut-down version of the play better seen in the Folio, but rather is a version first performed in the late 1590s. Dutton first made this argument, from different evidence, in an article reviewed in *YWES* 86[2007], covering work published in 2005. Here he explores the play's debt to *The Famous Victories of Henry the Fifth* (published 1598), which is extensive in the quarto. None of the Folio *Henry V* material that is absent from the quarto draws on *Famous Victories*, and hence, unless the process that cut down the manuscript underlying the Folio text to make the manuscript underlying the quarto managed somehow to cut only material not in *Famous Victories*, we have to

conclude that, as Dutton maintains, the situation was reversed. That is, the manuscript underlying the quarto (indebted to *Famous Victories*) must have been enlarged to make the manuscript underlying the Folio, and by addition of material not indebted to *Famous Victories*. Dutton addresses James Bednarz's argument (reviewed in *YWES* 87[2008], covering work published in 2006) that the *Henry V* Chorus's reference to conveying the audience across the English Channel is mocked in Jonson's *Every Man Out of His Humour* (first performed 1599) and hence was in the original performances of *Henry V*. Dutton considers the verbal and conceptual link between *Henry V* and *Every Man Out of His Humour* to be tenuous in the latter's quartos of 1600, becoming real mockery only in the 1616 Folio version, which reflects extensive authorial revision.

The authorial revision of the quarto version of *Henry V* to make the Folio version Dutton dates to 1602. Q makes no mention of Ireland, F makes several, and in 1602 Ireland was safely back under English control. Dutton details just how closely the quarto follows, action by action, the events in scenes 9–20 of *Famous Victories*, and there are a number of close verbal parallels. A particularly telling point is that, compared to the quarto, 'F loses Clarence and Warwick, replacing them with the earls of Westmorland, Salisbury and Shrewsbury (Talbot), seemingly concentrating on warriors already made famous in *1 Henry VI*—possibly anticipating the Epilogue's reminder of what would follow from all this' (p. 140, n. 16). Dutton accepts Scott McMillin and Sally-Beth MacLean's theory that the text for *Famous Victories* came from a good, non-piratical, memorial reconstruction by the actors dictating their lines to a rather inaccurate scribe. This they did to produce a script of a revised version of the play, cut for fewer actors, and of course Andrew Gurr also claims aural transmission for quarto *Henry V*. There are some tangles in *Henry V* that might be explained by the existence of a now lost even earlier version of the play in which Falstaff is alive, since in the play that we have Pistol seems married to Doll Tearsheet (Falstaff's whore) and describes himself as old, a word more suitable in Falstaff's mouth. Revision to remove Falstaff from such a lost early version of *Henry V*, in order to make the play we have in Q, might have occasioned collective dictation to a scribe.

*Famous Victories* was published in 1598, having been entered in the Stationers' Register on 14 May 1594, presumably because the international situation near the end of the century was much like the situation just before the Armada of 1588, when *Famous Victories* was first performed as a patriotic confidence booster. An uncomplicatedly patriotic *Henry V*, as we find in the 1600 quarto, would be an appropriate response by Shakespeare to such a situation a decade later. That *Famous Victories* and *Henry V* were felt to be competitive texts would explain why Thomas Creede, who had the rights to the former, printed the latter: he compelled *Henry V*'s owners, Thomas Millington and John Busby, to pay him to print *Henry V* because he had the rights to all stories about Prince Hal making good and conquering France. This might also explain why *Henry V* was printed in 1600 (and indeed twice reprinted, 1602 and 1619) without Shakespeare's name on the title page: Creede was marking that Shakespeare did not really deserve credit for the story. Dutton does not directly address the problem of the Bishop's speech

being mangled in the 1600 quarto so that he refers to 'King *Pippins* title' and 'King *Charles* his satisfaction' (A2v) having not spoken the Folio-only lines that ought to precede these allusions in order for them to make sense. Part of the traditional argument for Q representing a cut-down version of the play underlying F is that no one intentionally writes meaningless snippets of an allusion, so MSQ > MSF makes no sense while MSF > MSQ is perfectly plausible as botched reduction. However, this evidence could also be accommodated within the aural theory of transmission that Dutton accepts: the antecedents were dropped by the actor forgetting his lines or the scribe failing to capture them.

The other book chapter of certain relevance this year is Margreta de Grazia's argument that John Benson's 1640 edition of Shakespeare's *Poems* is not as bad as is frequently claimed: 'The First Reader of *Shakespeare's Sonnets*' (in Barkan et al., eds., *The Forms of Renaissance Thought: New Essays in Literature and Culture*, pp. 86–106). De Grazia sees it as not so much a pirating of the 1609 *Sonnets* as a reading of it, and modelled on the Folio albeit in octavo format. Putting into his collection the non-Shakespearian matter that appeared under Shakespeare's name in the 1612 edition of *The Passionate Pilgrim*, Benson was simply misled by its title page rather than wilfully dishonest. De Grazia insists that the 1640 book is not an edition of *Sonnets* because 'In order to have an edition, an editor is needed' (p. 89). She seems aware that this is not the usual meaning of the word edition but she does not retract the claim. The reason Benson's edition was not a piracy is that the rights to *Sonnets* seem to have been worthless: no one had reprinted it in thirty years, and by 1640 the sonnet form was well out of date. Benson's bundling of *Sonnets* with poems from *The Passionate Pilgrim* and his giving them titles were attempts to demystify and organize the miscellany. Stanley Wells and Paul Edmondson have a low opinion of Benson, but their grouping of the sonnets in a monograph published in 2004 matches Benson's in a number of ways. Giving the sonnets descriptive titles was nothing new: manuscript copyists, and owners, did that all the time, and like Benson they assumed a heterosexual norm. Also, Benson's titles are, according to De Grazia, pretty good. Readers in any case knew such titles to be provisional, and sometimes crossed them out and wrote in their own. The reproduction sonnets are heavily indebted to Erasmus's epistle on marriage, and there is a kind of self-conscious invitation to textual reproduction in Shakespeare making his verse so easy to quote in a commonplace book, as indeed happened. Contrary to the impression created by the Variorum editor Hyder Edward Rollins, Benson was a responsible man who published other poetry and was perhaps the first to introduce the innovation of numbering lines in vernacular works, as if they were classics. He also introduced emendations to the sonnets that we still use. We used to think that Thomas Thorpe himself, publisher of the 1609 *Sonnets*, was a rogue. But whether or not he had Shakespeare's permission to print the book, he presumably was responsible for its structure, which we now admire.

Of the journal articles this year, much the most surprising title was Paul Werstine's 'The Continuing Importance of New Bibliographical Method' (*ShS* 62[2009] 30–45). Werstine argues that Greg's characterization of dramatists' authorial foul papers as necessarily messy came largely from his

misinterpretation of the evidence of scribe Edward Knight's transcript of Fletcher's play *Bonduca*. Greg thought that this transcript differs from the text of the play printed in the 1647 Beaumont and Fletcher Folio wherever Knight could not read the foul papers (because they were messy), but in fact, Werstine proves, Knight was inclined to capricious departures from his source, exacerbated in this case by a desire to avoid making corrections in what was to be a presentation copy. Greg somehow failed to follow his own New Bibliographical procedures in studying *Bonduca*.

In his undated transcript, Knight uses the term 'fowle papers' to describe what he was copying and explains why some scenes in the fifth act were 'wanting'; presumably he recalled seeing them performed, else how would he know they were missing? In an essay not published until 1990, Greg explored what Knight meant by foul papers by studying the variants between the manuscript and the Folio, which latter has the missing scenes. Greg found twenty-two gaps in the manuscript where Knight left a space and F has meaningful words or phrases, and he diagnosed Knight's inability to read the foul paper and his scrupulous avoidance of misreading. Thus according to Greg, Knight's transcript was the best copy that could be made of the foul papers. Greg had already decided that the missing scenes were contained on two folded sheets (eight pages) of the foul papers that became detached from the rest before Knight made his transcript, and noticed that in the transcript some lines or part-lines were displaced from the F location, frequently to the damage of metre. This Greg attributed to the lines being additions to the foul papers that were awkwardly placed and so misled Knight about where they belonged, which led Greg to his idea that foul papers were the dramatist's final draft, too untidy to be used to run a play. Greg thought that in copying out his own foul papers to make the basis for the promptbook, Fletcher introduced a final layer of revision detectable in the transcript/F variants, and so Greg anticipated Honigmann's *The Stability of Shakespeare's Text*.

For his Malone Society reprint of *Bonduca* Greg changed his mind and reattributed a number of transcript/F variants—differences of wording and of placing of lines—to failures by Knight to read the foul papers correctly rather than authorial revision. What Greg should have noticed, Werstine argues, is that the Knight transcript is not obviously 'wanting' anything at all and indeed is more coherent than the version of the play in F. The additional two-and-a-half scenes in F that come before the ending—as it appears in the transcript and in F—make that ending nonsensical because two characters who are supposed to be pinned down in a cave are, in the first of these additions, seen to leave it. Thus the additions are not by Fletcher, who would hardly butcher his own play's logic. Someone other than Fletcher was able to copy the author's foul papers, add the additional material, and so make the book that Knight calls the one 'where by it was first Acted from', which supplied copy for the Folio. Greg must have been wrong to think that the author's foul papers were illegible to anyone but the author, and Werstine proposes that in fact 'foul papers' meant simply any document from which a fair copy was made.

His transcript of Nathan Field, Massinger, and Fletcher's *The Honest Man's Fortune* (for which we also have the Folio text) shows that Knight was prone

to eye-skip that made him miss out whole lines. When he realized what he had done he corrected his writing with crossings out and insertions, but because his *Bonduca* transcript was to be a presentation copy to an important person Knight was much less keen to make visible corrections. When he realized that eye-skip had made him miss out whole lines, he crossed nothing out but simply inserted the omitted lines further down the same page, at the point where he realized his error. This, and not the difficulty of his copy as Greg maintained, is the reason for transposed lines in the *Bonduca* transcript. Also, we know from his work on *The Honest Man's Fortune* that Knight was capable of dropping whole lines even where there is no reason for eye-skip. Perhaps, reasons Werstine, Knight's gaps in the transcript do show that where he could not read Fletcher's foul papers he left a space to be filled later, but in some cases it is clear that he later filled such spaces with words of his own invention. At this point something goes wrong in the typesetting of Werstine's article, where it reads 'It is clear that the last word "troopes" has been written in later—first, because it angles up toward the right, while the other words in the line tend to angle down, and, second, because the initial t of "troopes" is italic, unlike the secretary's found earlier in the line in "the" and "through"' (p. 40). There is no secretary's in the words *the* and *through*, and Werstine confirms in private communication that a symbol in his typescript representing the secretary *t* was garbled in typesetting.

One of the written-in-later words is *troopes*, and the Folio reading is *Carts*, which is the better word in this context. Werstine reports from LION that Fletcher's phrase *armed carts* is 'not recorded as appearing before *Bonduca*' (p. 41) and hence Knight would not have known it; thus the *troops/carts* variant shows Knight substituting a commonplace phrase for an unusual one. Here Werstine is mistaken, as LION shows that George Puttenham in *The Art of English Poesy* [1589] referred to the Nubians' use of *armed cartes*, and EEBO-TCP shows that Richard Knowles in *The General History of the Turks* [1603] also used the phrase in a description of machines of war; it was not so unusual a phrase and did not originate with Fletcher's account of the ancient Britons' chariots. Another substitution is *trac't* interlined over a boxed *trasht*, where in fact *trasht* (meaning encumbered) is right and is in F, again showing Knight overruling his copy where it has a word he does not know. Werstine shows that quite a few of the gaps and written-in-later words in the transcript appear where the Folio reading is an unusual word, so Knight's not knowing, or not liking, the new word is the best explanation for the transcript/Folio variant at that point.

Indeed, Knight seems to have interfered even when Fletcher's word was not unusual. He was just an interfering scribe, so Greg was wrong to see the transcript/F differences as essentially a matter of the foul papers (copied to make the transcript) being hard to read. According to Werstine, Fredson Bowers's characterization of these matters was more accurate and he was right to assert that authors submitted fair copy to the players. The only Shakespeare play in which Knight's hand is detectable is the 1634 quarto of *The Two Noble Kinsmen* with his production notes in it. But if a scribe like Knight could interfere as much as he did in *Bonduca*, producing variant readings that fooled Greg into thinking they were signs of authorial revision, then we should not

assume that the three *Hamlets*, two *Othellos*, and two *King Lears* are the result of authorial alteration. The scenes added to the fifth act of *Bonduca*—absent from the transcript based on foul papers and present in F—ruin the play artistically, but were essential because Fletcher wrote only 167 lines for this final act. Theatrical adaptation, then, might not be a polishing for the stage. Werstine finds that *Bonduca* challenges Gurr's maximal/minimal text theory since the shorter version of it is authorial and the longer is theatrical. Knight shows concern that his transcript should reflect what got acted and the 1647 Folio shows that concern too (since it is the acted version), so Gurr's idea that printed playtexts do not reflect performed versions is probably wrong. The term foul papers is certainly pejorative, but is essentially relational: when papers were used as the basis of a transcript they were called foul papers no matter how clean they were. Thus Greg's own New Bibliographical methods—the means for finding the agents of textual alteration—reveal his error about the foul papers of *Bonduca* and about foul papers generally.

