VII

Shakespeare

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This chapter has four sections: 1. Editions and Textual Studies; 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 Elisabetta Tarantino; section 4(b) is by Daniel Cadman; section 4(c) is by Arun Cheta; section 4(d) is by Gavin Schwartz-Leeper; section 4(e) is by Johann Gregory; section 4(f) is by Sheilagh Ilona O'Brien; section 4(g) is by Louise Geddes.

1. Editions and Textual Studies

No major critical editions of Shakespeare appeared this year. The only relevant monograph was MacDonald P. Jackson's *Determining the Shakespeare Canon: Arden of Faversham and A Lover's Complaint*, which is an extremely well put together combination of revised versions of previously published articles, joined together with discursive connective tissue and supplemented by fresh writing. The topic is of the highest interest to Shakespearians at all levels, and Jackson's handling of it manages to convey the technical complexity—to satisfy the specialist who is entirely 'up' on the subject—without losing the newcomer to this field. The introduction (pp. 1–6) surveys the history of belief in the Shakespearian authorship of *A Lover's Complaint* and at least part of *Arden of Faversham*, pointing out that if the former is not by Shakespeare then that changes our whole view of *Sonnets* [1609] in which it appeared. For *Arden of Faversham*, Jackson's key claim is that the middle portion—Act 3 in editions that divide it that way—centred upon the Quarrel Scene (scene 8) is by Shakespeare.

Jackson's chapter 1, 'Shakespeare and the Quarrel Scene in *Arden of Faversham*' (pp. 9–39), is substantially the same as his *Shakespeare Quarterly*

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article of the same title reviewed in *YWES* 92[2013], revised lightly to make an excellent introduction to his consideration of the play, beginning with the literary-historical context before moving to his computational method. Jackson's attribution method, now widely known, admired, and emulated, is to search in Literature Online (LION) for phrases and collocations found in the text he is trying to attribute, looking for those that are comparatively rare. In the present case he confined his searches to plays first performed between 1580 and 1600 and threw away all hits that occurred more than five times across LION. What matters is how many such rare links—that is, phrases-in-common—are found between the text to be attributed and each potential author's canon as represented in LION. For the Quarrel Scene in *Arden of Faversham*, twenty-eight plays in LION contain four or more such links, and of those eighteen are by Shakespeare. Even allowing for Shakespeare's canon being larger than anyone else's, that is a compelling predominance of links to Shakespeare, with nearly two-thirds of all the links pointing to this one dramatist.

Chapter 2, 'Reviewing Authorship Studies of Shakespeare and his Contemporaries, and the Case of *Arden of Faversham*' (pp. 40–59), responds to Brian Vickers's *Shakespeare Quarterly* review of Hugh Craig and Arthur F. Kinney's 2009 book *Shakespeare, Computers, and the Mystery of Authorship* (the book was reviewed in *YWES* 90[2011]), which review was also the subject of a brilliant critique by John Burrows in *Shakespeare Quarterly* in 2012 (reviewed in *YWES* 93[2014]). Like Burrows, Jackson here patiently explains where and how Vickers is unjust in his characterizations of the scholarship in Craig and Kinney's book. Then Jackson performs his usual LION search technique, counting how many phrases and collocations are shared between the suspect text and all plays in a certain period, and tabulating those that occur not more than five times; for this the suspect text is Arden's account of his nightmare in scene 6. The vast majority of the links are with Shakespeare plays. Also, Jackson finds a tight cluster of verbal links between the nightmare story and *Venus and Adonis* lines 554–648. In his chapter 3, 'Gentlemen, *Arden of Faversham*, and Shakespeare's Early Collaborations' (pp. 66–84), Jackson notes that Shakespeare's prologue to *Henry V* and his epilogue to *A Midsummer Night's Dream* characterize their audiences as gentle and ask their pardon for his play's shortcomings, and that no other play in the period 1575–1600 besides *Arden of Faversham* does that, according to LION. Jackson goes on to reuse the evidence in Craig and Kinney's book to comment upon his own findings about *Arden of Faversham*, and in particular the links between the part of it that Jackson thinks is by Shakespeare and the parts of several collaboratively written Shakespeare plays that Craig and Kinney think are Shakespeare's; the results are highly convincing. Likewise, Jackson returns to his previous work on compound adjectives in the play (a construction that Shakespeare favoured) and finds that if we separate out scenes 4–9 (that is, Act 3) from the rest of *Arden of Faversham* it has many more of them than the rest of the play (once we normalize for length of sample), and Jackson finds spots of Shakespeare elsewhere in the play too. In sum, as Jackson puts it, 'the old evidence, when revisited, confirms the new' (p. 78). There is a useful additional check in Jackson showing that a number of words and phrases that

Shakespeare almost never uses appear in *Arden of Faversham* but only either side of, not within, the central section that Jackson claims is Shakespeare's. Jackson's chapter 4, 'Parallels and Poetry: Shakespeare, Kyd, and *Arden of Faversham*' (pp. 85–103), is substantially the same as Jackson's 2010 literary-critical article of the same title in *Medieval and Renaissance Drama in England*.

Next comes a wholly newly written chapter on 'Counter-Arguments and Conclusions' to Jackson's claim about *Arden of Faversham* (pp. 104–26). Martin Wiggins reckons that *Arden of Faversham* must be an amateur play because no professional company would demand that a boy actor have so many lines as the heroine does: 588 lines compared to, say, Juliet's 541 in Shakespeare's *Romeo and Juliet*, which is normally considered quite extraordinarily difficult a role for a boy. But as Jackson points out, in the central part of *Arden of Faversham* that is Shakespeare's work, Mistress Arden gets relatively few lines, perhaps because Shakespeare at least could see that overloading the boy would be unwise (p. 105). Wiggins also reckons that the stage directions of *Arden of Faversham* are unprofessional-sounding in using the phrase 'Here enters . . .' and often beginning, like a narrative account, with the word 'Then . . .'. Such stage directions take up the perspective not of the performers but of the audience. Jackson counters that these stage directions might not be authorial but the work of 'a reporter or scribe preparing the script for publication' (p. 105). In any case, Jackson remarks, Thomas Kyd's *Soliman and Perseda*—which, like *Arden of Faversham*, was printed by Edward Allde for Edward White—has similar audience-perspective stage directions using the word 'Then . . .'. Thus Jackson convincingly demolishes Wiggins's claim that the unusual stage directions in *Arden of Faversham* reveal an amateur writer by showing that they can be paralleled with those from the professional drama.

Jackson likewise dismisses the claim that the writer had to know the geographical area around Faversham in Kent, pointing out that the misspellings of several place names tell against it. Regarding the possibility that we are chasing a mirage in author-hunting because the author might be an unknown writer, Jackson lays out the reasons why that is unlikely. In particular, 'The extant plays of 1576–1642 constitute a very large sample (about 700) of all those that were written, and a large sample can, within a slight margin of error, provide trustworthy information about the full population' (p. 117). This means that where we have a play of unknown authorship and find that in various objective tests it matches the works of a known playwright the reason for this is more likely to be that it was written by that known playwright rather than that it was written by someone else we know nothing about.

Chapter 6, '*A Lover's Complaint*: Phrases and Collocations' (pp. 129–40), is partly based on Jackson's 2004 *Shakespeare Studies* article '*A Lover's Complaint* Revisited' reviewed in *YWES* 92[2013]. The first test applied is Jackson's standard one of finding phrases and collocations occurring no more than five times in *A Lover's Complaint* and in LION plays from the period 1590–1610. The result is that links to Shakespeare predominate, even once Jackson normalizes for just how much more Shakespeare writing there is (which, all else being equal, makes a match to Shakespeare more likely). Of the links to Shakespeare plays, the links to plays written 1603–6 predominate, and the links to non-Shakespearian plays also peak around then, so certain phrases seem to have been simply fashionable and widely used.

Jackson then turns to Vickers's ascription of *A Lover's Complaint* to John Davies of Hereford, and starting with John Jowett's demonstration that *A Lover's Complaint* stanza 1 has lots of phrases that Shakespeare used and Davies did not, Jackson extends this approach to consider stanzas 2 to 7, finding the same result. Also, even where the words used to express it differ, particular poetic conceits are shared by *A Lover's Complaint* and Shakespeare. In the new field of computational stylistics there are methodological alternatives within certain practices and we do not yet enjoy a consensus about exactly how to count various phenomena. For example, how much weight should be given to the fact that a single phrase or collocation in the work to be attributed appears multiple times in a work within a known author's canon? Should we count it once for all, or count it once each time it occurs in that known author's canon? When the results of various methods are borderline cases, such questions matter greatly, but as Jackson here demonstrates beyond any doubt, the case of *A Lover's Complaint* is not borderline: 'Whatever mode of reckoning we adopt, the affiliations of *A Lover's Complaint*'s idiolect are with Shakespeare, rather than with Davies' (p. 140).

The next three chapters are essentially the same as previously published essays. Chapter 7, 'Spellings in *A Lover's Complaint* as Evidence of Authorship' (pp. 141–68), reprints Jackson's 2008 essay 'The Authorship of *A Lover's Complaint*: A New Approach to the Problem', published in the *Papers of the Bibliographical Society of America* and reviewed with strong approval in *YWES* 89[2010]. Chapter 8, 'Neologisms and "Non-Shakespearean" Words in *A Lover's Complaint*' (pp. 169–83), is substantially the same as Jackson's 2008 essay of the same title for *Archiv für das Studium der neueren Sprachen und Literaturen*, reviewed with strong approval in *YWES* 89[2010]. And chapter 9, '*A Lover's Complaint*, *Cymbeline*, and the Shakespeare Canon: Interpreting Shared Vocabulary' (pp. 184–206), is substantially the same as Jackson's 2008 essay of the same title for *Modern Language Review*, reviewed with strong approval in *YWES* 89[2010].

Concluding the second half of the book is the newly written chapter 10, '*A Lover's Complaint*: Counter-Arguments and Conclusions' (pp. 207–18). Marina Tarlinskaja has argued that the verse style of *A Lover's Complaint* is much unlike Shakespeare's verse style, but as Jackson points out, 'we have no way of knowing what metrical characteristics we should expect to find in rhyme-royal stanzas of a narrative poem by Shakespeare that was written in the first decade of the seventeenth century' (p. 207), because aside from this one (if he wrote it) he wrote no others. The only Shakespearian verse writing that uses the same stanzaic form as *A Lover's Complaint* is *The Rape of Lucrece* written more than a decade earlier—and Shakespeare's verse habits of the kind measured by Tarlinskaja demonstrably changed over time—so that we just do not have the right kind of samples to compare with. Next Jackson shows that the conclusion of Ward E.Y. Elliot and Robert J. Valenza that *A Lover's Complaint* is not by Shakespeare was based on flawed tests and misinterpreted results, as he illustrates in a separate article described elsewhere in this review. Lastly Jackson deals with the flaws in Vickers's arguments based on rhyme, that Jackson perceives as vitiated by multiple false assertions and misconceptions about chance. For example, Vickers finds it highly significant that *A Lover's Complaint* and John Davies of Hereford's *Humour's Heaven on Earth* share the triple rhyme *wind/find/mind*, but as Jackson shows this was a common triple rhyme, occurring twenty times in poems from 1593 to 1617 (p. 214).

The book ends with appendices (pp. 219–51) that provide all the data upon which the arguments depend, including extensive lists of phrase-matches from Literature Online.

One book-form collection of essays contains material that is relevant to this review: *Women Making Shakespeare: Text, Reception, Performance*, edited by Gordon McMullan, Lena Cowen Orlin, and Virginia Mason Vaughan as a Festschrift for Ann Thompson. The collection contains many fine essays, but only the few that are relevant to the topic of Shakespeare's texts are noticed here. All the contributors were required to keep to under 3,000 words (including all apparatuses) so the essays do not have the space to go into much detail. In 'Remaking the Texts: Women Editors of Shakespeare, Past and Present' (pp. 57–67), Valerie Wayne notes that the history of women editing Shakespeare starts with Henrietta Bowdler. I would have thought this a rather ignominious beginning since she censored him, but Wayne seems reluctant to condemn her for that. Wayne offers no new evidence in her whistle-stop tour of women editing Shakespeare, just a survey of what is already known, ending in virtually a list of who is active today in editing Shakespeare, and then an actual list of the gender balances of various series and teamwork editions. Surprisingly, Wayne omits Sonia Massai, an editor, textual critic, and historian of the book, even though she contributes to this collection.

In '"To be acknowledged, madam, is o'erpaid": Woman's Role in the Production of Scholarly Editions of Shakespeare' (pp. 69–77), Neil Taylor ponders why women do not edit Shakespeare as much as they teach and write about him. He does not mention the plausible but unfashionable possibility that on average male brains and female brains are attracted to somewhat different activities, so that while there is a considerable overlap—a lot of women do like the work of editing, and like it somewhat more than most men do—there is nonetheless an average difference in the size of the two populations of suitably interested persons. Indeed, given what we know about the evolved differences between male and female brains, the hypothesis of no average difference regarding a task that calls for quite specific cognitive abilities would on the face of it be more implausible than one that posited some difference. The prospect that this possibility raises is that even when all the biases and obstacles are removed there may still not be a 50/50 gender split amongst editors. Perhaps more people believe this than are prepared to say it out loud, for fear of being misunderstood as blaming women for their relative absence from the discipline when of course for most of its history the reason for their absence has been blatantly sexist bias and obstacle-raising.

H.R. Woudhuysen's 'Some Women Editors of Shakespeare: A Preliminary Sketch' (pp. 79–88) is about the biographies of various women editors, not about their work, and 'Bernice Kliman's *Enfolded Hamlet*' (pp. 89–98) by John Lavagnino has some interesting reflections on how user interfaces for digital editions have changed over the past twenty years, but offers nothing substantial on the texts of Shakespeare. In 'Women Making Shakespeare—and Middleton and Jonson' (pp. 99–108), Suzanne Gossett poses the question that the previous essays have avoided regarding inherent (or is it learnt?) gender bias: 'are women editors attracted to the comedies' and so choose to edit them, because of their 'very content' rather than because men are keeping the tragedies and histories to themselves? Gossett does not have an answer (p. 101), but is convinced that men and women have different tastes: 'Conventionally women are assumed to be more interested in fabric and clothes than men are; I have found it so' (p. 102).

Gossett ends with a couple of emendations that she thinks of interest to the feminist editing of Shakespeare. The first is Diana's remark to Bertram: 'I see that men make rope's in such a scarre | That we'll forsake ourselves' (*All's Well That Ends Well* 4.1i.39–40). Gossett finds Gary Taylor's emendation to 'I see that men make toys e'en such a surance . . .' to be incomprehensible, although she then quotes Taylor's careful unpacking of each term and its polysemy. Gossett prefers P.A. Daniels's 'I see that men may rope's [= rope us] in such a snare' and she gives some defence of it against Taylor's objection that it does not lead (as his emendation does) to Diana's sudden demand of a ring from Bertram, arguing that Diana considers herself one of the women (= 'us') who has been so ensnared, so she negotiates for terms.

Next Gossett turns to the problem of some apparently faulty speech prefixes in *Merry Wives of Windsor* 4.1, which Helen Ostovich fixes in the new Norton Shakespeare third edition of the play using a 'specifically feminist justification' (p. 107). Gossett seems to think this goes too far, since 'Even a feminist editor must respect the actual words of a text' (p. 108). Here Gossett comes perilously close to suggesting that the premise of all these essays on feminist editing may be faulty and that so long as editors are not being sexist—and whether that ideal has yet been achieved is an open question—editing has no need of feminist theory. The remainder of the contributions to this collection are about the reception of Shakespeare and hence are of no concern to this review, although they are highly interesting.

This year the theme of the book-form periodical *Shakespeare Survey* was 'Shakespeare's Collaborative Work'. In 'Why Did Shakespeare Collaborate?' (*ShS* 67[2014] 1–17), Gary Taylor observes that we now know that more than one-third of the plays by Shakespeare were collaborative. Shakespeare did not write the beginnings of the plays he collaborated on: he came in at the complicating phase because he was better at characterization (and especially characters experiencing some emerging conflict) than at plot or exposition. In the early 1600s Shakespeare could not alone satisfy the demand for plays about and set in London and he was in general better at comedy (on which he never collaborated) than at history and tragedy (on which he did). Collaboration certainly can produce inconsistency in plays, but it is not at all clear that early audiences and readers minded this: they seem to have valued variety at least as much as unity. And of course, Shakespeare's non-collaborative plays are full of inconsistencies too. Shakespeare collaborated because, in some genres, it made for better plays than he could manage on his own

The second essay is by the present reviewer and is titled 'What Is Not Collaborative about Early Modern Drama in Performance and Print?' (*ShS* 67[2014] 18–28). It argues that recent commentators, especially Tiffany Stern, have overstated the routine alteration and revision of play scripts—the Master of the Revels's licensing fee gave the players a strong disincentive—and have likewise overstated how far printing was an inherently collaborative process. In fact, Egan argues, what got licensed represented pretty well what got performed and what got printed represented pretty well what the printer was given to print. Much in the same vein, Will Sharpe's 'Framing Shakespeare's Collaborative Authorship' (*ShS* 67[2014] 29–43) diagnoses general overstatement of the collaborative nature of dramatic creativity and reasserts the importance of authorship, lone and collaborative. Sharpe sees Shakespeare collaborating to a lesser extent than Taylor does, counting not total plays but lines—initially excluding cases that Taylor considers proven—and finding that more than 90 per cent of Shakespeare's writing went into his sole-authored plays and less than 10 per cent into his collaborative ones. By this method of tallying, Shakespeare could have contributed small parts to many more plays and still put much more (in terms of word counts) into his sole-authored plays than his collaborations. Clearly, we need to be careful how we express ourselves regarding the amount of collaborative writing that Shakespeare undertook.

In 'Collaboration and Proprietary Authorship: Shakespeare *et al.*' (*ShS* 67[2014] 44–59), Trevor Cook takes the opposite line from Egan to argue the poststructuralist position that 'Shakespeare was probably accustomed to definitions of authorship, textual property and the individual very different from our own' because the 'radically collaborative nature of staging a play requires each participant to relinquish his (or her) individual interests' (p. 35). Cook supports Jeffrey Masten's claim that co-authorship was 'a dispersal of authority, rather than a simple doubling of it' (p. 46) and traces the various attempts by authors to assert ownership of, or at least get credit for, their bits of various collaborative works. Cook acknowledges that 'writers at the turn of the seventeenth century could and sometimes did observe proprietary authorship in the context of collaborative working arrangements' (p. 58), but he thinks that inevitably the practice of co-authorship blurs the boundaries of the individual writing stints. Cook repeatedly cites Masten and mocks the folly of scholars who 'are motivated to identify who wrote what in a collaboration so effective that it is difficult, if not impossible, to tell' (p. 59). The next essay, Barry Langston's 'Topical Shakespeare' (*ShS* 67[2014] 60–8), contains readings of topicality in *1 Henry VI* but nothing relevant to this review.

Amongst the highlights of the collection is William W. Weber's essay, 'Shakespeare After All? The Authorship of *Titus Andronicus* 4.1 Reconsidered' (*ShS* 67[2014] 69–84). Ever since scholars have accepted the case for co-authorship of *Titus Andronicus*, Peele has been given scenes 1.1, 2.1, 2.2. and 4.1, and Weber shows that the last of these has not been subject to stringent enough testing. Weber applies MacDonald P. Jackson's technique of looking for near-unique phrase matches in LION, which as remarked above is rapidly becoming the most widely used and trusted method of authorship attribution.

At first sight, though, 4.1 has rather too few feminine endings to be typical Shakespeare, with just three in its 128 blank-verse lines, but the right number to be Peele's. But Shakespeare's habitual deviation from his normal rate of feminine endings is easily broad enough to accommodate one scene having so few, and counting by acts is more reliable a way of using feminine-ending rates to attribute authorship.

Weber shows that *Titus Andronicus* 4.1 has tended to be lumped in with the rest of the Peele contribution to the play even in studies that could have tested it independently, and since it does not disrupt those studies' general conclusions of Peele's hand in the play scene 4.1 has remained in the putative Peele stratum. Only the feminine ending test puts it there. Another test that might suggest that 4.1 is Peele's rate of use of vocatives, but again, like the rate of feminine endings, this metric can swing wildly within anybody's scenes, depending on dramatic content. In particular, 4.1 uses a child actor and it might well be astute of a dramatist to use a lot of vocatives in such a scene so that the child has least trouble remembering who is who. Weber uses the handy checklist of all Shakespeare's child characters given in Kate Chedgzoy, Suzanne Greenhalgh, and Robert Shaughnessy's collection *Shakespeare and Childhood* [2007] to see if Shakespeare used vocatives more often in scenes involving children, and indeed he does: twice as often as in those scenes without children.

Then comes Weber's application of the Jackson-inspired tests of 4.1. Every phrase and collocation of the scene—he does not say how distant, for collocations—was entered into LION and looked for in Peele's and Shakespeare's canons; this makes for a two-horse race, which in this case is desirable since no other plausible candidate exists. The phrases and collocation unique to one canon were recorded as one hit for each unique phrase with the number of occurrences within each canon not recorded. For this test, the Shakespeare canon was restricted to *The Comedy of Errors*, *Love's Labour's Lost*, *Richard II*, *Richard III*, *Romeo and Juliet*, *The Taming of the Shrew*, *The Two Gentlemen of Verona*, and *Venus and Adonis* to make it as much like Peele's canon in size and genre-balance as possible; this test is demonstrably valid for even quite short samples, as 4.1 is. The result is that 25 per cent of scene 4.1's unique matches to either Peele or restricted-canon Shakespeare are to Peele and 75 per cent are to Shakespeare. Quite a few of the matches to Shakespeare are *epizeuxis*, which is supposed to be a Peele trait. Moreover, looking at individual words there is in 4 just one, *playeth*, that appears in Peele's canon but not in Shakespeare's canon, and more than a dozen that appear in Shakespeare's canon and not in Peele's. Turning to subjective criteria, Weber shows that in 4.1 we see Shakespearian sophistication in its use of literary and mythical allusions, something Peele was not at all sophisticated about. The conclusion is the Shakespeare, not Peele, wrote *Titus Andronicus* 4.1.

On the same play, Dennis McCarthy and June Schlueter argue, in 'A Shakespeare/North Collaboration: *Titus Andronicus* and *Titus and Vespasian*' (*ShS* 67[2014] 85–101), that the former is an adaptation of the latter, now lost, which the authors here attribute to Thomas North. The authors search within the database of the Early English Books Online Text Creation Partnership

(EEBO-TCP) but they mistakenly think that they are searching within the whole of EEBO, so that they unwisely comment of their findings that 'In a database of 128,000 texts, this cannot be coincidence' (p. 92). Depending on which version of the product one has, EEBO-TCP contains no more than about 53,000 texts. More importantly, McCarthy and Schlueter commit what Jackson has identified as the one-horse error in that they use the text-comparison software called Wcopyfind to determine phrases common to *Titus Andronicus* and North's *The Dial of Princes* and only then go looking for these phrases in EEBO-TCP. As Jackson has pointed out, any two substantial texts will have phrases in common that are unique to those two so such a shared link proves nothing.

In 'The Two Authors of *Edward III*' (*ShS* 67[2014] 102–18), Brian Vickers starts with a brief history of authorship-attribution studies about this play—with an in-passing disparagement of the counting of function word frequencies—and confirms the unavoidable conclusion that Shakespeare wrote scenes I.ii, II.i, and II.ii. Vickers contends that Thomas Kyd wrote the remaining sections of *Edward III*. The argument begins with the dramatic convention of 'the narration of an off-stage event, usually a catastrophe, conveyed by a Nuntius' (p. 105) that came from Senecan tragedy. *Edward III* and *The Spanish Tragedy* have this feature and, more unusually, both do it for both sides of a conflict (pp. 108–9).

To explore further the connection, Vickers uses software to find the trigrams—that is, three words in succession—that are common between the non-Shakespearian parts of *Edward III* and *The Spanish Tragedy* and Kyd's translation *Cornelia*, and then eliminates the ones that are found elsewhere in the drama generally, defined as 'plays written for the public theatres before 1596' (p. 111). This way of working is the classic one-horse-race error identified by Jackson, and it is remarkable that Jackson's proof that this method is fatally flawed—first given in a 2008 article in *Research Opportunities in Medieval and Renaissance Drama* (reviewed in *YWES* 90[2011])—has not deterred Vickers and others (such as McCarthy and Schlueter, above) from using it.

In his appendix of what he contends are unique matches between *Edward III* and *The Spanish Tragedy*, Vickers lists 'joynd in one' (p. 116) as such a case, but he overlooks the 1597 quarto of *Romeo* and *Juliet* (first performed 1594-96) which has 'ioynd ye both in one' (sig. E4v). Likewise Vickers claims that certain single words are found only in *Cornelia* and *Edward III* and nowhere else in pre-1596 drama (pp. 117-8), But for some of his examples this is disputable. The word ''engendered'' seems not to be rare: it is found in Christopher Marlowe's *Massacre at Paris* (first performed in 1593) and in Thomas Nashe's *Summer's Last Will* and *Testament* (first performed 1592) and *The Merchant of Venice* (first performed in 1596-8) and in other less-well known plays. Or take *coronet*, which again Vickers claims can be found only in *Cornelia* and *Edward III* and no other pre-1596 play. But in fact it appears in John Lyly's *Midas* (first performed in 1589) and in *A Midsummer Night's Dream* (first performed in 1595-96) and in Robert Greene's *Friar Bacon* and *Friar Bungay* (first performed in 1586-90).