Werstine's article appeared in the same volume of *Shakespeare Survey* as two others of relevance here. In the first, 'The Popularity of Shakespeare in Print' (*ShS* 62[2009] 12–29), Lukas Erne undertakes a series of counts that show that in his lifetime Shakespeare's books were much more popular than other playwrights' books. To start considering how big a deal Shakespeare was in the publishing world of his day, Erne approximates that 300 titles were published in the year 1600, one-third of them on religion. About another third, around 100, are on what we would call literature, and these include the first editions of Shakespeare's *Henry V*, *Much Ado About Nothing*, *2 Henry IV*, *A Midsummer Night's Dream*, and *The Merchant of Venice* and the second editions of *The Contention of York and Lancaster* and *Richard Duke of York*, and the fourth and fifth editions of *The Rape of Lucrece*. Moreover, Shakespeare's words were excerpted in three collections, making twelve books in all, or 4 per cent of the entire book market. The highly popular *Venus and Adonis* (ten editions in his lifetime) might make us think, as Roger Chartier and Peter Stallybrass did (in an essay reviewed in *YWES* 88[2009], covering work published in 2007), that in the book world Shakespeare was known as a poet not a playwright, but it is worth remembering that *Sonnets* [1609] did not get a second edition until 1640. Also, because more of his plays were published than his poems, there were more editions: forty-five play editions in his lifetime (twenty-six of them naming him on the title page), and only twenty poetry editions.

What, then, was Shakespeare's reputation in his time, compared to other dramatists? Erne starts counting editions as an index of popularity, treating co-authored plays as one hit for each of the dramatists involved and counting each collection as one investment by a publisher, not as one-hit-per-play. From the beginnings of play printing to the closure of 1642 Shakespeare had seventy-three editions, way out in front of the next most published writer, Heywood, with forty-nine, and more than three times as many as Jonson with twenty-two. The picture is the same if we take the endpoint of 1660 instead of 1642: Shakespeare out in front, Heywood next, Jonson way behind. A switch to counting each play in a collection separately (so the 1623 Folio counts thirty-six times) makes Shakespeare's lead over his rivals increase



still further: up to 1642 he had three times as many, up to 1660 twice as many, as anyone else.

Turning to reprints—so, capturing not what publishers predicted would sell but what actually did sell out and had to be reprinted—Blayney's figure of around 50 per cent of plays published 1583–1622 getting reprinted inside twenty-five years is pertinent, because within these limits the reprint rate for Shakespeare was 85 per cent. (The utility of the endpoint being 1622 is that it excludes an unexplained drop in reprints in the Caroline period identified by Alan B. Farmer and Zachary Lesser.) Erne compares that reprint rate to each of a number of Shakespeare's contemporary and successor dramatists, none of whom, he discovers, was anything like as popular in print. The closest to Shakespeare in reprint rates are Marlowe, Webster, Beaumont, and Fletcher. Slicing the data another way, Erne looks at the average number of reprints per play (as opposed to just asking if any reprinting happened) and tabulates them by author. The table is headed by Beaumont at two reprints per play, then Shakespeare (1.6), then Fletcher (1.45), Marlowe (1.4), then a big drop to the next writer Heywood (0.9). But Erne worries that writers with small oeuvres are distorting this table, so he recalculates it for writers with at least ten plays to their name. This puts Shakespeare back on top and with a reprint rate three or four times that of Middleton, Jonson, and Dekker. If we now require that the reprints being measured had to happen within ten years of the first edition, Shakespeare streaks ahead even further.

So Shakespeare was wildly more popular with readers than Jonson was, although Erne concedes that perhaps Jonson was more popular with other writers, as their frequent allusions to him and his work suggest. Erne now turns his attention back to Shakespeare's lifetime by counting sheer numbers of editions up to 1616. Shakespeare is way out on top at forty-five, then Heywood at twenty-three, and Jonson at fifteen. In his own lifetime Shakespeare was a play publishing giant, and in fact he achieved this by 1600, when he had twice as many editions out as his nearest rival, Lyly. What about Shakespeare's name being absent from early title pages: does this invalidate Erne's reckoning of his popularity in print? No, because even counting just title-page ascriptions, rather than all editions, Shakespeare rockets into the lead from 1598 when his name starts to appear on his books. Shakespeare could hardly have been unaware of his pre-eminence in the field of printed plays; he not only wanted to be a successful literary author—he was one. Whether or not he was the most popular of the pre-Commonwealth dramatists as far as Restoration audiences and readers were concerned—and Erne is willing to accept that he might not have been—in the first half of the seventeenth century Shakespeare stood head and shoulders above everyone else.

The last article of relevance from this year's *Shakespeare Survey* is by Sonia Massai, 'Shakespeare, Text and Paratext' (*ShS* 62[2009] 1–11), and argues that Shakespearian paratexts should be given the same status as texts. Defending the non-peripherality of books' preliminaries, Massai claims that they were generally the last part of a book to be printed only because of 'the practical challenge of casting-off the printer's copy before the presswork started' and not because of 'any perceived difference' in their status compared to the main

text (p. 2). This cannot be true because (i) they were printed last even in books for which the copy was not cast off, and (ii) their status was manifestly different since the copy for the preliminaries came on separate pieces of paper and, as she freely acknowledges in her own footnote (p. 2, n. 4), they might not be attached to all exemplars in the print run. Massai gives some examples of prologues and choruses being half-in and half-out the play and includes in the things we must treat as being part of Shakespeare's text such print entities as act and scene divisions and even running-titles. This is quite a mix of disparate materials, since running-titles have no dramatic equivalent and are seldom found in manuscript playbooks. Such things have only a mechanical or even an accidental explanation, and to overstress the paratextual can be to mistake the mechanical/accidental for the meaningful.

Massai reproduces the epilogue to *Lochrine* that was printed in 1595 as 'Newly set forth, ouerseene and corrected by W.S.' and writes that she is not interested in whether Shakespeare wrote it but in the fact that the epilogue refers to the monarch in the 'here and now' (p. 7). In fact the line 'That eight and thirtie yeares the scepter swayd' refers to Elizabeth I in the past tense and is numerically inaccurate: not until November 1596 had she reigned for thirty-eight years. Massai thinks the epilogue's here-and-nowness betrays a 'company man' concerned with serving his patron, but even if Shakespeare had once been a Queen's man he was in 1595 under a different patron as a Chamberlain's man. Massai makes a convoluted argument that perhaps the act intervals in Folio *As You Like It* do not reflect theatrical practice, since they do not mark temporal breaks, as though this were the only way of using intervals. The events of *The Tempest* happen more or less in real time, but Massai nonetheless agrees that its intervals reflect theatrical practice. More importantly, it is not obvious why act intervals count as paratext at all rather than just being the structure of the text. The intervals themselves are, to be literal about it, not texts but gaps between pieces of text.

In a surprising volte-face in the middle of her article, Massai objects that modern editions of *As You Like It* retain its merely scribal/editorial act divisions when they should in fact jettison them as nothing to do with Shakespeare. Yet she had earlier complained that because of New Bibliography's legacy—especially the lingering 'tendency to identify the printer's copy rather than the printed text as the ultimate source of textual authority'—we find in modern editions that 'all those features that were added to the printer's copy as the dramatic manuscript was transmitted into print and transformed into a reading text tend to be overlooked' (p. 1). It seems that now she wants to revert to the authority of the underlying copy too, and remove the Folio's editorial layer. Bravo, but why criticize others for doing the same? Massai wants the paratext to be edited in the same way as the text, and hence is surprised that the act intervals she has identified as non-theatrical have not been edited away in modern editions. The difficulty, of course, is that it is not clear that these intervals are entirely mechanical and can be got rid of: perhaps those in *As You Like It* and Folio *The Merry Wives of Windsor* (her other example) reflect what happened when a play that was written for continuous performance was revived after the use of intervals became normal. That is, the breaks we find unsatisfactory might nonetheless have been used in

performances. Even if we were certain that the intervals were imposed by a scribe or a printshop worker for the purpose of making the Folio, it would still be no easy matter to remove them, since we would have to speculate about—and try to restore for the modern reader—the scribe or the printshop worker’s underlying copy. That is the very New Bibliographical project that Massai complains about at the start of this argument.

The second issue of this year’s volume of the journal *Critical Survey* is devoted to the topic of ‘Questioning Shakespeare’, which turns out to mean asking the silliest question of all: did he write the plays? The first of the issue’s four articles is Roger Stritmatter and Lynne Kositsky’s repetition of their groundless claim that *The Tempest* is not dependent on the Strachey Letter report of the shipwreck of the *Sea Venture* off Bermuda ‘“O Brave New World”’: *The Tempest* and Peter Martyr’s *De Orbe Novo*’ (CS 21:ii[2009] 7–42). (Their previous attempt to establish this point is comprehensively invalidated in an expert study reviewed below.) Stritmatter and Kositsky argue that Richard Eden’s *Decades of the New World* of 1555—a translation of Peter Martyr’s *De Orbe Novo*—is the prime source for *The Tempest*, listing (and at certain points tabulating) what they think are striking parallels. Of course no one denies that Eden is a minor source—for example, providing the name of Sycorax’s god Setebos—so the whole argument depends on the reader sharing the authors’ conviction that long-acknowledged tenuous links are actually strong ones.

The second article, ‘*Cymbeline*: “The First Essay of a New Brytish Poet”?’ (CS 21:ii[2009] 43–59), is Penny McCarthy’s claim that *Cymbeline* was begun in the early 1590s and continually revised by Shakespeare until, but not after, the death of Elizabeth. McCarthy argues that thematically *Cymbeline* does not quite so tightly form a group with *The Winter’s Tale* and *The Tempest* as has been supposed: other, earlier plays have elements present in what has been called the Romances group and *Cymbeline* could have been written much earlier than 1609. (The main things to be overcome here are the copious stylometric evidence and the dependence upon Beaumont and Fletcher’s *Philaster* that both put *Cymbeline* around 1609; McCarthy has nothing to say about them.) The connection with *The Tempest* as a late play McCarthy tries to weaken by pointing to the anti-Stratfordians’ claim that the play is not indebted to the Strachey Letter, and she focuses on loose parallels between aspects of *Cymbeline* and things happening, and works published, in the 1590s and early 1600s. She finds great significance in coincidences, such as Innogen having a ‘cinque-spotted’ mole and the fact that ‘the personal emblem of Robert Dudley was a cinque-foil’ (p. 53). This kind of ‘evidence’ leads McCarthy to the clairvoyant conclusion that ‘Shakespeare long nursed a secret but rather vain desire, as did all the Dudley faction, that an alien Scot should not succeed to the “British” throne—leaving the way clear for the true British, the Dudleys’ (p. 56).

The third article is by Roger Stritmatter alone, ‘The Tortured Signifier: Satire, Censorship, and the Textual History of *Troilus and Cressida*’ (CS 21:ii[2009] 60–82), and needs no close examination. It is a literary-critical argument about personal satire, censorship, and topicality in *Troilus and Cressida*, built on slender evidence and attempting to co-opt to its own ends

the bibliographical facts of the play's publication. The attempt is frustrated by the author's ignorance of most recent discussions of the topic, evident in his treatment of Alice Walker's 1950s scholarship as if it were the latest thing. The last article, 'Shakespeare Authorship Doubt in 1593' (*CS* 21:ii[2009] 83–110), is by Rosalind Barber and aims to show that doubting Shakespeare's authorship was not a nineteenth-century innovation but first arose in the 1590s. Her logic is bizarre. Because scholars suspect that the verdict of the inquest into Marlowe's death was inaccurate—recording it as self-defence where we think it was murder—the evidence of the death itself should be disregarded as unreliable. This is like arguing that those who believe that the 1972 Widgery Report into the Bloody Sunday killings by the British army in Derry came to the wrong conclusion—determining that the soldiers acted in self-defence—are obliged to remain open to the possibility that, although thirteen bodies were buried, no one was killed that day. We should not believe that the body buried was Marlowe's, argues Barber, because the witnesses were known liars.