Vickers's essay is followed by Francis X. Connor's 'Shakespeare, Poetic Collaboration and *The Passionate Pilgrim*' (*ShS* 67[2014] 119–29). *The Passionate Pilgrim* was published in 1599 by William Jaggard and purported to be by 'W. Shakespeare', but it has only five of his poems in it—three from *Love's Labour's Lost* and two from *Sonnets*—and the rest of the poems are by other people. Connor treats *The Passionate Pilgrim* as a kind of collaboration—in the 'socialized production' sense—although we do not know if Shakespeare had anything to do with it, and Heywood's account of Shakespeare's response to the 1612 edition that put Heywood's work under Shakespeare's name tells us that he was not involved in that edition. Connor reckons that *The Passionate Pilgrim* has its own artistic coherence and he explores first its tangential links to Shakespeare and then its publishing history—what else the Jaggard publishing house was doing and the new market for Shakespeare's books—and how it figures in sammelbands and books of excerpts. Connor wonders why some poems in *The Passionate Pilgrim* are introduced with pilcrows or Aldine leaves, which may be marks showing that the slips of paper holding the poems in *Love's Labour's Lost* were separate from the script of the play and that these marks were the linking devices between the loose sheets and the script.

James P. Bednarz's 'Contextualizing "The Phoenix and Turtle": Shakespeare, Edward Blount and the *Poetical Essays* Group of *Love's Martyr*' (*ShS* 67[2014] 130–48) treats *The Phoenix and the Turtle* as Shakespeare's intentional collaboration in the 1601 book project *Love's Martyr*. This book contains Robert Chester's epic poem *Love's Martyr* followed by twelve 'Poetical Essays' by 'Ignoto', John Marston, George Chapman, Ben Jonson, and *The Phoenix and the Turtle* by Shakespeare. The essay is largely concerned with the ways that the book trade could produce such an innovative collaborative volume and the claim that this is a collaborative work is based on a rereading of the poetical relationship between Shakespeare's poem and Chester's poem to which it responds; so the essay is not of direct relevance to this review.

In 'Shakespeare's Singularity and *Sir Thomas More*' (*ShS* 67[2014] 149–63), James Purkiss reckons that the consensus from W.W. Greg to Gary Taylor is that Shakespeare was not closely involved in the collaborative writing of this play, and Purkiss sets out to show that in fact he was. Purkiss explores what has been discovered about the shares and actions of the various hands in *Sir Thomas More*, emphasizing just how much of this knowledge is speculative. He asserts without justification that a lot of relatively unreliable tests all pointing towards the same conclusion do not themselves add up to a reliable pointer towards that conclusion. In fact they do, and an entire branch of mathematics, much used in medical diagnoses and risk management depends on this principle. Purkiss quotes Michael Hays claiming that 'non palaeographic arguments may reach the same conclusion as palaeographic ones, but they cannot strengthen palaeographic arguments themselves' (p. 153). Is this, indeed, the case?. If the non-palaeographic arguments point strongly to the conclusion that writer *X* thought up the words in document *Y* and if document *Y* has some marked (but non-conclusive) handwriting similarities to document *Z* that is definitely in the hand of writer *X* (say, his will), then this non-palaeographic evidence really does strengthen the palaeographic case since the alternative hypotheses become less likely. That is, the field of candidates for whom the palaeographic facts must fit the evidence is thus, by the non-palaeographic evidence, narrowed to those who not only had similar handwriting but were also in a position to copy out the author's words.

Purkiss explores the non-essential point that scholars have disagreed about just how involved Shakespeare was in the writing of *Sir Thomas More*, making a lot of the relatively small differences of opinion about this. Those who think that Shakespeare was disconnected from the writing of the rest of the play complain that the rebels get more cartoonish as they get more rebellious—at the start of the play they are quite dignified and justifiably indignant—and Purkiss explains that this is just what happens to individuated characters when the needs of the drama require it. Hand D seems to pick up from earlier in the play the notion of simplicity in the rebels' action, and that is what Purkiss reckons shows Hand D's close connection with the rest of the play. That is, the representation of the rebels was already turning clownish before Hand D got started and Hand D made it more so. Purkiss revives Gerald Downs's claim (reviewed in *YWES* 88[2009]) that Hand D contains eyeskip errors and so it must be a transcript rather than original composition, in which case, says Purkiss, it might contain a mix of Shakespeare's and others' writing. Indeed, it might, but no one has brought forth anything significant to show that it is and this seems like an attempt by those who would deny Shakespeare's authorship of the crowd-quelling scene to suggest that it might not be wholly his. Purkiss ends by finding a couple of phrases in Hand D that can be found in others' writing, but this kind of non-systematic parallel hunting tells us nothing, as he must know since he reports Jackson's voluminous writing on the strict protocols that need to be followed if such parallels are not to mislead us.

Brean Hammond's contribution to the collection is called '*Double Falsehood*: The Forgery Hypothesis, the "Charles Dickson" Enigma and a "Stern" Rejoinder' (*ShS* 67[2014] 164–78). Like Gary Taylor in 'Sleight of Mind: Cognitive Illusions and Shakespearian Desire' (reviewed in *YWES* 94[2015]), Hammond seeks to show that Tiffany Stern's essay ' "The Forgery of Some Modern Author"? Theobald's Shakespeare and Cardenio's *Double Falsehood*' (reviewed in *YWES* 93[2014]) is quite wrong to suggest that in *Double Falsehood* Theobald passed off his own forgery as Shakespeare's play. Hammond also responds to Stern's other essay on this topic, called ' "Whether one did contrive, the other write, | Or one fram'd the plot, the other did indite": Fletcher and Theobald as Collaborative Writers' (reviewed in *YWES* 93[2014]). Hammond finds a series of factual errors in Stern's account of Theobald's literary activities: she just does not seem to understand that he was not speaking for himself in his regular publication *The Censor* and in general she tries to assassinate his character by implication, for example by observing that he was known for his pantomimes without indicating what a serious genre this was.

Hammond shows that the hypothesized transmission history for *Cardenio* proposed in his Arden3 edition is paralleled in the certain transmission history of the Philip Massinger and Nathan Field play *The Fatal Dowry* that survived in manuscript in the hands of Restoration theatre practitioners and thence reached mid-eighteenth-century performance. There's nothing miraculous or suspicious about this kind of transmission. Thus an eighteenth-century reference to another such manuscript by Francis Beaumont, John Fletcher, and Shakespeare turning up is treated seriously by Hammond and wrongly dismissed as vague by Stern. Hammond notes that Stern ignores the recently discovered allusions to *Cardenio* in pre-Commonwealth performance, and has nothing to say in response to the recent stylometric work that points to Shakespeare's hand in *Double Falsehood*.

The next eleven essays in this volume of *Shakespeare Survey*, fascinating as they are, are unconnected to the topic of this review. Then comes B.J. Sokol's 'John Berryman's Emendation of *King Lear* 4.1.10 and Shakespeare's Scientific Knowledge' (*ShS* 67[2014] 335–44). Some exemplars of Q1 *King Lear* have at 4.1.9–10 the line 'Who's here, my father poorlie, leed' (Q1u) and others '. . . my father parti, eyd' (Q1c) while Q2 and F have '. . . My Father poorely led?' There is no obvious dramatic reason connected to the wider Q/F differences that would explain Q1 and F differing on this reading. There is an attraction to the *poorly led* reading in that Gloucester enters with an old man (who in Q1's stage direction is explicitly leading him) and hence Edgar notices this detail at first before noticing the reason for it. Sokol thinks that the poet John Berryman's emendation to 'My father pearly-ey'd' is correct. Sokol traces the early modern association of pearls with cataracts, referencing his own previous work on Alonso's pearl-eyed blindness in *The Tempest*. For Berryman's reading to be correct, we have to say that Q1u is nearly correct in *poorlie* except that *oo* should be *ea* and that Q1c is entirely correct in *eyd*. How could this happen? Sokol cites personal correspondence from the present reviewer on a similar mix of good and bad readings occurring in a press variant before and after stop-press correction. In such cases the first setting may get some of the letters right while being, at the level of the word, incorrect and unintelligible. Proof correctors care more for overall intelligibility than the percentage of letters correctly set and may alter an entire reading to achieve it, thereby lowering the percentage of letters that are correct. The remainder of this volume of *Shakespeare Survey* is not relevant to this review.

And so to this year's articles. The most significant for our purposes are two by Gary Taylor on the subject of Middleton's adaptation of Shakespeare's *Macbeth*. The first contains a fresh exploitation of Jackson's attribution method described above: Gary Taylor, 'Empirical Middleton: *Macbeth*, Adaptation, and Microauthorship' (*SQ* 65[2014] 239–72). The present reviewer must disclose that he read a pre-publication version of this essay and is acknowledged amongst others for making comments that the author found helpful in revision of it. Once Middleton's *The Witch* was printed in 1778 it became clear that it had influenced *Macbeth* in at least scenes 3.5 and 4.1, but with recent computational approaches both supporting and, in the work of Brian Vickers, denying Middleton's adaptation of *Macbeth*, non-specialists must be tempted to shrug their shoulders and conclude that the matter is undecidable. Naturally, adaptation is harder to spot than collaborative writing because usually an adapter contributes fewer words to the final result than a co-author would. We have ample reason to suspect that 'What? is this so? . . . *Musicke*. | *The Witches Dance, and vanish*' in *Macbeth* (4.1.140–8) is a Middleton interpolation, but it is only sixty-three words in all. This sample seems too small for most methods to test unless we also use bigrams (two words in succession), trigrams (three words in succession), and larger *n*-grams, and also bring in collocations, variant forms, and variant spellings; together these increase the amount of data we have many-fold.

Taylor's method is to go searching for these strings in electronic databases of Shakespeare and Middleton that Oxford University Press now sells as Oxford Scholarly Editions Online. First, a validation stage: does the proposed test find Shakespeare to be the author of a work known to be by Shakespeare? Taylor takes a passage of sixty-three words from *King Lear* that, like the passage from *Macbeth*, is in rhymed tetrameters: 1.3.57–67. The test is whether this passage contains more *n*-grams and collocations from the Shakespeare canon than the Middleton canon, after we discard those that appear in both canons. (It would be interesting to hear of the result for those *n*-grams and collocations from the passage being tested that are found in Shakespeare's and in Middleton's canons: are they found more often in Shakespeare's?) Taylor finds thirteen such parallels with the Shakespeare canon and only two with the Middleton canon counting type-wise, so that if one *n*-gram or collocation matches to two different bits of the Shakespeare canon then it counts only once, and 17:2 counting token-wise, so that one bit of the *King Lear* passage matching two bits of the Shakespeare canon counts twice. Counting either way—13:2 or 17:2 in Shakespeare's favour and against Middleton—the method seems to have correctly identified Shakespeare as much more likely than Middleton to have written the passage from *King Lear*.

Taylor repeats the test with sixty-three words of rhymed tetrameters from undisputed Middleton writing: *A Mad World, My Masters* 4.1.43–51, and again looking only for *n*-grams and collocations that find a match in either the Shakespeare or the Middleton canon but not both. Surprisingly, this comes out at 12:8 in favour of Shakespeare if we count token-wise. Explaining this, Taylor's remarks seem to imply that he has been searching LION as well as OSEO although in his description of the method on page 246 he had mentioned only OSEO; as he rightly observes, Shakespeare is better represented in LION than Middleton is. Also offered as explanations for the failed attribution of *A Mad World, My Masters* are that Shakespeare influenced Middleton (and not vice versa) and that Shakespeare has the bigger canon (about twice the size) and so he has more, as it were, 'opportunity' to match any given *n*-gram or collocation. Other ways to accommodate this surprising failure are to say that this test tells us not to expect much Middleton-like writing in work truly by Shakespeare but to expect Shakespeare-like writing in work truly by Middleton, and that it tells us to count type-wise. (Personally, I would not expect one failed attribution attempt to tell us something so fundamental about the method, variations upon which should emerge only after a lot of randomized tests.)