The remainder of Barber's article relies upon the reader accepting her absurd premise that Marlowe did not die in 1593. In the line of duty, however, this reviewer read to the end and can report that the full panoply of anti-Stratfordian irrationality is present, including the idea that Shakespeare was not known as a writer in 1593, which requires that the allusion in *Greene's Groatworth of Wit* (published 1592) to a 'Shakes-scene' and the line about a 'tiger's heart wrapped in a woman's hide' from *3 Henry VI* have nothing to do with Shakespeare. Barber repeatedly mentions that certain works were entered into the Stationers' Register 'anonymously'—in the sense of their authors not being named, although the stationers' names are present—as if this should raise our suspicion of something untoward. In truth, of course, it did not matter to the stationer at this point who the author was, since it was his own exclusive right to publish on a particular topic, as expressed in the work's title, that entry in the register helped to establish. It does the reputation of the journal *Critical Survey* harm to publish articles as ill informed and prejudiced as the four that are supposed to be 'Questioning Shakespeare'.

A much better quartet of articles appeared in *Shakespeare Quarterly*. In the first, 'The Anachronistic *Shrews*' (*SQ* 60[2009] 25–46), James J. Marino argues that editors have come up with unwieldy narratives to avoid acknowledging that perhaps Fletcher revised *The Taming of the Shrew* around 1619–23. In the Folio text, the Lord praises one of the visiting players for his previous performance of a character whose name the Lord has forgotten, and the reply is '*Sincklo*. I thinke 'twas *Soto* that your honor meanes' (sig. S3r). *Sincklo* is clearly the actor of that name, who seems to have been a hired man. *Soto* is presumably the character of that name in Fletcher's *Women Pleased*, who does what the Lord says he saw performed: dresses up in his master's clothes to woo a gentlewoman. The action of Fletcher's play is like the class cross-dressing of *The Taming of the Shrew*, but it is dated much later, at 1619–23. So, the speech prefix ties *The Taming of the Shrew* to the 1590s and early 1600s—because *Sincklo* is in the plot of *2 Seven Deadly Sins* and is last heard of in 1604, while the *Soto* reference ties it to the late 1610s or early 1620s. The standard editorial explanation, from the Cambridge New Shakespeare of 1928, is to say that

either this line in *The Taming of the Shrew* (and those around it) are later interpolations, or there was once an earlier play with another Soto in it. The New Shakespeare editors were writing just after Peter Alexander made the claim that the manuscript underlying Folio *The Taming of the Shrew* preceded the one underlying the 1594 quarto of *The Taming of a Shrew*, which claim they accepted, so they found Soto an embarrassment to be got around.

We cannot, Marino insists, simply invent lost plays to solve our puzzles, and we cannot assume that just this small segment of Folio *The Taming of the Shrew* is late: why might not the whole play have been revised after Shakespeare's death? Marino rather unfairly mocks Wells for considering the possibility that Soto was a late authorial insertion into *The Taming of the Shrew*, which Marino thinks silly because *Women Pleased* was first performed around 1620, when Shakespeare was four years dead (p. 31, n. 15). This is unfair because Wells makes his suggestion in the context of considering the possibility that *Women Pleased* might have been first performed rather earlier than this, since the cast list that gives us the 1620 date—which appeared in the 1679 Beaumont and Fletcher Folio—need not be the cast list of the first performances. When complaining of the *Oxford Complete Works* editors' treatment of the problem, Marino gives no credence to the stylometric tests that unequivocally indicate an early date for *The Taming of the Shrew*. There are significant errors in Marino's handling of the detail of this problem. He is under the misapprehension that the term 'foul papers' is used in 'a letter from Edward Knight' (p. 34, n. 25), the King's men's scribe, but of course it is in Knight's transcript of Fletcher's *Bonduca*, and he misidentifies the 1595 octavo edition of *Richard Duke of York* as a quarto from 1594 (p. 41). Marino writes that 'In fact, John Sinckler or Sincklo's first name is only known to us because it appears in a playhouse manuscript that mixes actors' and characters' names indiscriminately' (p. 37). Presumably, he is here thinking of *2 Seven Deadly Sins*, and that is not an accurate description: every fictional character is named and many are glossed with actors' names; no mixing happens.

Regarding Sincklo's career, Marino rightly observes that we need not assume it ended with his appearance the print edition of *The Malcontent* in 1604. Indeed, oddly enough his name repeatedly turns up in connection with Chamberlain's/King's men's plays that came to them from other companies: *The Taming of the Shrew*, *3 Henry VI* (both Pembroke's), *2 Henry IV* (which has some connection with the Queen's men's *Famous Victories*), and the Induction to *The Malcontent*, which is about how the play jumped from one company to another. Aside from the desire to locate the manuscript underlying Folio *The Taming of the Shrew* before, and as the origin of, *The Taming of A Shrew*, 'all of the other available evidence places *Women Pleased* between 1619 and 1623' (p. 43). This is rather overstating the matter, since the only evidence for that date is the cast list in the 1679 Beaumont and Fletcher Folio. Marino finds it hard to believe that Shakespeare's foul papers would have been kept for thirty years and then allowed to be destroyed to print F, but in fact it is not necessary to suppose that F *The Taming of the Shrew* was based on foul papers to believe all the things that Marino says we should not believe. Moreover, the Folio project was big enough that Heminges and

Condell might well have done some rummaging and found the neglected—because no longer needed—foul papers in the theatre library. Referring to its plot inconsistencies, Marino also finds it hard to believe that Shakespeare would have left the play uncorrected for twenty years. But again, the obvious retort is that Shakespeare could have corrected any inconsistencies without altering the foul papers. It is easier to believe, according to Marino, that Fletcher revised Shakespeare's play and introduced those tangles. Marino's characterization of the preciousness of Shakespeare scholarship seems dated when he supposes a prejudice against the 'implicitly forbidden hypothesis that Shakespeare's works might have been substantially improved by his collaborators' (p. 44). I should have thought that such hypotheses are rather popular. Marino concludes by admitting that he has not got his own explanation of the Sincklo/Soto problem, which is rather a disappointment.

Michael Hattaway's article, 'Dating *As You Like It*, Epilogues and Prayers, and the Problems of "As the Dial Hand Tells O'er"' (*SQ* 60[2009] 154–67), shows that, contrary to recent arguments by Juliet Dusinberre, we do not have a new epilogue to *As You Like It*, nor can we date the play's first performance to Shrovetide 1599. *As You Like It* is not mentioned in Francis Meres's *Palladis Tamia*, entered in the Stationers' Register on 7 September 1598, but must have been written by the time its own 'staying entry' was made in the Register on 4 August 1600. The internal evidence for dating *As You Like It* is weak, as is the performance evidence such as the play's Robin Hood theme being perhaps an answer to the Admiral's men's Robin Hood plays at court during Christmas 1598 or Shrovetide 1599. We have a record of the Chamberlain's men being paid for a court performance on 20 February (Shrove Tuesday, Pancake Day) 1599 at Richmond Park, where Elizabeth liked to hunt and where there was a recently refurbished sundial. All three—the pancakes, the hunting, the sundial—are links with *As You Like It*, hence Dusinberre's suggestion that this was the first performance. Hattaway objects that the play's pancake jest would work just as well some time after this day, and that the play contains an allusion to the June 1599 Bishops' Ban burning of satiric books, which allusion could also work some time later. The allusion itself is 'for since the little wit that fools have was silenced, the little foolery that wise men have makes a great show' (I.ii.84–6). Moreover, the Chamberlain's men played at court during Christmas 1599 and on Shrove Sunday 1600, so if *As You Like It* were first performed at court it could have been during either of these visits.

The poem 'As the Dial Hand Tells O'er', dated 1598 in Henry Stanford's commonplace book, was clearly written for the queen at Shrovetide, but as Hattaway points out the Admiral's men also played at court on Shrove Sunday 1599 so it could as likely be theirs as the Chamberlain's men's, who played there on Shrove Tuesday 1599. The style of the poem—trochaic, with use of uninflected genitives—is more Jonsonian than Shakespearian. Also, it seems to wish the queen several dozen more years of life, which in 1598 would be ridiculous, and uses a phrase close to Mary Queen of Scots' motto, which would be unwise after her execution in 1587. In fact, Hattaway argues, there is no reason to suppose that it is an epilogue at all. It does not do the usual epilogue work of asking people to think well of the play, and is more like a

prayer, which might itself form part of an epilogue. Hattaway ends by surveying what we know about *As You Like It's* earliest performances, including the transition from William Kempe to Robert Armin as company clown, and the sign and alleged motto of the Globe. Much of this is speculation, he points out.

The last article from *Shakespeare Quarterly* in 2009 to be noticed here is only tangentially relevant. Jeffrey Todd Knight, in 'Making Shakespeare's Books: Assembly and Intertextuality in the Archives' (*SQ* 60[2009] 304–40), tells the history of the binding, rebinding, putting together, and taking apart of early editions by Shakespeare and others. Knight subscribes to the contentious idea popularized by D.F. McKenzie that 'forms effect meaning' (p. 306), which appears to be driving a fresh interest in the ways that purchasers chose to have their books bound together as collections. (Erne's article reviewed above also touches upon this.) It is easy to overstate the importance of these choices, since an element of happenstance must enter into them. Binding decisions may be driven by purely practical needs, and the fact that even modern libraries shelve books by size should warn us against over-reading physical juxtaposition. Knight makes the interesting point that when bound together the Thomas Pavier quartos of Shakespeare did not always preserve the continuity of signatures across plays that is usually taken to indicate that he was attempting to make a collected works. Some volumes even bound non-Shakespearean plays with the Shakespeare ones (pp. 324–6). Knight makes an unfortunate slip in claiming that *Pericles* depicts 'a Governor of Tarsus whose starving people rise up to kill their leader' (p. 332). In fact the people rise up against Cleon and his family, in anger at the attempt on the life of Marina, many years after the city's hunger is relieved by her father Pericles; during these years Marina is born and grows to adolescence.

In an article from *Shakespeare Quarterly* overlooked last year, 'William Strachey's "True Reportory" and Shakespeare: A Closer Look at the Evidence' (*SQ* 59[2008] 245–73), Alden T. Vaughan shows that William Strachey's Letter, or 'True Reportory', is indeed a source for *The Tempest* despite the contrary claim by Stritmatter and Kositsky in their article 'Shakespeare and the Voyagers Revisited' (reviewed in *YWES* 88[2009], covering work published in 2007). Richard Hakluyt was clearly preparing a third edition of *Principal Navigations* after the second edition of 1600, although it never appeared. Equally clearly, the Reverend Samuel Purchas acquired the material that Hakluyt was accumulating, with a view to putting together a posthumous third edition of Hakluyt. Purchas's 1625 volume *Purchas's Pilgrims*, where 'True Reportory' was first published, used the letter H (for Hakluyt) or P (for Purchas) before each item in the table of contents to show who was responsible for it, and Strachey's 'True Reportory' is marked with an H. After Strachey's Letter Purchas prints extracts from the Virginia Company's publications and it is likely that Hakluyt himself did the editing necessary to include 'True Reportory' in this larger narrative of exploration. Essentially, Purchas printed 'True Reportory' as he got it from Hakluyt.

'True Reportory' would not have pleased the Virginia Company in 1610, as it revealed bad behaviour by the colonials and it made Bermuda sound so attractive that Spain would be encouraged to try to take it. But by 1625 it

would be innocuous: Bermuda was secure and the Virginia Company had ceased to exist. There would be no reason for Purchas to meddle with 'True Reportory' when publishing it, other than adding a few notes, nor to give it a false date. Vaughan gives an account of the discovery in 1983 of a nineteenth-century transcript of Strachey's Letter that seems to derive from a version different from the one published. He thinks it likely to be based on Strachey's draft, written while still in Bermuda, whereas the published one was a more polished account later completed in Jamestown. Stritmatter and Kositsky claimed that 'True Reportory' cannot have been written in Jamestown because it describes the voyage back to England of Sir Thomas Gates, which voyage carried 'True Reportory' to London. Vaughan points out that this is a misreading of 'True Reportory', which says only that Gates is 'now bound for England', meaning that he is waiting for embarkation at Point Comfort, 40 miles from the Jamestown colony. Jamestown did not have the resources for a transatlantic voyage and fleets rendezvoused at Point Comfort; while the ships preparing to sail to England were gathering there, they took on board Strachey's letter.

Stritmatter and Kositsky's claim that 'True Reportory' is Strachey's answer to a request for information from Richard Martin, secretary of the Virginia Company, is implausible, Vaughan points out, since it is addressed to an anonymous 'lady' and reports many things Martin did not ask about. Having asserted rather than shown that 'True Reportory' was written in 1612 or later, Stritmatter and Kositsky accuse it of plagiarizing other works that, if anything, plagiarized it. However, Vaughan thinks in fact there was no real plagiarism: the ideas they have in common are simply ones shared by people in this circle. The survival of the shipwrecked passengers of the *Sea Venture* was extraordinary news in London in September 1610, and was made much of in pamphlets. Strachey's manuscript account, 'True Reportory', would have been very popular and widely repeated. The parallels with *The Tempest* are ample and well documented, and while Shakespeare certainly could have got them from a whole set of other sources, Strachey's account 'bundled them conveniently... at just the right moment for dramatic adaptation' (p. 272).

The volume of *Studies in Bibliography* published in 2009 was 'for 2007-8', which raises hope that it will soon return to currency. Three articles are of relevance to this review. The first, 'Mind and Textual Matter' (*SB* 58[2009 (for 2007-8)] 1-47), is by Richard Bucci and argues that the Greg-Bowers editorial techniques provide much the best way to present old texts to modern readers and that their detractors are ill informed and confused. Bucci provides a fine critique of the illogicality of postmodern positions on editorial theory such as Stephen Orgel's, and of inaccurate characterizations of Greg by Werstine. The postmodern textualist movement entirely misunderstood Greg's ideas about accidentals, and Bucci insists that editors who are author-centred—instead of concerning themselves with the socialized text—are not necessarily Platonists, nor blind to the instability of texts or their social contexts. Bucci describes how the Greg-Bowers approach affected the editing of American literature, for which it is more suitable than it was for early modern drama because the evidence of authorial revision, indeed the documentary evidence in general,



is more plentiful. This is an interesting line of argument, since it is more commonly held that the Greg–Bowers approach ought not to have been applied beyond early modern drama. Bucci continues his history of this line of thinking to embrace its extension by G. Thomas Tanselle and brings in a useful discussion of the tension between the editorial principles of *usus scribendi* (look for the author's usual practice) and *lectio difficilior potior* (the more difficult reading is preferable). To apply this to a concrete case we may observe that the recent Arden<sup>3</sup> edition of *Hamlet* (reviewed in *YWES* 87[2008], covering work published in 2006) has Osric say that he speaks *sellingly* of Laertes (V.ii.93), from the uncorrected state of Q2, rather than *feelingly* as most editions do, using the corrected state. The Arden editors applied the principle of *lectio difficilior potior* (*sellingly* being unique to this text) while others apply *usus scribendi*, noticing that *feelingly* is used by Shakespeare in a number of plays. Bucci ends with examples of modern editions made along author-centred lines that he thinks particularly fine.

S.W. Reid, 'Compositor B's Speech-Prefixes in the First Folio of Shakespeare and the Question of Copy for 2 *Henry IV*' (*SB* 58[2009 (for 2007–8)] 73–108), shows that Folio 2 *Henry IV* was most likely set from annotated quarto copy, since it preserves features of the 1600 quarto's speech prefixes that are unlikely to have survived in an intervening manuscript. The forms of Folio compositor B's speech prefixes can tell us what kind of copy he had, since he was conservative, tended to repeat the form of a name he had just set in a stage direction, and preferred short to long names. Looking at his work in *Much Ado About Nothing*, *Love's Labour's Lost*, *A Midsummer Night's Dream*, *The Merchant of Venice*, *1 Henry IV*, *Titus Andronicus*, and *Romeo and Juliet* it is clear that he frequently set two-, three-, or four-letter speech prefix forms even when there was room on the line to set longer ones. He strongly preferred three-letter forms and imposed them even when his quarto copy had longer forms, although this preference is more strongly marked in the comedies than the histories and tragedies, where he let his copy influence him into tolerating more four-letter ones. Compositor B seems to have wanted to avoid ending a speech prefix with a vowel, which accounts for a number of four-letter forms instead of three such as *Leon* and *Brag*. However, contrary to Bowers's description, he did not go for maximally abbreviated forms: he demonstrably set more letters than were needed for disambiguation of characters in a number of cases. Compositor B frequently first encountered a name in the middle of a scene—because of the order of page setting in a folio-in-sixes—and did not know the character's full name. For this reason it sometimes took him a while to settle on a preferred form of the speech prefix, and of course he could not expand a short form in his copy if he had not yet encountered the full name.

Reid traces compositor B trying to settle on a standard speech prefix for Poins in *1 Henry IV*, given his quarto copy's use of *Poy* and *Po*. The important thing to figure out is when compositor B discovered a character's full name; once that happened he used a three- or four-letter form right away in almost all cases. Compositor B's preference for his speech-prefix forms seems to have persisted even in long lines that show abbreviation elsewhere in the line, including extreme abbreviation such as setting an ampersand for *And*, and

*Emp* in place of *Emperour* in a line of dialogue. Occasionally he would lengthen a speech prefix just to help justify a line. In general, the evidence shows that where compositor B departed from his adopted form of a name—changing to a different name for the same character, or using a form longer than he was wont to do—it is because his copy showed this variation. Thus, with compositor B's general habits defined, we can use them to speak of the characteristics of other Folio plays for which we do not know the copy, such as *2 Henry IV*. Reid summarizes the arguments over the past 150 years about whether Folio *2 Henry IV* was set from an authoritative manuscript or a quarto annotated by reference to an authoritative manuscript, or something else. Evidence from compositor B's speech prefixes in *2 Henry IV* should help, although the matter is complicated by a casting-off error that made the compositors first try to compress the play to fit into a standard quire g and then to expand the play when it was decided to add an eight-leaf quire <sup>x</sup>gg.

In quire g compositor B departed from his usual practice by setting full-length speech prefixes (such as *Hostesse* and *Snare*) that are identical to the quarto speech prefixes at the corresponding points, so presumably his copy had the quarto speech prefixes and they influenced him to break his abbreviating habit. Some of these long settings might have been done for the sake of justification, but not all. Moreover, this happens even where we have reason to suppose he was trying to save rather than waste space because of casting-off error. The same use of full-length names as speech prefixes happens on quire <sup>x</sup>gg, although of course this is harder to evaluate since compositor B here needed to expand his copy to fill the quire. But there are several full-length speech prefixes here in lines that are short, so there was no hope of making a new line by using the long name. The obvious conclusion is that, since the longer forms are in Q, he was following copy that had Q's longer forms. There is some counter-evidence—compositor B setting long forms where Q has abbreviated ones—but Reid disposes of them as special cases induced by local matters, such as the need to expand copy or the influence upon compositor B of the appearance of a full name in a stage direction just before the speech prefix. Reid's conclusion is that either compositor B was setting Folio *2 Henry IV* from quarto copy or, much less likely, he was setting from manuscript copy that slavishly followed the forms of Q's speech prefixes.

Reviving an explanation of textual origin that has until recently been neglected, Gerald E. Downs, in 'Memorial Transmission, Shorthand, and John of Bordeaux' (*SB* 58[2009 (for 2007–8)] 109–34), argues that the manuscript of the play *John of Bordeaux* was created by stenographic recording of performance, and perhaps other surviving play texts have this origin too. G.I. Duthie's rejection of stenography as not the cause of the errors in Q1 *King Lear* is, as Adele Davidson has argued, not a logically strong one: all that Duthie achieved was to show that stenography does not have to be the cause of those errors, not that it cannot be. In any case, Duthie was not sufficiently expert in stenography and found problems in it that do not exist. Moreover, if we think Folio *King Lear* is an adaptation of Q1—the dominant view since the 1980s—then the F/Q differences cannot easily be used as evidence for Q's copy. The evidence from reprints, with authorial corrections,

of sermons first printed from stenographic recording indicates that the preachers considered those accounts to be good enough versions of what they had preached. *John of Bordeaux* is a manuscript written out by a scribe who, to judge from certain errors, cannot be its author, and it is lightly annotated by theatrical hands; it has the name of the actor John Holland in three marginal notes, and there is an added speech in the hand of Henry Chettle. It is a sequel to Greene's *Friar Bacon and Friar Bungay*, so presumably he is its author. Verse is lined as prose but with punctuation falling where the lines should end, as if the scribe recognized that it was verse, and there are plenty of unmetrical lines mixed with metrical ones.

Downs finds evidence of eye-skip in the manuscript, and points out that this can occur when a stenographer expands his shorthand symbols to make a longhand version. Downs spots a couple of repetitions best explained by an actor coming in with the wrong speech, ad-libbing to get himself out of trouble, and then repeating those lines later in the correct place. In the manuscript the first of these two speakings is deleted, but the ad-libbing remains, and Downs reckons this could not have happened in dictation, only in notes taken during performance. Downs quotes a garbling in Q1 *Hamlet*, first noted by B.A.P. van Dam, that is hard to explain other than as an actor coming in with the wrong one of his speeches and another actor noticing the mistake and adjusting his speech to make up for it. Claudius says to Laertes 'content your selfe, be rulde by me, | And you shall haue no let for your reuenge' to which Laertes replies (meaninglessly) 'My will, not all the world', which is in fact the correct response to 'Who shall stay you?', a question present in Q2 and F but not in Q1. Spotting the error, the actor of Claudius comes back with 'Nay but Learthes, marke the plot I haue layde' (H3r) to restore the exchange to sense. Downs finds in this a parallel for his claims about actors recovering from error in *John of Bordeaux*. There are also misnamed characters that are best explained by a stenographer not knowing who is who in an early entrance of several people—because stage directions are not spoken but witnessed—and getting ascriptions wrong in a way much more difficult to do in the transcription of writing.

There are strange, phonetic spellings in *John of Bordeaux* such as *grattewlat* for *gratulate* and *anenstrewment* for *an instrument*, and while it is true that spelling in this period was variable, a scribe who was this idiosyncratic would not get much work. More likely is that these are the effects of stenography, especially where the manuscript has gibberish Latin, for which the stenographer presumably could do no more than represent the sound he heard because he did not know the language. As Downs points out, Greg argued the same kind of aural corruption as the source for the gibberish Latin in the memorially reconstructed quarto of *Orlando Furioso*, where Edward Alleyn's actor's part has it basically right. In stenography the *p/b* distinction would not be recorded but rather left to be recovered later, from context, when writing out longhand, and there are a number of *p/b* errors in *John of Bordeaux*, some corrected by subsequent overwriting. Similarly, stenographic use of the same symbol for *k/c/q* would explain a number of odd spellings and subsequent corrections. In a number of places the manuscript makes the *u/v* distinction based on sound (as we now do) rather than on place in the word (as was more

normal at the time), so this too points to notes taken by ear. Downs draws attention to other manuscript oddities, such as *in cappable* for *incapable*, that he thinks are best explained from stenography. Since the attempt to correct *John of Bordeaux* to remove the errors arising from stenography could have gone much further, it is distinctly possible that many other plays we have were copied this way and then tidied up so well that we cannot see how they were made.

*The Review of English Studies* contained one article of relevance this year, 'Did Shakespeare Own His Own Playbooks?' (*RES* 60[2009] 206–29), in which Andrew Gurr argues that perhaps a playing company did not always own its own playbooks but rather the players owned them personally, as did impresarios and authors. With one exception, playbooks containing performance licences never went to the printer, and there are only two extant licensed playbook manuscripts: Middleton's *The Second Maiden's Tragedy* (Lansdowne 807) and Massinger's *Believe as You List* (Egerton 2828). The exception is the printing of *The Walks of Islington and Hogsdon* in 1657 from the allowed book and including its licence. Who owned the allowed book? We assume the company did, but our model for this is Shakespeare's relationship with the Chamberlain's/King's men. The Admiral's men and other companies seem to have operated differently. Shakespeare is the obvious candidate for being the agent by whom his pre-1594 plays entered the repertory of the newly formed Chamberlain's men, but who brought Marlowe's plays to the newly formed Admiral's men in 1594? The inventory of playbooks owned by the Admiral's men in 1598 contains only twenty-nine plays, yet since 1594 they had performed ninety-four plays at the Rose, so who owned the other sixty-five? Perhaps it was Alleyn personally. How did *Titus Andronicus* get from Derby's to Pembroke's to Sussex's to Chamberlain's men by 1594? If by Shakespeare's personal ownership of it, this would violate our idea of the obligations of a sharer in a joint-stock company. Perhaps these obligations emerged only after 1594. Gurr thinks that there was a variety of different forms of each play, so there can be no editorial singularity to represent them all.

An entire issue of the journal *Textual Cultures* (formerly *TEXT*) was devoted to W.W. Greg because 2009 contained the fiftieth anniversary of his death. A.C. Green's article, 'The Difference between McKerrow and Greg' (*TC* 4:ii[2009] 31–53), is irritatingly written, with half its words as discursive footnotes, as if attempting to tell two stories at once. After some rather inconsequential discussion of terminology—just who meant what by (New) Bibliography and when—the article ends up using personal correspondence plus manuscripts and typescripts of Greg's lectures, articles, and books (conserved at the Beinecke Library of Yale University) to throw light on the genesis of McKerrow's *Prolegomena for the Oxford Shakespeare* [1939] and Greg's *The Editorial Problem in Shakespeare* [1942]. Apparently the former felt put out by the latter stealing his thunder, and Greg's responses to McKerrow's book—plus his interactions with Paul Maas—led to his celebrated essay 'The Rationale of Copy-Text' [1950–1]. A useful tidbit is that in manuscript correspondence just before his death McKerrow qualified his view on the

drying of sheets between printing and perfecting, having wrongly described in his *An Introduction to Bibliography* [1927] their being hung up for this purpose.