Counting type-wise instead of token-wise, the present failure to detect Middleton's hand is turned into a marginal success: 6:7 in favour of Middleton. Taylor reckons that the results of this test show that 'collocations are more significant than consecutive word strings' (p. 252) because in this test none of the collocations find matches in Shakespeare and two find matches in

Middleton. (Again, I would counsel that it is too soon to draw any such conclusions about the method from just two validations of it.) Taylor observes that in this test one play, *Hengist, King of Kent*, provides several of the Middleton matches, so we could test 'concentrations in a single work' (p. 253), and that we could also constrain the test by date, so looking for Elizabethan versus Jacobean plays. He notes that if he had applied these constrains to his first validation test on *King Lear*—counting by types, looking only at plays written in the same monarchical reign, looking for concentrations in a single work, and only at collocations—then it would still have pointed to Shakespeare as the author in that case.

Taylor decides to introduce another criterion: overall rarity of the *n*-gram or collocation, as judged by its appearances outside the Shakespeare or Middleton canons in LION. This refinement of dropping those *n*-grams and collocations that also appear in other writers' canons—that is, other than those of Shakespeare and Middleton—makes the test work the way Taylor wants it to: the *King Lear* passage is conclusively Shakespearian, the *Mad World* passage is conclusively Middletonian. Using this newly refined test, Taylor tests the passage from *Macbeth* we started with. He finds: more Middleton than Shakespeare types (8:9) but not tokens (13:11), that Middleton has more Jacobean types (3:9) and tokens (5:11), that Middleton has more unique parallels (1:4 on types, 1:6 on tokens), that Middleton has more unique Jacobean parallels (0:4 on types, 0:6 on tokens), and more concentrations in a single work: 2-types-1-unique for Shakespeare's *Twelfth Night* versus 2-types-2-unique for *The Witch*.

Taylor uses the statistical procedure called Fisher's Exact Test to try to see how likely it is that chance alone would produce the results he has found for the Shakespeare and Middleton parallels to the passage from *Macbeth*. This part of the essay I find least convincing, since his null hypothesis is 'that the Folio *Macbeth* passage was written by Shakespeare' and I am not clear how he thinks Fisher's Exact Test could be used to test that hypothesis. Something is clearly wrong with how Taylor uses Fisher's Exact Test in that he comes to the conclusion that it shows that 'there is a 100 percent probability that the *Mad World* sample and the *Macbeth* sample have the same author' and yet he also asserts that 'This 100 percent probability does not mean there is absolute certainty that they were written by the same author' (p. 256). In fact, as a matter of language, the first claim does entail the second—they are the same claim—and importantly Fisher's Exact Test is not mathematically capable of telling us anything with 100 per cent probability so this application of it must be faulty.

What if someone other than Shakespeare or Middleton wrote the passage? Taylor repeats his searches of *n*-grams and collocations from the *Macbeth* passage in all the Jacobean drama in LION, and finds that the matches come preponderantly from Middleton works: 2:5 by types, 2:9 by tokens. (In fact these are hits he got before, so this is really applying the 'must be Jacobean' constraint and loosening the authorship constraint to be 'by anyone'.) But is it not unfair to look at only Jacobean drama, since Shakespeare had done most of his work by 1603? To meet this hypothetical objection, Taylor relaxes the date constraint to '1576–1642' (for first performance) and finds that

Middleton still predominates. The unavoidable conclusion is that the passage is by Middleton.

After glancing at his own paper on Middleton authoring the five lines between '"*Enter Hecate and the other three witches*" and "*Music and a song*" (4.1.38.1–43.1)' (reviewed below), Taylor turns to *Macbeth* 3.5, where Hecate first appears, which is often claimed to be entirely Middleton's work. It comprises 259 words, almost entirely rhymed tetrameters. For his Shakespeare parallel passage Taylor chooses *Pericles* scene 10 (= 3.0), one of Gower's choruses, from which he picks 259 words. By the same tests as above, Shakespeare predominates in matches to the *Pericles* passage no matter which way you slice it, and Middleton dominates matches to *Macbeth* 3.5. Again, to these entirely convincing results Taylor applies Fisher's Exact Test in ways that are not clearly valid statistically.

To the second edition of Robert S. Miola's Norton Critical Edition of *Macbeth*, Taylor contributes a new essay on the play's authorship in which he takes issue with Brian Vickers's objections to Taylor's claim that Middleton adapted Macbeth ('*Macbeth* and Middleton', in Miola, Robert S., ed. *Macbeth*, Second edition, pp. 296-305). Taylor responds primarily to Vickers's 2010 *Times Literary Supplement* essay called 'Disintegrated: Did Thomas Middleton Really Adapt *Macbeth*?' and the associated files made public on the London Forum for Authorship Studies website (reviewed in *YWES* 91[2012]). Grace Ioppolo wrongly claimed that because the songs in *Macbeth* are merely cued with a few opening words followed by '&c' they were probably not added by an author, since an author would write out the whole song. In fact, as Tiffany Stern showed, such a pointer to the full text of a song held on another piece of paper would be perfectly normal, and authors used them.

In order to argue that Shakespeare might have added the two songs from *The Witch* to *Macbeth*, Vickers had to use an old dating of *The Witch* that assumes that it was written in 1609–16, but in fact the modern dating of the play is late 1615 or 1616. For Vickers to be right, Shakespeare would have had to adapt *Macbeth* in the very last months of his life, which is odd. Taylor reports that Vickers's account of R.V. Holdsworth's work on stage directions that use the present participle *meeting* simply misrepresents Holdsworth's work, and that Holdsworth himself has now declared that it does. Taylor objects (as did this reviewer at the time) that Vickers's use of the evidence of the entrance direction 'Enter Bast[ard] and Curan meeting' from *King Lear* is a red herring because it clearly calls for both men to enter. What is at stake in this discussion is the ambiguity generated by entrances of the form 'Enter A meeting B', not specifying whether B is already on stage, and this is a kind of ambiguity that is common in Middleton and not found in Shakespeare. This ambiguity is found twice in the bits of *Macbeth* that Taylor attributes to Middleton. Shakespeare never used the word *seam* but Middleton used it many times (and *Macbeth* uses it once) to create images of bodies being ripped apart, especially from neck to navel. Jonathan Hope's work on the rates of regulated *do* is not conclusive, but it too points in the direction of Middleton's authorship of *Macbeth* 3.5 and 4.1.

Vickers tries to show that Hecate's rhymed lines are like those of other supernatural characters in Shakespeare, but, as Taylor points out, none of those Shakespearian characters speak in rhymed iambic tetrameters as Hecate does and as lots of Middleton characters, especially supernatural ones, do. Marina Tarlinskaja's analysis of the prosody that Vickers draws upon has now been withdrawn by her because she realizes that she was not grasping exactly which lines Taylor was claiming as Middleton's; once she knew that she decided that there was too little evidence for her approach to work upon. Regarding Vickers's argument based on failing to find certain trigrams from *Macbeth* in the Middleton canon, Taylor reports this reviewer's demonstration (in *YWES* 91[2012]) that they are there and that Vickers simply missed them. Once we search in 'comprehensive, public databases' such as LION, we can find in Middleton many and in Shakespeare few parallels for another excerpt from *Macbeth*, the seven lines from '*Enter Hecat, and the other three Witches*' to '*Musicke and a Song*. *Blacke Spirits, &c*' (4.1.39–43) that Taylor claims are Middleton's. Taylor here lists them all.

Three articles by Hugh Craig in collaboration with others address the methods by which authorship attribution is currently being carried out. The first, 'An Information Theoretic Clustering Approach for Unveiling Authorship Affinities in Shakespearean Era Plays and Poems' (*PLoS ONE* 9:x[2014] n.p.), shows that, contrary to the assertions of poststructuralism and postmodernism, authorship trumps all other considerations (such as genre and topic) when weighing the likenesses of plays from Shakespeare's time by means of their rates of usage of all words. The authors took 256 plays from Shakespeare's time and used the Intelligent Archive software (described in *YWES* 91[2012]) to regularize their variant spellings and disambiguate (from context and frequency) strings that point to different words, such as the multiple verbs and nouns all represented by the three-character string *r-o-w*. For the resulting 66,907 unique words in these 256 plays they then counted how many times each word appears in each play, producing a data matrix of 66,907 × 256 cells. What followed was the application of an algorithm to see if the rates of usage of these words varied in a way that can be called 'clustering': that a particular set of plays are all alike in their rates of usage (high or low) of particular sets of words. Then they looked to see if the clusters that the algorithm comes up with—and that it was not, as it were, 'informed' of before—align with some known criterion such as author, or genre or date or topic.

The algorithm used was 'Minimum Spanning Tree k-Nearest Neighbour' (MST-kNN), and it was applied after using as the 'distance' between two works the Jensen–Shannon Divergence (JSD) between the frequencies of the words in these two works. Full appreciation of the mathematical formulas in which MST-kNN and JSD are explained is beyond the limit of this reviewer's comprehension. The resulting clusters were clearly dominated by authorship (not genre, not topic) as the most powerful determinant of 'closeness'. As an authorship attribution test this is quite powerful: the authorship of the near neighbours of a work in a cluster is a reliable guide to the authorship of that particular work. The authors talk the reader through the various branches and rings of works in their large cluster-chart, acknowledging the few cases where similarity of genre and topic seem to have shaped the connections. The big conclusion, though, confirms other recent work in this field: authorship is not a post-Romantic principle of categorization and is not subordinate to genre and topic, but really is an objective, detectable facet of the surviving works of this period. Impressively, the authors include their entire raw datasets for others to work on.

In the second of Craig's articles, 'Language Chunking, Data Sparseness, and the Value of a Long Marker List: Explorations with Word N-grams and Authorial Attribution' (*L&LC* 29[2014] 147–63), it is shown that Brian Vickers is wrong to believe that trigrams are inherently better markers of authorship than single words are. The intuition on which this fallacy is based is that *n*-grams where $n > 1$ must be better for authorship attribution than those where $n = 1$ (individual words) because they reflect how the mind uses language. The problem with long strings of words is that there are many different unique instances of them even in quite long texts, with each unique instance being as rare as rare can be. As well as strict *n*-grams (certain words in a certain order), this study uses 'skip *n*-grams' in which 'we find the first instance of one of the listed words, then move to the next of them, ignoring any intervening unlisted words. The second 2-gram begins with the second of these words and adds the third, and so on'. (It is not clear from this description whether or how the number of 'intervening unlisted words' that are skipped might matter here.)

The first corpus tested is 174 English Renaissance sole-authored, well-attributed professional plays in which the old spelling of function words has been modernized and their elisions expanded. The second corpus is 254 articles from Victorian periodicals. In each case the corpus is divided into segments, and finding (even multiple times) or not finding something is counted as a presence or absence for that whole segment. The authors went looking for *n*-grams common or rare or absent in one authorial set compared to others. The key question is what difference it makes when *n* goes from 1 to 5. The authors applied John Burrows's Zeta test that calls one set of text segments (say, an author's) the *base* and another set (say, of other writers') the *counter* and for each *n*-gram gives a number calculated as follows: (number-of-base-segments-containing-this-*n*-gram / number-of-base-segments) + (number-of-counter-segments-lacking-this-*n*-gram / number-of-counter-segments). Thus the Zeta score has a theoretical maximum of 2 for *n*-grams that occur at least once in every base segment and never occur in any counter segment. By repeating this for *n* going from 1 to 5 they were able to see which length of *n*-gram is most distinctive of authorship.

The authors also performed a version of Burrows's Iota test by counting all *n*-grams that appear twice or more in the base set but never in the counter set. Doing this for one author among the Victorian periodical writers and taking the top 500 scoring *n*-grams and plotting how high their Zeta scores are produces a gently sloping downward trend. The top, most authorially distinctive, *n*-gram scores between 1.3 and 1.5 (out of a theoretical maximum 2) and that is true whether the *n*-gram is single words, 2-grams, or 3-grams, but for 4-grams and 5-grams the high score is only around 1.1. Just as interestingly, for the remaining 499 *n*-grams in the 500 top-scoring *n*-grams the rate at which the scores drop off as we go down the list is different for different values of *n*: 1-grams' scores drop off more slowly than 2-grams' scores, 2-grams' scores drop off more slowly than 3-grams' scores, which drop off more slowly than 4-grams' scores, which drop off more slowly than 5-grams' scores. Thus, on average, the lower that *n* is, the more discriminating of authorship is the *n*-gram, so 1-grams (individual words) are best.

Next the authors tried to replicate what Vickers's method does: to isolate long *n*-grams that occur repeatedly in one author and then see if they can reliably attribute one text by that author to that author after they have taken it out of the set and treated it as if it were of unknown authorship. It turns out that 3-grams provided the largest number of markers appearing in more than one work, but 2-grams provided a greater number of markers if we are looking for markers that appear in more than 2, 3, 4, or 5 works. In general then, for this kind of investigation, 2-grams are better than 3-grams. Turning to 'skip *n*-grams', the authors clarify what this means and it turns out that distance does not count. (Presumably, though, all *n* of the words have to occur within the same text segment for the skip *n*-gram to count.)

Because for their skip *n*-gram test the authors used a pool of predetermined function words, there was no guarantee that the top 500 Zeta-scoring *n*-grams would be more used by the author in question than in the context set of other authors' writing, and in the event for 1-grams only the top 100 were so used: the other 400 got scores less than 1 (out of a maximum of 2). But for 2-grams to 5-grams the graphs stay above 1 as they peter out, and 2-grams turn out to be best. Just which length of *n*-gram works best for distinguishing authorship depends on just where you set the threshold for rarity, so that for the author in question, Anne Mozley, 'The 4-grams set yields the largest number of markers appearing in more than one Mozley article, the 3-grams set yields the largest number appearing in more than two, and the 2-grams set provides the single strongest marker: *over.over* does not appear in the articles by others, but appears in four Mozley articles' (pp. 155–6). Thus, contrary to Vickers's assumption, we cannot just say 'look how many works of author *X* this *n*-gram appears in without appearing in anybody else's work—this must be beyond coincidence', since in fact the significance of that discovery varies with the length of the *n*-grams.

When repeating these experiments for other authors and consolidating the results, the outcome is the same: 1-grams are best overall if one is allowing the texts themselves to choose the words (that is, the ones with the highest Zeta score), but if one is using function-word-skip-*n*-grams then 2-grams are best, and indeed in overall discriminating power the function-word-skip-2-grams are best. With all-word-strict-*n*-grams, 3-grams are best, and with function-word-strict-*n*-grams, 4-grams are best. Again, there is no simple rule of thumb for what length of *n*-gram will be best for authorship attribution. This work was all done with nineteenth-century periodicals, and turning to early modern drama the results are that with all-words-strict-*n*-grams 1-grams are best and with function-word-skip-*n*-grams 2-grams are best, and of these two the former are the best for authorship distinction. The authors' conclusion is that 'no one style of *n*-gram outshines the others in providing authorial markers and that attributionists would be wise to keep an open mind about the usefulness of each' (p. 159). Importantly, function-word-skip-2-grams that do better than 1-grams overall might be getting some of their advantage not because of the particular combination of words but merely because they embody multiple individual function words that are themselves highly discriminatory of authorship. In general, on these results (and contrary to Vickers's assertion) 'rare markers are less useful for attribution than regularly occurring ones' (p. 161).

The third of Craig's three articles, 'Language Individuation and Marker Words: Shakespeare and His Maxwell's Demon' (*PLoS ONE* 8:vi[2013] n.p.), should have been noticed last year but appeared in a publication not normally seen by Shakespearians. The point is essentially the same as that of the article just reviewed—that authorship is detectable in the rates of usage of high-frequency words—but it is pursued here in strict mathematical form. The authors took 168 plays from Shakespeare's time and for each they counted (using the Intelligent Archive) the occurrences of the 55,055 unique words they contain between them. Then the investigators counted using a new metric they have invented, called CM_1, for the rates of usage of these words by John Fletcher, Ben Jonson, Thomas Middleton, and William Shakespeare compared to the other writers. The word choices (for and against each word) are like Maxwell's Demon in the famous gas-physics thought-experiment of the same name, who admits certain highly energetic molecules through his partition by opening it, and shuts it to keep out other, slower molecules.

From the 55,055 unique words found, the ones that are most distinctive of the authorship of Fletcher, Jonson, Middleton, and Shakespeare (that is, four sets of most-distinctive-words) were found using some mathematics of frequency distribution that this reviewer does not fully comprehend. The real advance of this paper appears to be in the mathematical detail of how one processes the frequencies of occurrence of the words to find the words that are most distinctive. Specifically, the authors' newly invented CM_1 score for a word's distinctiveness within the dataset is a refinement of Welch's *t*-test, itself a refinement of Student's *t*-test, to suit a particularly common situation in authorship attribution testing. That situation is where one is comparing a set of plays by a single author with a set of plays known to be by different authors, as in 'Shakespeare versus the Marlowe-Jonson-Middleton set'. The article's authors were able to show that their new CM_1 score beats the usual *t*-test by feeding its results for these 168 plays into the WEKA machine-learning software package, the algorithms of which are not disclosed in this article (although the software is open source), and using 50 of its methods to produce models of authorship based on these data. That is, WEKA was asked to develop tests for authorship based on the frequencies of occurrence of the most distinctive words (as scored by CM_1), which tests were then ranked for how reliably they did in fact detect authorship, and the most effective tests were isolated. The efficiency of these tests (based on CM_1 scoring) was then compared using the same tests based on *t*-test scoring to show that CM_1 is better.

Douglas Bruster and Genevieve Smith, 'A New Chronology for Shakespeare's Plays' (*DSH* [2014] n.p.), offer a new chronology of Shakespeare's plays based on a new analysis of existing verse-style data, and it is largely but not entirely in agreement with the widely accepted chronology. This study uses Ants Oras's pause counts to put the plays in a new order, and other data are brought in to anchor the chronology, such as particular plays' known dates of first performance and the known dates of theatre closure due to plague. (The last of these will, of course, require some assumptions about how Shakespeare reacted to the theatres being closed: did he cease writing plays or carry on regardless?) Oras counted pauses in each syllabic position from 'after 1' to 'after 9' and tabulated the result, using three strengths of pause: A (the weakest) marked by any punctuation, B marked by any punctuation stronger than the weakest punctuation, which is a comma, and C marked by a change of speaker. The use of iambs makes the pauses tend to come after evenly numbered syllables. Early in his career Shakespeare favoured pauses after position 4, but he gradually shifted to favouring position 6, or at least the second half of the line, over his lifetime.

The Oxford *Complete Works* of 1986–7 used Oras's lists to help produce its chronology, but in many cases it insisted on an order that does not quite follow Oras's trends. As Bruster and Smith admit, this sometimes is inevitable since Oras's data put *2 Henry IV* before *1 Henry IV* and *The Tempest* before *Pericles*. (A key point here is that this happens if one assumes that the trend that Oras was tracing drifted consistently in one direction, with no reversals where a new play displays less of the phenomenon than its predecessor; this assumption is not obviously sound.) In an article reviewed in *YWES* 83[2004], MacDonald P. Jackson in 2002 more or less confirmed the Oxford chronology by a new statistical examination of Oras's data, but some anomalies stood out. According to Jackson, *The Merchant of Venice*, *The Merry Wives of Windsor*, and *All's Well That Ends Well* are later than the Oxford *Complete Works*' editors reckoned, and *2 Henry IV*, *Troilus and Cressida*, and *Othello* are earlier.

Oras treated each play as equally important for his work, but of course short plays give less evidence than long ones and should be discounted, and so should plays with a lot of prose (because they have less verse). Bruster and Smith describe the statistical technique of Correspondence Analysis (CA) that they use, and it is like the more familiar Principal Component Analysis (PCA) but suited to categorical rather than continuous data. They acknowledge that plays may have no single date of authorship because they are revised over time, and they decide to exclude from their study Oras's C-pauses because they think that shared verse lines are a different phenomenon altogether. Bruster and Smith are able to also add new data from knowledge of Shakespeare's collaborations that was unavailable to Oras.

Having done the PCA and CA analysis that plots the plays on just two axes (each axis representing a bundle of favoured pause positions), Bruster and Smith explain their 'bootstrap' procedure: they resample by randomly choosing various subsets of datapoints to run the PCA and CA again, which 'affords us some measure of uncertainty for our CA scores' (p. 5). Adding uncertainty sounds undesirable, but what they mean is that the resampling enables them to estimate how much uncertainty attaches to their original results, so they can add what are called 'confidence bars' to the data points. Unfortunately they do not explain why resampling enables this. Presumably if the randomly chosen samples give much the same results as the full dataset then the results are more reliable than if the randomly chosen samples give highly different results. But that is just my guess; it may be wrong, and the principle ought to have been explained by the authors. The '95 per cent confidence intervals' from this resampling 'produce a polygon for each play and trace a gradual arc up and to the right' (p. 5). The authors give no detail on how a confidence interval produces a polygon nor what the arc represents nor why it projects upwards and to the right, but presumably each confidence interval is a one-dimensional value for either CA1 or CA2 so that when CA1 and CA2 are plotted as x/y co-ordinates on a graph the result is a polygon. (I would have guessed that it would be an ellipse, so perhaps this explanation too is wrong.) This does not help us understand the arc unless this simply refers to the drift of the polygons over time as the favoured pause position drifts.

A variant of CA called Constrained Correspondence Analysis (CCA) allows the fixing of certain points when trying to find the seriation (= correct ordering), which is just what we need with the Shakespeare plays. The seriation itself comes entirely from the assumption, not yet made clear by Bruster and Smith, of a continuous one-directional drift in CCA scores with no reversing; this is not necessarily an unreasonable assumption but it does need to be foregrounded. The fixed points used to ground the seriation are *3 Henry VI* being written in late 1591, *Henry V* being written in mid-1599, *Pericles* being written in early 1607, and *The Tempest* being written after 1611. The authors provide a helpful diagram showing how an assumption of one-way and steady drift in CCA score gives a straight line running upwards and to the right on a plot in which the *y*-axis is CCA score and the *x*-axis is time. Because we have known CCA scores for certain plays and known dates for those plays, we can fix the *x*-axis's time-scale and hence allow other plays' dates to be derived from their CCA scores on the *y*-axis.

This picture enables the generation of an entire chronology, with 95 per cent confidence intervals, although Bruster and Smith also added in further fixed points based on their acceptance of Leeds Barroll's claim that Shakespeare stopped writing plays when the theatres were closed. Moreover, they were able to add in Marina Tarlinskaja's prosodic data, but these are continuous (as percentages) not classes (like Oras's data) so they ran PCA not CA on it. (Just how they combined the results of their analysis of Tarlinskaja's data with the results of their analysis of Oras's data is not made clear.) Bruster and Smith provide a complete listing of their entire Shakespeare chronology and for each play they give a brief discussion of the evidence and how their results compare with those of earlier studies. The especially noteworthy conclusions are that: *Titus Andronicus*, not *The Two Gentlemen of Verona*, is Shakespeare's first play; that *The Two Gentlemen of Verona* comes as late as 1594; and that *As You Like It* is dated 1597, *Troilus and Cressida* 1598, *Measure for Measure* 1602, *Antony and Cleopatra* 1610, *Coriolanus* 1611, and *The Winter's Tale* and *Cymbeline* 1613. (The last two are especially surprising since Simon Forman records seeing the first—and probably the second depending on how we read his account—in 1611.) At the close the authors give the important caveat that their work assumes 'that Shakespeare's verse line developed in one direction, and regularly, without significant deviation' (p. 16). It is notable and comforting that for many of the plays this new analysis more or less confirms the existing chronology derived by quite different means.

Matt Steggle edited Shakespeare's *Measure for Measure* for the third edition of the Norton Shakespeare, and in a spin-off article, 'The Cruces of *Measure for Measure* and EEBO-TCP' (*RES* 65[2014] 438–55), he shows how judicious use of EEBO-TCP can help us make sense of the play's cruces and emend them where necessary. Steggle gives a technically astute introduction to EEBO-TCP and its strengths and weaknesses, applauding Jonathan Hope and Michael Witmore's term 'prosthetic reading' for what we are doing when we use such a resource. The term is particularly salient when we assert that certain phrasings are absent from any book, since even the most diligent manual reader could not be sure of that for thousands of books, although such a reader might be able to confidently assert the presence of certain phrasings. Steggle rightly complains that research in textual criticism that uses EEBO-TCP frequently fails to give enough detail on just how the claimed results were obtained, and he is scrupulous in this regard.

The first crux considered by Steggle is Escalus's 'Some run from brakes of Ice' (*Measure for Measure* 2.1.39). A widely adopted emendation is Nicholas Rowe's '. . . brakes of vice', and although *vice* fits well in the context it is unclear what a *brake of* it would be. W.W. Skeat objected that a *brake* cannot be a thicket since nobody ever ran away being chased by one of those. EEBO-TCP shows no examples of *brakes of ice* or *breaks of ice*, but '*brakes of* OR *breaks of*' has seventy-eight hits, 'a small enough number to check one by one' (p. 444). Steggle found that in devotional literature the notions such as *brakes of sensuality* and *brakes of vanitie* show that 'the words "brakes of" can indeed, in writing of this period, be followed by an abstract noun introducing a metaphorical register' (p. 444) and that the recurrent idea is to avoid them. So, he supports Rowe's 'brakes of vice' emendation.