A.S.G. Edwards's article, 'W.W. Greg and Medieval English Literature' (*TC* 4:ii[2009] 54–62), is unfortunately outside the scope of this review, being concerned with Greg's (largely dropped) interest in Old English and medieval literature. Likewise T.H. Howard-Hill's 'W.W. Greg as Bibliographer' (*TC* 4:ii[2009] 63–75), a description of neglected early work by Greg (before 1902) that was preparatory to his *Bibliography of the English Printed Drama*, which reveals how his ideas were emerging, and Laurie Maguire's 'W.W. Greg as Literary Critic' (*TC* 4:ii[2009] 76–87), a brilliant analysis of Greg's 1917 essay 'Hamlet's Hallucination' and the ways in which his literal editorial mind responded to the duplications and contradictions of the play by trying (and failing) to reduce them to coherent singularities. Gary Taylor, 'In Medias Res: From Jerome through Greg to Jerome (McGann)' (*TC* 4:ii[2009] 88–101), concerns himself with a comparison of St Jerome as a translator of the Hebrew Bible into Latin and Greg as a transcriber who refused to translate and for whom modernization was a form of translation. Taylor finds that the transcription/translation binary is not terribly secure and that all such activities—which he sees as existing along a spectrum—necessarily 'remediate' the text to a greater or lesser extent, according to the needs of the target audience. Unmediated transmission is, of course, impossible. Implicitly opposing Maguire's argument, Sukanta Chaudhuri, in 'W.W. Greg, Postmodernist' (*TC* 4:ii[2009] 102–10), sees Greg as a proto-postmodernist in that he accepted and embraced textual multiplicity and resistance to closure. Or at least Greg became a proto-postmodernist between writing *The Calculus of Variants* [1927] and 'The Rationale of Copy-Text' [1950–1]. Annoyingly, Chaudhuri repeatedly references a work by Jerome J. McGann given as '(McGann 2001)' for which there is no corresponding entry in the list of works cited.

In the journal *Shakespeare* co-edited by this reviewer, B.J. Sokol, 'A Warwickshire Scandal: Sir Thomas Lucy and the Date of *The Merry Wives of Windsor*' (*Shakespeare* 5:iii[2009] 55–71), finds a historical allusion indicating that *The Merry Wives of Windsor* was written in or after 1600. Leslie Hotson came up with and popularized the dating of the first performance of *The Merry Wives of Windsor* to an Order of the Garter feast at Westminster on 23 April 1597, although there is little evidence for it. Shakespeare undoubtedly knew of Sir Thomas Lucy (< 1532–1600), who was tutored by John Foxe and who had Shakespeare's mother's second cousin arrested as a Catholic conspirator in 1583 and subsequently executed. Sokol lists other less horrific connections between Lucy—and his son and grandson, both also called Thomas—and Shakespeare's cultural and social world in London. Lucy's granddaughter Elizabeth Aston was orphaned, and Lucy looked after her and her siblings at Charlecote, hiring for them a tutor called Bartholomew Griffin. In 1600 Elizabeth Aston ran away to marry, against her family's wishes, a former servant from Charlecote called John Sambach of Broadway. Lucy engaged the Attorney General Sir Edward Coke to fight the marriage in court, alleging that Griffin helped the couple in the hope of financial gain. Elizabeth Aston, then, was like Anne Page in the play: old enough to marry of her free

will, in possession of a small inheritance, and likely to do a lot better financially if she married according to her family's wishes. But whereas Lucy fought the marriage, and kept Elizabeth Aston from part of her inheritance, the Pages accept their daughter's choice.

In the play, Shallow says that his family coat contains 'a dozen white luces' (I.i.14), meaning a type of fresh water fish, and the Lucy coat also contains luces, but only three. Sokol shows that we can find twelve luces in a picture of an early Lucy in the first edition of William Dugdale's *The Antiquities of Warwickshire Illustrated* [1656]. The charges Shallow wants to bring against Falstaff in the play—riot, park breaking, and deer stealing—are the ones that the historical Lucy brought in complaint against the abduction of his granddaughter Elizabeth Aston, if we allow one 'cherished creature' (p. 365) to stand for another. The allusion to Elizabeth Aston's marriage would date composition of *The Merry Wives of Windsor* to 1600 or later, and hence after the completion, with *Henry V*, of the second tetralogy. This would make sense of *The Merry Wives of Windsor* bringing on Shallow, Bardolph, Nym, and Pistol at its beginning and then not using them very much, since if they were already known and loved—from *1 Henry IV*, *2 Henry IV*, and *Henry V*—then such exploitation would 'attract spectators who were already familiar with them' (p. 368), as G.R. Hibbard put it. Conversely, if *The Merry Wives of Windsor* were written in 1597 it makes little sense for the tetralogy to expand on these minor figures from it, especially as Nym is not used again until *Henry V* in 1599.

Alan Galey, 'Signal to Noise: Designing a Digital Edition of *The Taming of a Shrew* (1594)' (*ColL* 36:[2009] 40–66), gives an account of the editorial attempts, especially in the eighteenth century, to patch *The Taming of the Shrew* with bits of the metatheatrical framing material from *The Taming of A Shrew*, and shows how he will present these plays' relationship in his new digital edition. Gefen Bar-On Santor, 'The Culture of Newtonianism and Shakespeare's Editors: From Pope to Johnson' (*ECF* 21[2009] 593–614), argues that eighteenth-century editors used the language of Newtonianism and the new sciences to describe Shakespeare's contributions to knowledge, which were especially concerned with human motivations. Anthony James West, 'Ownership of Shakespeare First Folios over Four Centuries' (*The Library* 10[2009] 405–8), tabulates according to social class (or 'institution' such as a library) the known owners of First Folios since 1623, and finds that the institutional ownership took off in the nineteenth century and now dominates the field.

The journal *Research Opportunities in Renaissance Drama* changed its name to *Research Opportunities in Medieval and Renaissance Drama* in 2006, and this seems to have made it almost invisible to the Modern Language Association's International Bibliography (MLA-IB), which lists just three articles under this title although many more have appeared. Because MLA-IB omits them, a couple of articles relevant to this review were missed in 2007 and 2008 and will be examined now. In the first, 'The Date and Authorship of *Thomas of Woodstock*: Evidence and its Interpretation' (*ROMRD* 46 [2007] 67–100), MacDonald P. Jackson shows that Samuel Rowley wrote the play *Thomas of Woodstock* in 1598–1609, comprehensively refuting Michael Egan's claim

(reviewed in *YWES* 87[2008], covering work published in 2006) that the play is early Shakespeare and should be called *Richard II Part One*. It is unfortunate that at one point (p. 96, n. 2) Jackson attributes Egan's claim to this reviewer, but hopefully use of the correct first name (Michael, not Gabriel) in his body text will prevent this misattribution spreading. Around 1599–1600 a lot of colloquial contractions that had not been used before became popular in drama, as David J. Lake showed. *Thomas of Woodstock* has these in abundance and in lines where they perfectly fit the metre, so those lines were probably composed after 1600 as opposed to being revised then. Other features such as rates of feminine endings and distributions of pauses within lines also point to post-1600 composition for the play, as Jackson has previously shown. The play also has a collection of linguistic features that are fairly rare but occur abundantly in Rowley's play *When You See Me You Know Me*, so he is probably the author of *Thomas of Woodstock*.

Jackson works through some of the thirty-seven words that he previously showed are in *Thomas of Woodstock* but were not common before 1598, refuting in each case Egan's claim that they were available in the early 1590s. (In fact, Jackson mainly shows that they were still in use in the early 1600s, not that they were unavailable before then.) Jackson concedes that *Thomas of Woodstock's* phrase describing the king of England as 'Superior Lord of Scotland' would likely infuriate James I, but would have been innocuous before 1603. However, Jackson thinks it could have been written, incautiously, after 1603 but then excised by the censor or the self-censoring company before first performance, and indeed it is marked for deletion in the *Thomas of Woodstock* manuscript. From a hint given by Egan, Jackson has realized that the appearance together of unapostrophized *bith*, *oth*, *ith*, and *tother* (for *by the*, *of the*, *in the*, and *the other*) is unique to Rowley's *When You See Me You Know Me* and the *Thomas of Woodstock* manuscript and *Wily Beguiled*, which last has, on other grounds, been tentatively attributed to Rowley. Jackson adds five contractions/colloquialisms—*ant* for *an it*, *ont* for *on it*, *ist* for *is it*, *thart* for *thou art*, and *tush*—that are never seen together outside these three plays. *Thomas of Woodstock* also shares more expletives with *When You See Me* than it does with any Shakespeare play, and they are listed by Jackson.

Feminine endings and run-on lines became increasingly common in drama over the period 1580–1642, and the caesura shifted from after the fourth to after the sixth syllable. *Thomas of Woodstock's* rate of 21 per cent of lines having feminine endings is way above what Shakespeare was averaging in early 1590s history plays, and likewise its rate of rhyming couplets, and especially the scene-ending rhyming couplet, which was a Jacobean practice. The proportion of caesuras falling after the sixth syllable in Shakespeare's 1590s plays varies from 16.9 per cent of lines (3 *Henry VI*) to 23.1 per cent (*Julius Caesar*), whereas *Thomas of Woodstock's* proportion is 32.2 per cent, far outside of Shakespearian practice; not until well into the 1600s does Shakespeare's caesura practice start to resemble that of *Thomas of Woodstock*. These figures derive from use of punctuation, but more reliable is measurement taken from the splitting of a verse line between characters, for that phenomenon is more certainly authorial. Using just these more reliable data, the results point the same way: the pause patterns in *Thomas of Woodstock* are

unlike Shakespeare's 1590s practices, and indeed unlike anyone else's 1590s practices. (In fairness, caesuras falling in the breaks where verse lines are split between speakers are still not entirely reliable evidence, since the early editions have no consistent way of marking such shared verse and it has to be inferred by modern editors.)

There is yet more evidence on Jackson's side and against Egan's. *Thomas of Woodstock* and Rowley's *When You See Me You Know Me* have strikingly high frequencies of polysyllabic rhyming, such as *tyranny/eternally*, which frequencies are considerably higher than those found in Shakespeare's 1590s plays, even the ones that are full of other kinds of rhyme. Mid-speech rhyming couplets are frequent in *Thomas of Woodstock* at 112 (out of 492 rhyming lines), while Shakespeare used only 116 in his entire career. *When You See Me You Know Me* has ninety-eight such mid-speech rhyming couplets out of 454 rhymed lines and so is much like *Thomas of Woodstock*. Other features such as two rhyming couplets separated only by a line of blank verse connect *Thomas of Woodstock* and *When You See Me You Know Me*, their having eighteen and seven respectively, and are extremely rare in Shakespeare. Likewise, assonantal rhymes are rare in Shakespeare and common in *Thomas of Woodstock* and *When You See Me You Know Me*. The Rowley rhyming features are also prominent in the additions that he and William Birde contributed to *Doctor Faustus*. Early Shakespeare is fond of *-eth* endings (as opposed to *-es*) for verbs in the third person singular, but *Thomas of Woodstock* almost entirely uses the more modern *-es* endings. These linguistic features cannot be accounted for by saying that *Thomas of Woodstock* was an early 1590s play revised in the early 1600s, as they are too deeply embedded: the whole thing would have to be rewritten to put them in. The fact that they are also found in Rowley's *When You See Me* makes him the likely author and 1598–1609 the likely date.

The second overlooked article from *Research Opportunities in Medieval and Renaissance Drama*, 'New Research on the Dramatic Canon of Thomas Kyd' (*ROMRD* 47[2008] 107–27), is also by Jackson, and in it he argues that Brian Vickers's methodology for adding *Arden of Faversham*, *King Leir*, *Fair Em*, and bits of *1 Henry VI* to the canon of Thomas Kyd is fatally flawed and the attributions are false. Vickers's attribution appeared in the *Times Literary Supplement* and was reviewed in *YWES* 89[2010], covering work published in 2008, where its methodological weakness was overlooked by this reviewer. Vickers first set out to find the three-word collocations, 'triples' he called them, in the unattributed plays and in the known Kyd canon of *The Spanish Tragedy*, *Soliman and Perseda*, and *Cornelia*; having found them he sought to discover how common these triples are in the rest of the pre-1596 drama. (Or rather, not all the pre-1596 drama but the seventy-five plays that he has electronic texts of.) A substantial number of the triples that link *Arden of Faversham* to the Kyd canon are found in no other play. As Jackson points out, the mistake is in first finding the shared triples and only then looking to see how often they occur elsewhere, since a certain number of them are bound to be common only to *Arden of Faversham* and one other playwright's canon. Had Vickers look for triples shared by the *Arden of Faversham* and the Marlowe canon and then excluded all those that also appear in other men's plays, he would have been bound to likewise get a residue of triples unique to



*Arden of Faversham* and Marlowe, but this does not mean that Marlowe wrote the play.

Vickers used plagiarism detection software to pick up the triples within the Kyd canon, which is smart as such software is intended to find approximate (sometimes called fuzzy) matches as well as perfect ones, and hence does not require the matches to have identical spelling. But when hunting these triples in the rest of the canon Vickers used simple string-searching software that demands perfect identity, so he probably missed a few matches because of spelling differences. Jackson repeated Vickers's methodology, but first searched for triples common to *Arden of Faversham* and the known Shakespeare play *2 Henry VI*, using modernized, regularized texts and a different plagiarism package, and then hunted in LION for the same triples occurring in plays from 1580 to 1596. (LION's 'variant spelling' feature is not perfect, he points out, since it misses the common spelling of *hart* for *heart*.) In any case LION found all the matches identified by Vickers, and 437 triples shared by *Arden of Faversham* and *2 Henry VI*. Of these 437, fifty are unique to these two plays and a further six are found only in *Arden of Faversham*, *2 Henry VI*, and another Shakespeare play. Thus *Arden of Faversham* has fifty-six unique matches with *2 Henry VI*, more than the number of unique matches with any one of Kyd's plays. Jackson interrogates Vickers's list of triples unique to *Arden of Faversham* and the Kyd canon, which list is supposed to be on the website of the London Forum for Authorship Studies, but was not there when this review was written in November 2010. Jackson works through Vickers's list, whittling away entries that in fact appear outside *Arden of Faversham* and the Kyd canon but were overlooked, for example because Vickers was searching for exact strings, not for words in their variant spellings. This whittling leaves twenty-one collocations unique to *Arden of Faversham* and *The Spanish Tragedy*, twenty-nine unique to *Arden of Faversham* and *Soliman and Perseda*, and four unique to *Arden of Faversham* and *Cornelia*. None comes close to the fifty-six collocations that Jackson found to be unique to *Arden of Faversham* and *2 Henry VI*.

Jackson repeats the process for *Arden of Faversham* and *The Taming of the Shrew* and finds forty-four unique matches, and four more that appear only in these two plays plus another Shakespeare play. Thus Vickers's methodology is useless for proving that *Arden of Faversham* was written by Kyd, since it can also—with greater strength of evidence—be used to show that Shakespeare wrote *Arden of Faversham*. Because Vickers's own tables (publicly available when Jackson was writing) show the numbers of triples shared by plays in his new expanded Kyd canon (that is, *The Spanish Tragedy*, *Soliman and Perseda*, *Cornelia*, *Arden*, *King Leir*, *Fair Em*, and bits of *1 Henry VI*) and the numbers of these triples that do not occur elsewhere in the drama of 1580–96, Jackson is able to calculate the proportion of unique matches—those not appearing outside Kyd—as a percentage of the total matches shared amongst the Kyd canon. Topping the list are the uncontroversial Kyd plays, *The Spanish Tragedy*, *Cornelia*, and *Soliman and Perseda*. Why should they have more unique shared links than the other plays? The obvious answer is that they really are Kyd plays and the ones further down the list are not. A statistical procedure called Wilcoxon's signed-rank test shows that it is most unlikely

that coincidence would put these three plays at the top of the list if all seven plays were by the same person. Jackson dices the data several ways and the outcome is always the same: the three plays definitely by Kyd—*The Spanish Tragedy*, *Soliman and Perseda*, and *Cornelia*—have stronger links with one another than with the four Vickers claimants for Kydness. Jackson dices the data yet another way, looking at unique matches per 1,000 lines, and the results are the same: the accepted Kyd plays are like one another and the ones Vickers wants to add to the Kyd canon are unlike them.

Yet a third Jackson article was overlooked in previous *YWES* reviews: 'Is "Hand D" of *Sir Thomas More* Shakespeare's? Thomas Bayes and the Elliott-Valenza Authorship Tests' (*EMLS* 12:iii[2007] n.p.). In it he argues that there are flaws in the tests by Ward E.Y. Elliott and Robert J. Valenza that deny Shakespeare's composition of 'Hand D + Addition III' of *Sir Thomas More* and the bits of *Edward III* normally attributed to him. (Elliott and Valenza's tests were also critiqued by Thomas Irish Watt in his essay reviewed above.) Jackson thinks Elliott and Valenza did not apply the correct procedure for working out how likely it is that something is the case when the test for that something is known to be less than perfect. Jackson explains the statistics of Bayes' Theorem, which is appropriate to such cases yet produces decidedly counter-intuitive results. Suppose that in cancer screening a test is 79 per cent reliable, meaning that when the patient has a cancer the test will indicate this 79 per cent of the time, and hence will falsely indicate no cancer (despite there being one) 21 per cent of the time. Suppose also that for cancer-free patients the test indicates this freedom 90 per cent of the time, and (falsely) comes back positive for cancer 10 per cent of the time. Finally, suppose that the actual rate of cancer in the population is 1 per cent. If I take the test and it comes back positive, what is the likelihood that I really have cancer? Most people guess that it is highly likely I have cancer, but in fact it is most unlikely. The formula relies on two statistics. The first is the rate-of-true-positives times by the real-cancer-rate, so here  $0.79 \times 0.01 = 0.0079$ . The second is the rate-of-false-positives times the real-no-cancer-rate, so here  $0.1 \times 0.99 = 0.099$ . (The real-no-cancer-rate is 1 minus the rate of cancer in the population, in other words the proportion of the human population not suffering cancer.) The Bayes formula divides the first of the above statistics (0.0079) by the sum of the two statistics (0.1069), which gives 0.074. So in fact if this test indicates that I have cancer, there is only about a 1 in 13 chance I really have it.

Elliott and Valenza give figures for how often their tests declare something to be by Shakespeare when it is by Shakespeare and when it is not by Shakespeare, the true positives and false positives, and how often they declare something to be not by Shakespeare when it is by Shakespeare and when it is not by Shakespeare, the false negatives and true negatives. But we lack one number, equivalent to the real-cancer-rate: what is the actual probability—independent of the test—that Hand D is by Shakespeare? It seems odd, of course, that in order to establish how likely it is that Hand D is by Shakespeare we must first put a figure on how likely it is that Hand D is by Shakespeare. Jackson's idea, though, is to plug into the formula various estimates of this likelihood—reflecting the range of scholarly opinion—in order to determine

how likely it is that Elliott and Valenza's tests give a false verdict. Jackson summarizes the purely palaeographical evidence that Hand D is Shakespeare's composition, and mentions that LION confirms that the unusual spellings in Hand D of *scilens*, *iarman*, *elamentes*, *a levenpence*, *deule*, and *argo* are extremely rare outside of Shakespeare. A lot of the highly odd spellings in Hand D are absent from all printed works in LION but present in a few manuscripts, and they seem to have been considered old-fashioned in the 1590s. Thus, the reason that they do not occur in other Shakespeare works is probably that those works are printed and these spellings were routinely modernized by compositors. It is well known (and apparent to the casual reader) that there are thematic and imagery links between Hand D and Shakespeare's works.

Overall, and in the light of all this independent evidence pointing the same way, Jackson reckons the likelihood of Hand D being by Shakespeare is 99.9 per cent. Putting this number into Bayes' Theorem shows that, on this assumption, it is 97.8 per cent likely that Hand D is by Shakespeare despite the fact that it fails Elliott and Valenza's tests. It is hard not to respond to this by saying that of course if one starts with a near-certainty of Shakespearian authorship as one's premise, one is bound to conclude that Elliott and Valenza's demurrals are wrong. Jackson tries lowering the initial likelihood to 99 per cent and finds that this makes the chance that Elliott and Valenza are wrong fall to around 80 per cent, and with 95 per cent as the initial likelihood it drops to about even money, 50:50. Starting with what Jackson thinks is the highly sceptical view that the likelihood of Hand D being by Shakespeare is only 80 per cent, the chance that the tests are wrong and it really is by Shakespeare drops to 15 per cent.

Jackson examines closely one of Elliott and Valenza's tests that Hand D fails, which is called 'grade level', meaning the complexity of the writing as measured by determining what grade (year) a person would have to reach in (presumably American) school in order to be able to understand it. He shows that the software used to calculate this might well count numbers of words wrongly and points out that because Hand D is unlike all other Shakespeare in being a foul papers manuscript, where quite a few things seem not to be fully worked out, the test for complexity might be thrown off. The other of Elliott and Valenza's tests that Hand D fails is based on two lists of high-frequency words that are (list *a*) more frequent in *Macbeth* than in Middleton's *The Witch* and (list *b*) less frequent in *Macbeth* than in *The Witch*. Elliott and Valenza established upper and lower frequency limits—within which a sample must fall to be declared Shakespearian by this test—by running all the Shakespeare plays through the test. That is, they counted the total of list *a* words in the play, subtracted the total of list *b* words, then divided this by the total number of occurrences for both lists, and they finally scaled up the answer to avoid dealing with small decimal fractions. Jackson notices that this test is highly sensitive to genre, with tragedies and histories rating highly, so a tragical history such as *Sir Thomas More* would rate highly in any case. Jackson also objects to Elliott and Valenza's tests being created from analysis of just seven plays, which arose because some of their procedures depend on linguistic data that are available only for these plays. Finally, he objects to the

tests being developed and refined alongside their application. The better procedure is to perform a blind process of developing sets of Shakespearian and non-Shakespearian writing by randomized sampling and then calibrating the tests once it has been determined how well they attribute authorship under conditions that prevent the investigator unconsciously tipping the scales.

So, finally, to *Notes and Queries*. Christopher Mead Armitage argues that Dr Caius in *The Merry Wives of Windsor* is indeed a mockery of the historical figure Dr John Caius (1510–73) of Gonville and Caius College in ‘Dr Caius: Cambridge Scholar, Shakespearian Buffoon’ (*N&Q* 56[2009] 46–8). Todd Pettigrew made an argument (reviewed in *YWES* 81[2002], covering work published in 2000) that the play’s Caius was supposed to be someone impersonating the historical figure. The play alludes to the real Caius’s reputation as a stickler for pronunciation—hence the fictional one is a terrible mispronouncer of English—and to his views on the Latin and Greek pronunciation to be taught to boys, hence William’s Latin lesson in the play. The historical figure also published a book on English dogs, the classifications of which match those in Macbeth’s speech to the two murderers (III.i.93–102).

Thomas Merriam has three notes this year. In the first, ‘Six-Word Collocations in Shakespeare and *Sir Thomas More*’ (*N&Q* 56[2009] 48–51), he argues that Shakespeare contributed not merely the Hand D and Addition III parts of the play *Sir Thomas More*, but was also one of the authors of the original text that is in Munday’s hand in the manuscript. Merriam has found seven of what he calls six-word collocations in *King Lear* that first occurred in previous Shakespeare plays, and five of these seven are unique to Shakespeare’s canon while the other two are very rare (up to five other occurrences) outside Shakespeare. (The notion of collocation is usually indifferent to word order—so *blue skies* and *skies blue* would count as a match—but Merriam’s matches comprise six words in a particular order.) Merriam has found two seven-word collocations and two six-word collocations that are common to *1 Tamburlaine* and *2 Tamburlaine* but are not found in Shakespeare. He has also found six six-word collocations that *The Comedy of Errors* shares with other Shakespeare plays, and of these two are unique to Shakespeare while the other four are very rare (up to three other occurrences) outside Shakespeare. The point he is making (none too clearly) is that such long collocations are a good test of authorship as they are fairly frequently shared by works in one writer’s canon—because he self-plagiarizes—but are rarely shared across canons.