The second crux is Angelo's 'Let's write good Angell on the Deuills horne | 'Tis not the Deuills Crest' (*Measure for Measure* 2.4.16–17). Samuel Johnson read this as conditional: if we write that on the devil's horn, then his horn is no longer understood to be his crest (= insignia). Bawcutt read it as imperative: Angelo has discovered that he is no angel, so appearances are deceptive and we might as well write 'good angel' on the devil's horn since we cannot trust that his appearance reveals his true nature. Alwin Thaler understood the idea to be that all sorts of people are now like the devil so his crest no longer exclusively denotes him. A number of critics have taken the antecedent of *'Tis* to be not the horns but the inscription 'Good Angel', and others have argued that we cannot make sense of this crux and emendation is needed, with various, not terribly widely accepted, proposals.

Steggle points out that a crest does not have to be worn by the person it denotes but could be carried by, for example, servants on their livery. One could wear the devil's livery or crest, and indeed in *Measure for Measure* Isabella goes on to say exactly that about Angelo in 3.1, that he is wearing 'the cunning Liuerie of hell'. Steggle finds in Richard Braithwaite's work an occurrence of *devil's crest* meaning his livery (sometimes literalized in fancy clothes) that we humans wear when we sin. This enables Steggle to gloss Angelo's lines as saying that 'When the devil looks like the devil, with horns and so on, you can see him for what he is. His threat is neutralized, then, and you might as well call him harmless. The real danger is the grave-seeming person with hidden evil intent. They are the ones wearing the true livery of the devil' (p. 448). This seems rather a lot of meaning to be compressed into the thirteen words of the crux.

The third crux is Angelo's 'Admit no other way to saue his life | (As I subscribe not that, nor any other, | But in the losse of question) that you . . .' (*Measure for Measure* 2.4.88–90). The really tricky bit is 'in the losse of question', which has been glossed a number of unconvincing ways, including 'for the sake of argument' and 'provided that nothing can be said in his defence'. Others have tried emending the words and/or punctuation, for example by moving the closing bracket to after 'other' and reading 'But in the loss of question' as 'when his case is lost'. But Steggle has found in EEBO-TCP that losing the question was a common idiom: in disputes it meant losing the thread and going off-topic, and that suits the context admirably.

The fourth crux is Isabella's characterization of Angelo as one whose grave appearance and pronouncements 'Nips youth i' th' head, and follies doth emmew | As falcon doth the fowl' (*Measure for Measure* 3.1.89–90). The difficulty here is the meaning of 'emmew'. Thomas Keightley's emendation to *enew*, 'the hawking term for a falcon driving a fowl into the water (*en eau*)' (p. 451), has been widely accepted. But Steggle finds that, despite the apparent French etymology, this hawking term was often spelled *emew* and the sense of enclosing (*mewing up*) seems also active. The fifth crux is Angelo's claim that his 'Authority beares of a credent bulke | That . . .' no one will dare dispute his honesty (*Measure for Measure* 2.4.25), where *credent* seems to mean *believable* and *believing* although no one else used it that way, and various emendations have been proposed. EEBO-TCP shows Steggle that this reading is indeed most unusual: a *bulk* can be the direct object of *bear* but not of *bear of*, nor of *bear off* (the currently favoured emendation). Steggle proposes the emendation *bears so far credent bulk* and EEBO-TCP gives plenty of parallel phrasings: belief and disbelief are often conveyed in metaphors of physical distance.

Lukas Erne and Tamsin Badcoe, in 'Shakespeare and the Popularity of Poetry Books in Print, 1583–1622' (*RES* 65[2014] 33–57), show that poetry books consistently enjoyed about twice or thrice the market share of all books that was enjoyed by plays, but reprints were rarer, that Shakespeare's *Venus and Adonis* was the most popular poetry book of its age, that his *The Rape of Lucrece* and *The Passionate Pilgrim* did very well, and that his *Sonnets* was merely typical in not getting a reprint since 80 per cent of such books did not. Erne and Badcoe focus on the popularity of all poetry books published in 1583–1622; as they point out this is companion work to that done by Alan Farmer and Zachary Lesser on the popularity of play books (reviewed in *YWES* 86[2007]). Of necessity they start with some definitions of terms. What makes a book, which may have mixed content, a poetry book? Their answer is that it has to be 'chiefly' verse. What is a book? Answer: not a broadside. What counts as a second edition if the contents change? Answer: they rely on the Short Title Catalogue to make this call. How should we count republication in collections of mixed authorship and miscellanies? Answer: they do it on a case-by-case basis depending on how much of the collection comprises material from the first edition.

Erne and Badcoe's first table shows raw counts for poetry books published each year in 1583–1622, and the average is 17.5 first editions and eight reprints a year. The numbers rise quickly at the beginning of this period before plateauing, and the key transitional year seems to be 1594, which of course is when both Shakespeare's narrative poems were newly out and when playbooks flooded the market too. Political events such as the death of Queen Elizabeth in 1603 and of Prince Henry in 1613 seem to have caused spikes in commemorative poetry books. What about the market share enjoyed by poetry books? We have from Peter W.M. Blayney's work the figures for the total book market size, and it was growing rapidly. Since poetry book sales were largely static after the 1594 jump this means that poetry books had a decreasing share of the market. On average across the period poetry books had about 10 per cent of the market share for all books.

Erne and Badcoe discover that there were always twice or three times as many poetry books on the market as play books, and as demand for one rose or fell so did demand for the other. Of course, numbers of first editions indicate what publishers think will sell, but numbers of reprints indicate what actually did sell. There being more poetry books published than play books, the reprint rate for poetry books was much lower (about half) than that for play books. Perhaps, speculate Erne and Badcoe, the continually renewed publicity for successful plays in the form of theatre revivals kept driving up demand for play reprints. Shakespeare bucks the trend for poetry books generally: he had few published and they were often reprinted. Because there were so many poetry books published, Shakespeare never dominated this market as he did the play-books market if we count first editions. But if we count reprints instead he out-performed the average by a long way. Within that profile, *Venus and Adonis* and *The Rape of Lucrece* were extremely popular (compared to the market average) and even *The Passionate Pilgrim* was well above average. The failure of *Sonnets* to get republished was just normal for this market: as noted, 80 per cent of poetry books were not reprinted. *Venus and Adonis* in particular popularized the heroic sestet verse form as well as the epyllion content concerned with 'youthful eroticism, luxury, and transgression' (p. 49). In 1608 Robert Raworth tried and failed to publish a pirated edition of *Venus and Adonis* (he was caught), which was worth the risk because this was 'the best-selling poetry book of its time, going through more editions than any of the other 701 poetry books first published between 1583 and 1622' (p. 53).

As well as producing his own studies showing that *A Lover's Complaint* is by Shakespeare, MacDonald P. Jackson, in '*A Lover's Complaint* and the Claremont Shakespeare Clinic' (*EMLS* 16:iii[2013] n.p.), is able to show that others' studies that reach the opposite conclusion are flawed. This essay is a critique of the methods by which Ward E.Y. Elliot and Robert J. Valenza of the Claremont Shakespeare Clinic have dismissed *A Lover's Complaint* as non-Shakespearian in an article in *Shakespeare Quarterly* in 1997 and in a collection of essays called *Words That Count* edited as a Festschrift for Jackson in 2004 and reviewed in *YWES* 85[2006]. Their method was to devise a series of counts of various features in the works and then set upper and lower limits for these counts so that as many as possible known-Shakespeare works fall within the boundaries and as many as possible known-non-Shakespeare works fall outside them. Their failure was that they did not hold aside—in the sense of not using them in the determining of their boundaries—some blocks of known-Shakespeare poetry in order to test the validity of their boundaries. The fact that the known-Shakespeare poetry almost all falls within their boundaries is misleading, since that is what those boundaries were set to achieve.

Jackson repeats the Elliott and Valenza method using just *Venus and Adonis* and *The Rape of Lucrece* to set the boundaries and shows that the resulting limits would declare the *Sonnets* to be un-Shakespearian by a greater margin than it declares *A Lover's Complaint* to be un-Shakespearian. (Or rather, he uses their own published data to recalculate the boundaries without actually running their tests again.). Elliott and Valenza's method uses what they call 'handfitting' to determine the upper and lower boundaries: they moved them around manually to include the Shakespearian and exclude the non-Shakespearian. Jackson shows that it is better to use a consistent mathematical procedure to set the boundaries, based on averages and allowing two standard deviations from the average in either direction above and below the average to be the boundaries. Applying this rule with Elliott and Valenza's own results, Jackson is able to show that the tests get better at excluding the non-Shakespeare and that *A Lover's Complaint* now looks Shakespearian.

Jackson has a specific objection to one of Elliott and Valenza's tests, which counts the rates of use of *no* and *not* and divides the occurrences of the former by the occurrences of both. This test is demonstrably incapable of separating Shakespearian from non-Shakespearian poetry, for which the averages on this test are almost identical. Because we are here dealing with comparative rates, a text with lots of *no*s and *not*s can have the same rate as one with hardly any *no*s and *not*s, thereby obscuring the vast difference in absolute terms that makes their figures differently significant. Elliott and Valenza are effectively counting just a small subset of all the function words in a text, and instead the proper way to proceed is to count '*all* those [function words] that occur in Shakespeare's works above a certain level of frequency and compare blocks by principal component analysis'. Jackson has further objections to the counting of averages for features such as *with* being the penultimate word in a sentence where the data's standard deviation is very high; that is, some blocks score highly on this test, some score zero, and the average is not really typical of any one block. Other critiques are that tests derived from plays are demonstrably (Jackson shows it) not reliable for poems and vice versa, and that some of the features they measure, such as rates of feminine endings in verse, were clearly drifting over time. With all these faults, Elliott and Valenza's tests' finding that *A Lover's Complaint* is not by Shakespeare should not be taken as substantial evidence in the matter.

John Jowett, 'Disintegration, 1924' (*Shakespeare* 10[2014] 171–87), traces just why E.K. Chambers chose the word *disintegrators* for those whose approaches to Shakespeare scholarship he vigorously rejected. The rhetorical power of the word *disintegration*, which as Jowett shows still gets used pejoratively about those investigating Shakespeare's co-authorship of plays, comes from Chambers's 1924 talk 'The Disintegration of Shakespeare', which 'encapsulated cultural anxieties flowing from theoretical science and reinforced by inter-war fears among the English elite of weakening social cohesion' (p. 172). Disintegration was, of course, a modernist concern, with Albert Einstein breaking up the Newtonian certainties and postwar social cohesion breaking down. Ernest Rutherford had recently split the atom—named from *a-tome*, meaning indivisible—and he used the word *disintegration* in the titles of several of his works. For Chambers the word especially connoted the social disintegration warned of in the 1860s in Matthew Arnold's *Culture and Anarchy*, and for him English literature and especially Shakespeare were bulwarks against that. Chambers's disintegrators were of two kinds: (1) the author-attribution specialists giving Shakespeare's works (or parts thereof) to other authors, and (2) A.W. Pollard and John Dover Wilson for their particular form of New Bibliography.

As Jowett notes, Chambers was accepting of Wilson's ideas when they matched his own. Jowett traces the biographies of the two men and their connected labours in developing the British schools system and their both being adherents of Matthew Arnold's ideas about the positive social benefits of education in general and English literature in particular. The early Wilson thought that Shakespeare's plays were much worked and reworked, coming originally from other authors, and that the print editions give only the illusion of 'stability and integrity' (p. 180). Chambers strenuously denied the idea of endless revision of plays and that this took place in endlessly revised manuscripts, the 'continuous copy' theory. The manuscript of *Sir Thomas More* created this impression of endless, untidy revision, and the key question is how typical one takes this manuscript to be. Jowett sketches the harmful effect on authorship-attribution scholarship that Chambers's essay had for decades after its publication. We live now in a age that does not value literary integrity and coherence, and yet authorship attribution scholarship is not fashionable, for lingering postmodernism rejects its very model of authorship as something assignable to a person. Jowett argues that we can think of co-authorship 'as an articulated conversation or contestation between authors' (p. 182) so that it is both social and individualistic: 'the development of Shakespeare's drama can be re-animated as a narrative of intersections with other dramatists and other dramatic styles' (p. 183).

Thomas Merriam, 'Was Munday the Author of *Sir Thomas More*?' (*Moreana* 151[2014] 245–56), argues that the Original Text of *Sir Thomas More* looks, on certain tests, rather Shakespearian, so perhaps Shakespeare worked with Anthony Munday on that as well as the later additions. He starts by summarizing his own 1987 article that counted 'Five stylometric word habits' in Munday's *John a Kent and John a Cumber* and Munday's contributions to the two parts (*Death* and *Downfall*) of *Robert Earl of Huntingdon*, and *Julius Caesar*, *Titus Andronicus*, *Edward III*, and the Original Text of *Sir Thomas More*. This article claimed that Shakespeare was more likely than Munday to be the author of the Original Text of *Sir Thomas More*. Unfortunately, the habits in question are not described in enough detail. For example, does the habit '*be* followed by *a*' mean followed immediately after or at some distance? If the former, then why not simply say he counted the occurrences of the bigram *be a* and if the latter we need to know the maximum permitted distance. Likewise for the habit '*be* not followed by *a*' are we saying that some other word (other than *a*) had to follow *be* or would it count if *be* were at the end of a sentence, or line, or speech, which are all other ways for *be* to be not followed by *a*? The same kind of uncertainty that could easily have been cleared up applies to all the other habits counted.

Merriam repeats these same tests now with all thirty-six plays in the Folio and plots the resulting data's two principal components for each play—presumably reducing each play's five data points to two—as a scatter-graph. The Shakespeare plays all cluster together with the Original Text of *Sir Thomas More* and *John a Kent* and *John a Cumber*, and the *Robert Earl of Huntingdon* plays cluster together away from this group. But, as Merriam admits, *Edward III* also clusters with the Shakespeare plays despite having only quite a small bit of Shakespeare in it, so Merriam has not proved that this test is good at distinguishing authorship in general. What his test needs is systematic validation by being given randomized samples of plays where the authorship is known and seeing how convincingly it distinguishes plays taken out of that sample and tested as if they were of unknown authorship; this would give an overall reliable rate we could judge. Being right more than 90 per cent of the time would be good.

Rodney Stenning Edgecombe offers some emendations and reinterpretations of particular cruces in Shakespeare in 'Five Notes on Shakespeare' (*BJJ* 21[2014] 289–302). Where the Folio text of *1 Henry VI* has 'I: Beauties Princely Maiesty is such, | Confounds the tongue, and makes the senses rough' Edgecombe thinks *rough* needs emendation because senses cannot be made so. He suggests *rush* as it nearly rhymes, and other pairs of lines in this scene rhyme, and elsewhere in Shakespeare the senses, similarly confounded, take flight. In *2 Henry IV* the Lord Chief Justice says that Falstaff lives in 'great infamy' and Falstaff replies 'He that buckles himself in my belt cannot live in less' (1.2.138–40). The joke, according to Edgecombe, is not about Falstaff deliberately pretending that he thinks *infamy* is a cloth he might wear, but that he takes it to be a word meaning hunger (as in *famished*). In Falstaff's claim that 'The young Prince hath misled me. I am the fellow with the great belly, and he my dog' (*2 Henry IV* 1.2.146–7) it is not clear why he calls Hal his dog. Edgecombe reckons that the point is the topsy-turvydom of the dog leading its master, which is what the man-in-the-moon's dog does to him—in one tradition, that dog is really the devil—and Falstaff says elsewhere that he and his crew are minions of the moon (*1 Henry IV* 1.2.26).

The Folio text of *All is True*/*Henry VIII* has the Duke of Buckingham complain of Wolsey that 'his owne Letter | The Honourable Boord of Councell, out | Must fetch him in, he Papers', which many editors leave unemended. Edgecombe thinks that it makes more sense if the comma in the second line is moved to the end of the line and *he* changed to *the*, giving '. . . The honourable board of council out, | Must fetch him in the papers', which he glosses as meaning that Wolsey puts the king's council out of the picture (circumvents them) and writes letters demanding money from various gentlemen who 'are de facto inscribed in his list or file ("must fetch him in the papers")' (p. 295). I cannot see how 'must fetch him in the papers' means that. When Buckingham says 'my life is spand already: | I am the shadow of poore Buckingham, | Whose Figure euen this instant Clowd puts on, | By Darkning my cleere Sunne' the precise meaning is not immediately clear. Edgecombe suggests that adding a *d* to *figure* solves the problem because then *even* clearly means *evening*. Edgecombe does not make explicit what he thinks the adjective *figured* does in modifying *evening*, but it seems to have the same meaning as *prefigured*.

Comparing herself to Sylvia, Julia in *The Two Gentlemen of Verona* says 'What should it be that he respects in her, | But I can make respectiue in my selfe?' Most editors gloss *respective* as *make worthy of respect*, but Edgecombe thinks that Julia uses the word in the sense of *in respect of* (= regarding) herself and with a pun on *respicere* (Latin for providence) so he puts a comma after *make*. The gloss he gives to the resulting construction seems to me too complex for the occasion it serves. Sonnet 119 has the lines 'How haue mine eies out of their Spheares bene fitted | In the distraction of this madding feuer?' Edgecombe objects to the usual gloss of *fitted* as meaning *convulsively dislodged* (from *fit* = *seizure*) and reads this as an example of Shakespeare thinking of one person seeing with another's eyes, so that eyes that are *fitted* are eyes 'taken out of their spheres and fitted into others'' (p. 300). In a separate note, 'The ''Present Quality of War'' Crux in *2 Henry IV* 1.3' (*ShN* 63[2014] 96), Edgecombe attends to Folio *2 Henry IV* having Lord Bardolph say 'Yes, if this present quality of warre, | Indeed the instant action: a cause on foot, | Liues so in hope'. Edgecombe proposes turning this into sense by emending *indeed* > *indued* (in the sense of clothed). That is, if 'Hotspurian impulsiveness' were clothed with the quality of war—namely foresight, which Bardolph is about to detail *present*ly—then, yes, a military cause is hopeful. This is indeed better than the currently accepted emendations of punctuation only.

An article by Terri Bourus and Gary Taylor, '*Measure for Measure*(s): Performance-Testing the Adaptation Hypothesis' (*Shakespeare* 10[2014] 363–401), reports that Bourus directed her university theatre company in two versions of *Measure for Measure*: one based on the Folio text and one on the pre-Middleton-adaptation version as constructed by John Jowett's 'genetic text' for the Oxford Collected Middleton. The practitioners found the aesthetic effects of the two productions quite different even though the textual variants are not extensive; a few strategically placed changes can make a lot of difference. An interesting and previously overlooked point is that the revival of the play must have opened at the Blackfriars theatre because as John Jowett showed (in work reviewed in *YWES* 88[2009]) the adaptation occurred between 6 October 1621, when one of the sources for it became available, and late March 1622 when Crane's transcript was handed to the Folio printers; that being winter-time the King's men would be using their indoor theatre. The Globe performances of the original version of the play in 1604 probably ended with a jig, which was usual, but these were banned, and the 1621 performances probably omitted the jig. The remainder of the article has many interesting and important comments on the performance consequences of the textual differences between the two versions of the play, but they fall outside the scope of this review.

Pervez Rizvi, in ‘Stemmata for Shakespeare’s Texts: A Suggested New Form’ (*PBSA* 108[2014] 96–106), proposes a new way of writing editions’ stemmata in a tabular form with horizontal rows for individual textual objects (manuscripts, editions), with time running left to right, with boxes in each row denoting the transformations of the object in that row, and with lines between the boxes denoting acts of copying or consultation. One possible objection to this admirable scheme is that it has places for physical objects but not for texts that get stored in actors’ heads when they read their parts and that later get expressed in performances. The stemma for *Henry V* in the *Textual Companion* to the Oxford *Complete Works of Shakespeare* has a place for ‘performances’ and Rizvi objects that ‘since a performance is neither a material object nor a change in a material object’ (p. 98) it should not be in the stemma. Thus in his stemma for this play there is ‘no arrow between the promptbook and the memorially reconstructed text. This shows that the material object called the promptbook was neither copied nor consulted in creating the material object called the memorially reconstructed text’ (p. 100). One can see the logic of Rizvi’s argument, but if we follow it we lose the link between the authorial papers and the performances they gave rise to. In his stemma the actors seem able to create a memorial reconstruction of something, the script of the play, without there being any connection (any line) between this reconstruction and the thing it reconstructs. How did they do this? If Rizvi could address this objection—perhaps by indicating why it is illusory for the kind of work that stemmata should do or else by making a place for the actors’ parts as physical objects—then his plan for a new layout would clearly improve on the existing design.

And so to the round-up from *Notes and Queries*. Karen Britland, ‘Psalm 140 and Diana’s Crux in *All’s Well That Ends Well*’ (*N&Q* 61[2014] 241–4), returns to the familiar crux that Suzanne Gossett looked at above. In the Folio *All’s Well That Ends Well*, Diana says to Bertram (who is trying to seduce her): ‘I see that men make rope’s in such a scarre, | That wee’l forsake our selues. Giue me that Ring.’ What is meant by ‘rope’s in such a scarre’? Britland approves of P.A. Daniel’s emendation of *scarre* > *snare* that Gary Taylor found impossible because one does not get roped in a snare. The curious *rope’s* Britland considers a simple plural with an obsolete apostrophe. (I would have thought that it is far from obsolete and its currency has given it the slang name of a greengrocer’s apostrophe.) What about the problem that the image of snaring does not suit well the idea of forsaking oneself? Britland explains this by pointing to Psalm 139 (or 140 according to varying religious opinion) that refers to the ropes and snares by which the innocent are led by the sinful to forsake themselves. (In fact that is not quite what the psalm seems to say, so there is some forcing of the argument to make the allusion seem to fit.)

Thomas Merriam, ‘A Phrasal Collocation’ (*N&Q* 61[2014] 231–3), ponders in a general and noncommital way how texts come to use the same strings of words. Charles Forker’s edition of *The Troublesome Reign of King John* listed a lot of trigrams (and longer) in common between that play and the plays of George Peele, and Merriam wonders how *n*-grams with a large *n* (such as 7) can come to be in common between two works. He has a particular example in mind: ‘the queen and her two sons; And’ which appears in Shakespeare’s *Titus*

Andronicus and Robert Southwell's *The Epistle of Comfort* (printed perhaps in 1587). Merriam seems to make no more of this than the conclusion that it shows 'the influence of a Jesuit on Shakespeare', which does not seem especially helpful.

Jane Kingsley-Smith, 'A Method Unto Mortification: A New Source for *Love's Labour's Lost*' (*N&Q* 61[2014] 233–6), reckons that *Love's Labour's Lost* was partly inspired by a Protestant theological work about renouncing the vanities of this world. In the play Dumaine says of himself 'Dumaine is mortified', in the sense of having suppressed his appetites. Kingsley-Smith reckons that Shakespeare got the word and the idea from Thomas Rogers's book *A Method of Mortification* (published in 1586), a Protestant theological work based on a Catholic original. The original Catholic author, Diego de Estella, was born in Navarre and, having disapproved of court life there, was forced into a monastery. This biography sounds somewhat like that of the play's Don Armado—Shakespeare's first and most memorable Spanish character—who comes to Navarre's court and considers himself one of those who has signed up for its ascetic life. As Kingsley-Smith shows, the concerns of Rogers's book—just how does one abjure worldly vanities?—are like those of the play, and she traces a number of parallels. Both allude to Ecclesiastes 13:1 on being defiled by touching pitch, both refer to breath being a vapour that is destroyed by sunlight, both refer to stumbling in the darkness of moral ignorance, and both have Judas Maccabeus hurt by idle words.

Chunxiao Wei, ' "Saint Peter's Church" in *Romeo and Juliet*' (*N&Q* 61[2014] 236–8), responds to the claim by Richard Paul Roe in *The Shakespeare Guide to Italy* [2011] to have identified the particular St Peter's Church used by the Capulets in *Romeo and Juliet*. Rather pointlessly, Wei sets out to debunk all this as geographically and historically implausible. It could just as easily be dismissed on literary-historical grounds: we have no reason to think that Shakespeare was using local knowledge since his sources account for everything. Henry Buchanan, '*The Merchant of Venice*, III.ii.99: A Proposed Emendation' (*N&Q* 61[2014] 238–40) has a new solution for the crux in Bassanio's speech about a 'beauteous scarf | Veiling an Indian beauty' (*Merchant of Venice* III.ii.98–9), which has puzzled editors by its notion of beauty hiding beauty. Buchanan reckons that *scarf* is a nautical term for the sails of a ship, which veil its valuable interior or *booty*. (I cannot see that a ship's sails, which go on top, veil its insides.) Buchanan reckons that the line should be emended to refer to the 'beauteous scarf | Veiling an Indian booty', which of course suits the wider maritime-commerce theme of the play.