The Munday section of *Sir Thomas More* shares one six-word collocation with Munday’s *John a Kent and John a Cumber*, and shares it also with *1 Henry IV* and *The Two Gentlemen of Verona* and more than five other works (he lists only five), so it is not very rare. Munday’s *John a Kent and John a Cumber* shares two six-word collocations with the thirty-six plays in the Shakespeare Folio. But, and this is the important point, the Munday section of *Sir Thomas More* shares seven six- to eight-word collocations with Shakespeare’s plays, of which four are not found anywhere else and of which three are very rare (each having only one occurrence) outside Shakespeare. One of these seven is tricky as it is ‘God save the King! God save the King!’ being used twice (twenty-three

lines apart) within one scene of the Munday section of *Sir Thomas More*, and being used twice (seventy-four lines apart) in two successive scenes in *2 Henry VI*, which repetition happens in only one other work Merriam has found. Merriam does not make clear that it is the double exclamation being repeated that constitutes the collocation, and he muddies the waters by also indicating which other works use 'God save the King! God save the King' without repeating it. This example is rather unlike his other six collocations. Of Merriam's seven collocations, two have a further connection: the two lines containing them are adjacent in the Munday section of *Sir Thomas More* and their corresponding lines in Shakespeare are not only in the same play, *2 Henry VI*, but are just ten lines apart in it. In a sense, then, these collocations themselves collocate, which is most rare. Merriam concludes that either this is all extraordinary coincidence, or the writers of the Munday section of *Sir Thomas More*, supposedly Munday and Chettle, borrowed from Shakespeare or vice versa, and perhaps by ear since the plays were unpublished. (The uncertainty about the direction of borrowing arises from uncertainty in dating *Sir Thomas More*.) Or, more likely still, Shakespeare was one of the writers of the whole of *Sir Thomas More*.

Merriam's second note, 'Feminine Endings in *King John*' (*N&Q* 56[2009] 576-8), argues that Shakespeare co-wrote *King John* with a person or persons unknown. Rates of usage of feminine endings in blank verse are an important statistic for stylometricians, but unfortunately there is no agreed way to count them. For example, does a weak ending caused by the last syllable of a proper noun count? Merriam tabulates the counts for the first sixteen Shakespeare plays (in chronological order), as counted by six scholars, and although there are discrepancies there emerges enough correlation to say that feminine ending counts are sufficiently agreed upon for meaningful analysis to use them. Merriam presents a diagram containing curiously shaped polygons that he says show 'the two central quartiles of the Shakespeare feminine ending counts for the sixteen plays' (p. 577) in the preceding table. The vertical axis shows the rate of feminine-ending use (from 0 to 15 per cent of all lines) and the horizontal axis represents the different scholars' counts, so the polygon for E.K. Chambers's counts stands horizontally adjacent to the one for Elliott and Valenza's counts. Thus the widths of the polygons have in fact no meaning: these quartiles should be lines running vertically to represent minima and maxima, not overlapping polygons. The diagram shows that everyone puts Shakespeare's feminine-ending use above 5 per cent and Merriam adds in P.W. Timberlake's counts that show that across forty-nine non-Shakespearean plays of the same period the rate of feminine-ending use is below 5 per cent. Thus feminine endings are a good discriminator of Shakespearean from non-Shakespearean drama. In his book *Co-Authorship in King John* (reviewed in *YWES* 88[2009], covering work published in 2007) Merriam used different criteria to divide the Shakespearean and non-Shakespearean parts of the play. Merriam now shows that feminine-ending use in what he ascribed the non-Shakespearean parts falls below the 5 per cent threshold and in the Shakespeare parts falls above it, so this is an independent confirmation of his conclusion in that book.

In his third note, 'Marlowe Versus Kyd as Author of *Edward III* I.i, III, and V' (*N&Q* 56[2009] 549–51), Merriam tries to show that the non-Shakespearian parts of *Edward III* were not written by Kyd. The word *the* occurs more often in Marlowe's work than in Kyd's, and the words *but, for, I, me, and not* occur less often in Marlowe's work than in Kyd's. By counting occurrences of these function words in the non-Shakespearian parts of *Edward III* Merriam hopes to work out if Marlowe or Kyd wrote them. In other words, assuming that one of these two men wrote the non-Shakespearian parts of *Edward III*, which is it? He is not aiming to establish that either of them is the actual author, and, as we saw above, Timothy Irish Watt offers evidence to reject both. Counting the frequencies of these words in the non-Shakespearian parts of *Edward III*, the values are typical of Marlowe not Kyd. Principal Component Analysis enables the plays to occupy positions on a graph if we represent the first component along one axis and the second along the other. On such a graph, the proximity of the non-Shakespearian parts of *Edward III* to Marlowe's works is clear, as is their distance from Kyd's works. Merriam claims that the same result is obtained if other function words are used in place of his six. Thus the non-Shakespearian parts of *Edward III* are not by Kyd, as Brian Vickers claims. Merriam stops short of claiming that these parts of *Edward III* were written by Marlowe, since obviously any number of writers (except Kyd) might have word-usage habits that are closer to those of Marlowe than those of Kyd.

Brett D. Hirsch, in 'Rousing the Night Owl: Malvolio, *Twelfth Night*, and Anti-Puritan Satire' (*N&Q* 56[2009] 53–5), discerns in Sir Toby's singing to 'rouse the night-owl' and draw three souls out of one weaver (*Twelfth Night* II.iii.57–8) a reference to Malvolio, since *owl* (a roundhead) and *weaver* (a Flemish Calvinist refugee of that profession) were slang terms for Puritans. Dennis McCarthy, in 'A "Sea of Troubles" and a "Pilgrimage Uncertain": *Dial of Princes* as the Source for Hamlet's Soliloquy' (*N&Q* 56[2009] 57–60), finds in Thomas North's translation of Antonio de Guevara's *Dial of Princes* [1557] a source for Hamlet's 'To be . . .' speech. We know that Shakespeare used whole phrases, as well as the stories, from North's translation of Plutarch, and McCarthy thinks that in *Dial of Princes* Shakespeare found the ideas of death as sleep, as a pilgrimage, as a place from where no one returns, and as an escape from suffering. (These ideas seem rather too commonplace to count as sources.) More specifically, North uses the phrases 'sea of troubles' and 'of so long life' that appear in Hamlet's speech, and also collocates the words *sleep, perchance, and dream*. McCarthy goes looking for these words and phrases in others' writing using EEBO (he means EEBO-TCP) and discovers that 'sea of troubles' is not rare: fifteen other works of the period use it. But 'of so long life' is fairly rare: only three other works use it. The collocation of *sleep, perchance, and dream* is, according to McCarthy, unique to *Hamlet* and *Dial of Princes*, and the order of ideas in Hamlet's speech also follows that of *Dial of Princes*. McCarthy is mistaken about the collocation. He reports that his EEBO-TCP search term was 'sleep near perchance near dream', and applied to EEBO-TCP at the time of writing of this review this search term also hits John Florio's *First Fruits* [1578], which contains 'if perchance thou aske me, because thou hast dreamed it, sleping'. Perhaps the

Florio book was added to EEBO-TCP after McCarthy looked. Scholars should be aware, and make clear in their work, that the searchable texts of EEBO-TCP currently represent only about 20 per cent of EEBO, but the percentage is rising all the time as more works are keyboarded.

Brian Vickers, 'Shakespeare or Davies? A Clue to the Authorship of "A Lover's Complaint"' (*N&Q* 56[2009] 62–3), thinks that 'A Lover's Complaint' is by John Davies of Hereford, not Shakespeare, despite being published in the same volume as *Sonnets* [1609], but MacDONALD P. JACKSON has evidence that Vickers is wrong. In 'A Lover's Complaint' there are three uses of the article *a* instead of *an* before a word beginning with *h*: *a hill*, *a hell*, *a heart*. Shakespeare always put *a* not *an* before these words whereas in his known works Davies prefers *a* over *an* half the time for *a/an hell*, one time in six for *a/an hill*, and one time in ten for *a/an heart*. Thus the chance that Davies would write *a hill*, *a hell*, *a heart* together in one work is half a sixth of a tenth, or 1 in 120. Jackson reports that he used an online concordance, but the printed URL has been truncated and does not work (p. 63, n. 3); the correct URL is <http://www.it.usyd.edu.au/~matty/Shakespeare/test.html>. Jackson does not report which edition of Shakespeare this concordance is based upon. I repeated Jackson's searches using an electronic text of the 1863–6 Cambridge–Macmillan edition, which produced a few more hits for *a hill*, *a hell*, *a heart* than Jackson counts, and none for *an hill*, *an hell*, *an heart*, so his case is not weakened. In a second note, 'Arcite's Horsemanship: A Reading in *The Two Noble Kinsmen*, II.v.13' (*N&Q* 56[2009] 605–7), Jackson considers Arcite's boast of his 'feat in horsemanship' (II.v) in the 1634 quarto of *The Two Noble Kinsmen*. It is an odd phrase and editors would be tempted to emend to *feats* were not the singularity emphasized in the next line ('it was'). Yet *feat* cannot easily mean *ability*. No one seems to have noticed that when the play appeared in the second Beaumont and Fletcher Folio [1679] the line was 'seat in horsemanship', which makes much better sense and a simple long s/f confusion would explain Q's reading.

Roger Stritmatter, in 'Shakespeare's *Ecclesiasticus* 28.2–5: A Biblical Source for Ariel's Doctrine of Mercy' (*N&Q* 56[2009] 67–70), reckons that one passage from *Ecclesiasticus* on the subject of mercy is a Shakespearian source. Unfortunately, Stritmatter's sense is harmed by what seems to be a printing error on page 68, for a paragraph ends 'To these must be added, although previously undetected in the secondary literature, a line from *Romeo & Juliet*:' and the reader does not find out what the line is. Lord Say's attempt to dissuade the rebels in *2 Henry VI* by saying that to receive mercy from God they must show mercy to their fellow men (IV.vii), Henry V's similar reproach to the conspirators Cambridge, Scroop, and Grey (*Henry V* II.ii), Ariel's reproach to Prospero for lacking mercy (*The Tempest* V.i), and Prospero's epilogue about the audience's hopes to be pardoned, are based on *Ecclesiasticus* 28:2–5, according to Stritmatter. He admits, though, that the necessity for reciprocal forgiveness appears throughout Shakespeare's work and need not have just one source. Stritmatter shares the common misapprehension that Jack Cade says 'kill all the lawyers' in *2 Henry VI*, but this is Dick the Butcher's line (p. 69, n. 21). Howard Jacobson, in '*King Lear* I.i.271–2' (*N&Q* 56[2009] 63), reckons that Cordelia's 'the jewels of our father' (said of

her sisters when parting from them in the first scene) comes from the story of Cornelia, the mother of the Gracchi, as told by Valerius Maximus, who called her sons her jewels. Jacobson neglects to name the Roman source, giving only the cryptic reference 'Val. Max.4.4 praefatio', but it is Maximus's *Factorum Dictorumque Memorabilium* (*Memorable Words and Deeds*). As well as being a famous anecdote, the story of Cornelia saying this is in Robert Burton's *Anatomy of Melancholy*.

Katherine Duncan-Jones, in 'Shakespeare, Guy of Warwick, and Chines of Beef' (*N&Q* 56[2009] 70–2), finds that Shakespeare's knowledge of the legend of Guy of Warwick is apparent in his plays. In *King John* I.i the Bastard responds to being called 'good Philip' with, in the Folio's styling, 'Philip, sparrow'; editors vary their styling and punctuation according to their sense of what this means. Duncan-Jones thinks this an allusion to the play *Guy of Warwick*, since Colbrand the Giant, who was killed by Guy, is mentioned by the Bastard six lines earlier, and Sparrow is the name of Guy's attendant clown in the play. Shakespeare's *Henry VIII* V.iii also alludes to Guy of Warwick in the lines 'I am not Samson, nor Sir Guy, nor Colbrand . . . Let me ne'er hope to see a chine again— | And that I would not for a cow, God save her!' In the legend and the play of it, Guy slew the enormous Dun Cow of Dunsmore Heath and its huge rib was hung up in Warwick Castle. We know there was such a huge bone on display in Warwick Castle in the middle of the sixteenth century, and Duncan-Jones thinks this bone prompted the chine mentioned in *Henry VIII* in the context of an imagined contest between adversaries of unequal size. Another unequal fight featuring the word chine is between hungry Jack Cade and Alexander Iden in the latter's garden in *2 Henry VI*. Duncan-Jones reckons Shakespeare may have seen the Dun Cow bone at Warwick Castle and that he would also have been familiar with such sights from the slaughterhouses of Stratford-upon-Avon.

Boris Borukhov, in 'Was the Author of *Love's Martyr* Chester of Royston?' (*N&Q* 56[2009] 77–81), shows that the Robert Chester who wrote *Love's Martyr* was not Robert Chester of Royston, as is often claimed. *Love's Martyr* was published as 'by Robert Chester' in 1601 with Shakespeare's poem 'The Phoenix and the Turtle' alongside it, and there are two candidates for its authorship: Robert Chester of Royston in Hertfordshire and Robert Chester of Denbighshire. The latter is not known to have existed, but is inferred from the name in *Love's Martyr* and the book's dedicatee, John Salusbury, being from there. The recent Arden3 edition of Shakespeare's poems, edited by Katherine Duncan-Jones and H.R. Woudhuysen (reviewed in *YWES* 88[2009], covering work published in 2007) favoured Robert Chester of Royston, but Borukhov lists a number of reasons why he is not likely to be the man. He was not closely associated with John Salusbury, he was not Welsh (there is evidence of Welsh pronunciation in *Love's Martyr*), his signature does not match the one of the Robert Chester who we know was connected to Salusbury, he was an esquire (whereas the author of *Love's Martyr* would have boasted of that if he were), and he was Salusbury's equal (whereas the author of *Love's Martyr* makes clear his dependence on Salusbury).

Continuing his work on the 1821 Boswell–Malone edition of Shakespeare, Arthur Sherbo published three notes this year. In the first, 'More on the Bible



in Shakespeare' (*N&Q* 56[2009] 270-4), he lists a collection of Shakespeare's uses of biblical terms and ideas that do not appear in Naseeb Shaheen's standard works on the topic but are recorded by editors cited in the Boswell-Malone edition. Most are really quite tenuous and we need not think that Shakespeare was consciously echoing Scripture. The second note, 'Pope and Gray: Gleanings from the 1821 Boswell-Malone *Shakespeare*' (*N&Q* 56[2009] 274-6), lists echoes of Shakespeare found in poems by Alexander Pope and Thomas Gray, as noted in the Boswell-Malone edition, and the third, 'Military Language in Shakespeare' (*N&Q* 56[2009] 607-10), lists some military language that Sherbo considers well glossed in Boswell-Malone but not in modern editions nor in Charles Edelman's dictionary on the subject. William Sayers, 'Two Etymologies: *Inkle* and *Natty*' (*N&Q* 56[2009] 350-4), traces the etymologies, unknown to *OED*, of the words *inkle*, a linen tape mentioned in a few Shakespeare plays, and *natty*, which has no Shakespearian uses.

Daniel Pollack-Pelzner, in "'Another Key" to Act Five of *A Midsummer Night's Dream*' (*N&Q* 56[2009] 579-83), decides that, as in the Folio, Egeus should be the manager of mirth in the last act of *A Midsummer Night's Dream*, rather than Philostrate as the 1600 quarto has it. Both versions have Philostrate be the man ordered to manage the merriment in the first act, but according to Pollack-Pelzner Egeus performs a kind of prologue function in saying 'Stand forth Demetrius . . . Stand forth Lysander' in the first scene and saying 'this is my daughter . . . this Lysander; this Demetrius . . . This Helena' when he finds the lovers sleeping in IV.i. Egeus keeps bringing in a tragic tone to disrupt Theseus's desire for a comic atmosphere—in the first scene, and when the hunt comes across the lovers—so it has to be him whom Theseus overrules regarding the lovers and regarding the choice of play-within-the-play. The titles of possible entertainments that he lists embody the genre confusion the play is concerned with. In *Hamlet* III.ii Hamlet calls Polonius 'so capitol a calf' when he refers to acting the role of Caesar in a play where he was killed by Brutus. As well as alluding to the play *Julius Caesar* (in which the actor of Polonius may well have played Caesar), Steven Doloff, in 'Killed Behind the Curtain: More on Hamlet's Calf Allusion' (*N&Q* 56[2009] 583), thinks the joke alludes to the itinerant shadow-play entertainment of killing a calf behind a curtain, because Polonius himself will soon die that way. When Sir Toby says 'they have been grand-jurymen since before Noah was a sailor' (*Twelfth Night* III.ii), Horst Breuer thinks he is referring to Fabian's family background as a local magistrate with a long and distinguished pedigree, rather like Shallow and Silence in *2 Henry IV*; such a man would plausibly organize a bear-baiting and resent the new, upstart servant ('"They have been grand-jurymen since before Noah was a sailor": A Note on Shakespeare's *Twelfth Night*, III.ii.12-13' (*N&Q* 56[2009] 584-5)).

Cheering up Cordelia as they are being taken off to prison, Lear says in F 'The good yeares shall deuoure them' and it is not clear who 'them' are, nor what he means by 'good yeares'. Editors have suggested a number of emendations for 'good yeares', but Stephen Rollins, in 'The Good Years in *King Lear* V.iii.24' (*N&Q* 56[2009] 585-8), thinks F's reading is fine: Lear is reversing the events of Pharaoh's dream from Genesis 41 in which the seven

lean cows (seven bad years) eat the seven fat cows (the seven good years); in reversal the good shall eat the bad. Confused after his capture by Cordelia's men, Lear asks if he is in France. This means not only that he remembers that Cordelia married the king of France, but also that he remembers the play's sources, for as Heather Hirschfeld points out in "'Am I in France?": *King Lear* and Source' (*N&Q* 56[2009] 588–91), in all the sources Lear goes to France to meet Cordelia. Hirschfeld finds it a metatheatrical comment, reminding the audience of the sources precisely because Shakespeare is going to depart from them in order to create a tragic ending.

Oliver R. Baker's note, 'Duncan's Thanes and Malcolm's Earls: Name Dropping in *Macbeth*' (*N&Q* 56[2009] 591–5), cannot properly be summarized because this reviewer does not understand it. Holinshed's *Chronicles* names eight thanedoms turned into earldoms by King Malcolm of Scotland: Fife, Menteth, Atholl, Leuenox, Murrey, Cathnes, Rosse, and Angus. Six of these thanes appear in *Macbeth*, and if we exclude Fife (Macduff) then Shakespeare chose five from seven, leaving out Moray (Murrey) and Atholl. Baker looks for meaning in this choice, but there is a typo in his formula for calculating how many ways there are of picking a five-person committee from a list of seven candidates. The factorial symbol (!) has been omitted after the first  $n$ , and the formula should read  $n!/(r!(n-r)!)$ . More seriously, Baker's grasp of probability is faulty. Because there are twenty-one ways to pick five men from seven, he thinks that 'the chances of Shakespeare's selections from Holinshed being random are 1 in 21, or less than 5 per cent' (p. 592, n. 6). This is like saying that the draw for the National Lottery is unlikely to be random because there was only a 1 in 10 million chance that the numbers selected would come up. Random means unguided or haphazard, not unlikely. The remainder of the note (pp. 592–5) is almost incomprehensibly written, mainly because the argument is unevenly split between the footnotes (amounting to 2,230 words) and the body text (just 1,200 words). The thrust might be that a study of Scottish and English history shows that Shakespeare was pandering to contemporary aristocratic sensibilities in leaving out Moray and Atholl.

Richard M. Waugaman, in 'The Sternhold and Hopkins *Whole Book of the Psalms* is a Major Source for the Works of Shakespeare' (*N&Q* 56[2009] 595–604), thinks that there are echoes of Thomas Sternhold and John Hopkins's *The Whole Book of Psalms Collected into English Metre* [1565] in Shakespeare's *Sonnets*. The echoes he lists are faint, including common words such as *save*, or even just common prefixes such as *mis-*. In certain cases, the meaning differs in the two uses: *save* means *rescue* in the psalm and *except* in the sonnet. There are ideas in common too, but again the connections are loose, and when Waugaman finds links between the psalms and *Macbeth* and *The Rape of Lucrece* the result is equally unconvincing. Claire R. Waters, in 'The *Tempest*'s Sycorax as "Blew Eye'd Hag": A Note Toward a Reassessment' (*N&Q* 56[2009] 604–5), thinks that the handwriting of the scribe Ralph Crane, who provided the Folio copy for *The Tempest*, can help solve an old crux. Prospero calls Sycorax a 'blew ey'd hag'—Waters misquotes this as 'eye'd'—and the problem is whether to modernize to *blue* or something like *blear*. Bleared eyes certainly were associated with witchcraft, but looking

at Crane's handwriting in surviving manuscripts Waters decides that a confusion of *-ar* for *-w* is distinctly possible, and so she supports the *blear* emendation. Azar Hussain, in 'The Reckoning and the Three Deaths of Christopher Marlowe' (*N&Q* 56[2009] 547-8), thinks that the Jailer's lines in *Cymbeline*, 'A heavy reckoning for you... fear no more tavern bills' (V.v), are an allusion to Marlowe's death, as are Falstaff's 'A trim reckoning!... He that died o' Wednesday' (*I Henry IV*, V.i), since Marlowe was indeed killed on a Wednesday. This last claim seems improbable. For members of one generation President Kennedy's assassination is supposed to be highly memorable, and for another 11 September 2001 is indelible. Who remembers that these were a Friday and a Tuesday respectively?

## 2. Shakespeare in the Theatre

Erica Sheen's *Shakespeare and the Institution of Theatre: 'The Best in this Kind'* (what is the point of that subtitle?) considers the emergence of the professional theatre during the Elizabethan period and its eventual arrival at a position of cultural invulnerability: by 1601, Shakespearian theatre 'was above political accountability' (p. 109). Jacobean patronage puts this establishment beyond contention: 'Shakespearean theatre had achieved generic institutional status in the London theatrical scene' (p. 112). This is a brave book in as much as it challenges the last thirty-odd years of new historicist and cultural materialist scholarship which, in spite of their differences, share (broadly) the contention that early modern theatre was occasionally, potentially, incipiently subversive. As we know, the Elizabethans were a litigious lot and Sheen charts the formation of the institution of professional theatre in the light of complicated debates to do with family law, property law, monopolies, and contract. This is a complicated and (it must be said) not always interesting technique. At the end of a particularly murky summary of property and right, title and possession, phrased in terms of a series of questions, Sheen offers the lacklustre 'And so on' (p. 114). Occasionally the book is just plain hard going. As she discusses the connections between property law and the actor-audience relationship in *The Merchant of Venice*, she asserts that it is 'not simply that Shakespeare did not observe strict principles of continuity. It is, rather, that these complex spatio-temporal articulations create a continuum of dramatic duration within which Antonio's stipulation of Bassanio's witness of the embodied display of his ruin maps the term of Shylock's "merry bond" and the conditions for its satisfaction onto the organisation of the text and the audience's experience of the play' (pp. 86-7). It is not a user-friendly sentence, and indeed much of the book occludes the already foggy world of early modern law with a critical discourse that is neither concise nor lucid. Her assertion is that 'in Shakespeare's plays, legal thinking opens an intellectual threshold for the creation of a special property in theatre' (p. 48). For instance, *The Taming of the Shrew* dramatizes 'the contrast between love as industry and marriage as conveyance' (p. 50), while in the case of *Richard III* 'the principle of beneficial ownership provided the basis of a fully fledged principle of dramatic action' (p. 71). But in places such suggestive propositions are

and George Buchanan's Latin translation of the *Alcestis* [1539]. Persuasively suggesting that the statue motif, along with Perdita's name, have their origins in Buchanan's *Alcestis*, Dewar-Watson rightly calls for greater critical attention to the influence of neo-Latin writers on Shakespeare.

Phebe Jensen similarly situates *The Winter's Tale* in its literary contexts, but in the concluding chapter of her monograph *Religion and Revelry in Shakespeare's Festive World* she focuses on contemporary Protestant recuperations of festivity. In 'Singing Psalms to Hornpipes: Festivity, Iconoclasm and Catholicism in *The Winter's Tale*', she returns to a topic that animated so much of last year's scholarship on the play—its Catholic subtexts. Reading the play's exploration of festivity against contemporary anti-Catholicism, Jensen identifies Leontes and Perdita with iconoclasm, but suggests that an 'antidote' is provided by the festive celebrations in Act IV and the statue scene in Act V. She thus argues that the play explores 'the dangers of Protestant iconoclasm' and endorses 'exactly those aspects of traditional pastimes—their link to a Catholic past, and affinity with Catholic forms of ritual behavior—that had been mostly purged from customary pastimes' (p. 212). In refocusing attention on the festivities in Act IV, and offering an illuminating analysis of that scene and its importance in the play, Jensen uncovers in *The Winter's Tale* and Shakespeare's other festive plays a subtle sympathy for Catholicism.

In bringing my review of the year's work on Shakespeare's late plays to a close, I would like to mention Nicholas Tredell's *Shakespeare's Late Plays: Pericles, Cymbeline, The Winter's Tale, The Tempest*, which is part of Palgrave's Readers' Guides to Essential Criticism series. In line with other books in the series, Tredell aims to provide easy access to criticism on Shakespeare's late plays, and he charts critical responses to the plays from the eighteenth, nineteenth, twentieth, and into the twenty-first centuries, concluding with some speculations on future directions of research. Although Tredell mentions *Cardenio*, *Henry VIII*, and *The Two Noble Kinsmen* in his introduction, they are excluded from detailed discussion, apparently on the basis that they have 'never excited much critical interest' (p. 3). Tredell seems to connect this to the fact that they were co-written with John Fletcher, although at the same time he acknowledges the collaborative authorship of *Pericles*, which he suggests may be the reason for its exclusion from the First Folio. *Shakespeare's Late Plays* ably guides the student through criticism on some of the late plays, although its decision to exclude *Henry VIII* and *The Two Noble Kinsmen* continues to propagate the myth of 'late Shakespeare', the dismantling of which has undoubtedly represented the most exciting development in criticism on the late plays in this and recent years.

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