Leo Daugherty, 'A Previously Unreported Source for Shakespeare's Sonnet 56' (*N&Q* 61[2014] 240–1), reports a new source for one of Shakespeare's poems. Sonnet 56 begins 'Sweet love, renew thy force. Be it not said | Thy edge should blunter be than appetite, | Which but today by feeding is allayed, | Tomorrow sharpened in his former might'. Daugherty hears in this an echo of George Whetstone's sonnet (in *The Rock of Regard* [1576]), 'First loue renue thy force, my ioyes for to consume . . .', for which the context of rekindling lost love with verse is the same. David George, '*Hamlet* and the Southwark Ghost' (*N&Q* 61[2014] 244–6), also has a new source. A ghost story published in 1674 seems to have echoes of Hamlet's father's ghost returning from the dead to talk to him, including the shared detail of an orchard: in *Hamlet* that is where the murder takes place, and in the ghost story the murdered man is buried—and maybe was murdered—in his orchard. Furthermore, in the ghost story, the murderer initially gets away with it, his wife is none the wiser, the ghost walks about in a cellar, and the murdered man has buried wealth, which last detail matches what Horatio speculates is the cause of the ghost's return in *Hamlet*. Also, in the story the ghost says that he must not speak of his experiences after death. When published, the story included accounts of the ghost appearing in Southwark in the 1500s. George points out that the known sources for *Hamlet* lack these details and wonders if Shakespeare heard of this story in Southwark prior to writing the play.

Ingrid Benecke, 'The Shorter Stage Version of Shakespeare's *Macbeth* as Seen through Simon Forman's Eyes' (*N&Q* 61[2014] 246–53), reckons that *Macbeth* as we know it from the Folio was cut before being performed at the Globe in 1611 and seen by Simon Forman, with the omissions being reflected in what Forman omits. Benecke begins with the surprising assertion, for which no reason is given, that Forman's eyewitness account of *Macbeth* at the Globe in 1611 was not influenced by his knowledge of Raphael Holinshed's prose chronicles. (It is commonly thought that Forman's reference to the Weird Sisters as 'feiries or Nimphes' might be such a recollection of Holinshed.) The next sentence is even more confusing as it claims that the play was 'written before 1610/11, most likely between the year 1603 and sometime after spring 1606'. That last clause covers all time from 1606 to the present day, so presumably Benecke means 'in 1606, after its spring'. The next sentence begins 'It therefore . . .' but it is unclear what the antecedent is; the previous sentence began with its own backwards pointer ('That supports . . .'), and the one before that simply asserted that Forman's account is 'closely related' to the Folio text instead of Holinshed's account, which seems to be stating the obvious: Forman primarily recalled the play he saw, not its source. I have no idea what the author is trying to tell us by all this.

Then begins Benecke's account of how Forman's notes differ from the play we have in the Folio, and it wrongly ascribes agency: Forman 'cuts the number of characters down to Duncan, Macbeth and his Lady, Banquo, ''Dunkins 2 sonns'', and the Macduffs' (p. 247). Surely, no, he simply does not recall or think it worth his trouble to mention the others; this is not cutting in any recognized sense of that term. Because in Forman's notes the main characters are 'flat' and 'far less complex' than in the Folio, Benecke wonders if the psychological subtlety we know from the Folio was not present in what got performed at the Globe in 1611. The simpler explanation would be that it takes many words from a great artist to convey psychological complexity, and brief notes from a physician-spectator cannot be expected to do it.

Benecke rehearses the familiar observation that it is surprising that Forman did not mention the play's explorations of the physical manifestations of mental illness, since that would have been bound to interest him. In places Benecke uses language to describe Shakespeare's play that critics and theatre practitioners might well consider to be highly loaded, for example calling the Weird Sisters 'malevolent hags'. Benecke has a most peculiar notion of the hypothesis that the play was revised between the time Forman saw it and the printing of the Folio—that is, the Middleton adaptation theory—in that she wonders whether 'Hecate and her subservient spirits (III.v, IV.i) were excised' (p. 248) for the show Forman saw, whereas of course the adaptation hypothesis, of which she seems only dimly aware, is that they were added by Middleton after Forman saw the play. Benecke offers many observations and speculations about the play as staged in 1611 based on what Forman does not write, and appears not to appreciate that this is all much too speculative because we do not know why someone might have not recorded something. Some of the writing is particularly awkwardly phrased: 'Macduff meeting Macbeth's enemy in England can be taken to be a traitor to the Scottish throne' (p. 248) and 'He thus centres treasonable evil on Macbeth' (p. 249). In the second half of the article, Benecke gives a second complete summary of how the play looks in Forman's account. According to Benecke, the Forman account represents a coherent cutting of the play and since that cutting is unlikely to have been done by Forman she concludes that what was performed at the Globe was a cut version of the Folio text.

Thomas Merriam, 'A Reply to "*All Is True* or *Henry VIII*: Authors and Ideologies"' (*N&Q* 61[2014] 253–6), reckons that his redistribution of the shares of Shakespeare and Fletcher in *All is True/Henry VIII* is confirmed by a fresh look at some old data. This is a response to MacDonald P. Jackson's article in *Notes and Queries* the previous year (reviewed in *YWES* 94[2015]) in which Jackson showed that the moving of the boundaries of the authorial stints proposed by Merriam would give to Fletcher passages that have Shakespearian (and un-Fletcherian) rates of various verse features and likewise give to Shakespeare passages that have Fletcherian (and un-Shakespearian) rates of various verse features. Merriam responds that 'Metrical and linguistic criteria, of the kind which Jackson carefully summarizes, are not *by themselves* capable of delimiting texts by author' (p. 253).

Merriam offers a cumulative sum (cusum) graph, reprinted from 'page 424 of *Notes and Queries* ccxlviii (December 2003)' (p. 254), which is a cryptic reference: he means it is from his article 'Though This Be Supplementarity, Yet There Is Method In It' (reviewed in *YWES* 84[2005]). But in fact it is not quite a reprint: the picture on page 424 of that article looks quite different from the one reproduced here in the overall shape of the graph and the horizontal axis's labelling. This last point is the clue to why the graphs look different: in the original, the *x*-axis ran from 0 to 3500 (representing sequential lines in the play) and in the present one it runs from 400 to 700. Thus the present graph is about a one-tenth part of the original that has been stretched horizontally by a factor of ten while the *y*-axis (which has no scale in the original or the reproduction) remains unstretched.

A cusum graph like this shows, line by line in the play, the total of all occurrences of a set of words and verbal features: counts of *all*, *are*, *conscience*, *did*, *'em*, feminine endings, *find*, *from*, *hath*, *in*, *is*, *it*, *little*, words ending in *-ly*, *must*, *now*, *sure*, *they*, *'tis*, *too*, and *where/there*. To understand how the graph is made and hence how to read it, one must turn not to Merriam's 2003 article but to the article to which that one refers its reader, in *Literary and Linguistic Computing* from 2000. The method is that for each word or feature the total number of occurrences in the play is counted and divided by the number of lines, which gives us the expected number per line if this word or feature were uniformly distributed across the play. A typical figure might be, say, 0.25 for *and*, meaning that we expect one *and* every four lines. For each line of the play is plotted how many times that feature has occurred up to that point (hence *cumulative* sum), minus the number we would expect to be the sum for that feature up to this point if the feature were uniformly distributed across the play.

Thus if the first *and* occurs in line 3 and there is another in line 4 but none in lines 5 or 6 then the cusum figures for lines 1 to 6 would be –0.25, –0.50, 0.25, 1.0, 0.75, and 0.50. Plotted with line numbers running horizontally and cusum figures running vertically, this means that negative slopes (running north-west to south-east) represent parts of the play where the feature is consistently occurring less often than expected (say because author A wrote those lines), as with our first, second, fifth, and sixth lines having deficits of *and* (none, where 0.25 is expected), and that positive slopes (running south-west to north-east) represent parts of the play where the feature is consistently occurring more often than expected (say because author B wrote those lines), as with our third and fourth lines running a surplus of *and* (one each, where 0.25 each is expected).

Merriam talks the reader through his cusum graph, referring to where the 'breaks' occur, but it is not clear to this reader just what he means by these breaks because there are micro-trends of positive and negative slopes occurring within larger trends that are generally positive or generally negative. In other words, just what counts as an overall positive or negative slope is a function of how closely one looks at the data. But for the present application this is not a serious problem because the passages to be redistributed between Shakespeare and Fletcher occur at known line numbers so Merriam is able to isolate in a separate picture exactly which parts of the graph refer to those passages. As he rightly claims, for the seven passages that he has reallocated from Fletcher to Shakespeare the slopes are clearly positive (indicating Shakespearian authorship). But as he admits, for the two passages that he has reallocated from Shakespeare to Fletcher the slopes are not so clear: 'mostly negative slopes [= Fletcherian] except for their tails' (p. 256).

N.M. Ingebretson, 'A Hound in Shakespeare's Addition to *Sir Thomas More*' (*N&Q* 61[2014] 256–7), spots a crux where one is not usually thought to exist. In the Hand D part of *Sir Thomas More* (by Shakespeare), More refers to the rebels' desire to 'lead the majesty of law in lyam | To slip him like a hound' (6.137–8). The problem is that a *lyam* is a line used to hold a scenting-dog called a lyam-hound (also called a lymer) while greyhounds were held not on a lyam but on a slip. So, this combination of *lyam* and *slip* is a crux. Even if we think *slip* just means *let loose* the problem does not go away, for lyam-hounds were not let loose to chase the quarry but were kept restrained and walked with their handlers behind the pack to pick up the scent again if the pack lost it. Ingebretson offers no solution to this crux, but simply points it out.

For the same play, Regis Augustus Bars Closel, 'The Marginal Latin Tag in the Manuscript of *Sir Thomas More*' (*N&Q* 61[2014] 257–60), has a solution to an old puzzle. In the middle of the rewritten scene of Erasmus meeting More in *Sir Thomas More* there is a strange marginal Latin line '*Et tu Erasmus an diabolus*' (8.191). (It means 'And you are either Erasmus or the devil', which translation Closel neglects to give.) The line comes from anecdotes about More that circulated at the time of the play, and his great-grandson Cresacre More referred to it in his biography of More published around 1631 and possibly in manuscript circulation before then. The line in question is More's response to Erasmus's line, told in the anecdote but not in *Sir Thomas More*, of '*Aut tu es Morus aut nullum*'. (This means 'Either you are More or nobody', and again Closel does not help the reader here.) Closel's suggestion is that both Latin lines were meant to appear in the play as an exchange ('Either you are More . . . And you are either Erasmus . . .') in the midst of their conversation when they recognize one another. Closel is not sure if this exchange was part of the original writing or added during the revisions.

Also on *Sir Thomas More*, Thomas Merriam, 'Determining a Date' (*N&Q* 61[2014] 260–5), reckons that the Original Text was written before the mid-1590s, to judge from its rare-word usage. Different readers of the manuscript of Anthony Munday's *John a Kent and John a Cumber* see different figures in the date written on it, which is not in Munday's hand. Some see 1590, some 1595, and some 1596; most recently, MacDonald P. Jackson, in an article reviewed in *YWES* 92[2013], saw 1596. Fresh examination of the manuscript shows that part of Munday's signature overwrites or is overwritten by part of the date, throwing further doubt on the date because it is not certain which was written on top of which. Since the date of *Sir Thomas More* depends in part on the date of *John a Kent*, this uncertainty spreads to the former play. Jackson has advanced an argument for a late date for *Sir Thomas More* based on various verse features, such as lots of feminine endings, which did not become so common until after 1600. Merriam objects that a couple of late plays (from 1598–1605) also have low rates of feminine endings. Even if true, a couple of such outlier cases would not disprove the overall trend, which is well established.

Merriam turns to Eliot Slater's method of dating plays using their frequencies of rare words, to which he applies the cusum graphing method to show the occurrences in various plays of the rare words collected by Slater. When graphed so that the *x*-axis is the date and the *y*-axis is number of occurrences of Slater-rare-words that are common to the play to be dated and various plays for which the date (and hence the *y*-axis point) is known, the typical cusum pattern is observed. That is, the occurrences in a given play of Slater-rare-words common to various other plays form a rising slope and a falling slope with the peak between the two occurring at the point occupied by the play in the known chronology that has most Slater-rare-words in common with the play to be dated. For *John a Kent* this play is *A Midsummer Night's Dream* of 1595–6, but for the Original Text of *Sir Thomas More* it is *The Comedy of Errors* of 1594, which is rather earlier than Jowett dates the play in his recent edition of it. As a check, Merriam applies the same test to *Sir John Oldcastle* and his method dates it just as we would expect from the external evidence, which is its dependence on *1 Henry IV* and its completion in October 1599 according to Henslowe's Diary.

Adrian Blamires, 'Ben Jonson's Additions to *The Spanish Tragedy* as the Subject of Ridicule' (*N&Q* 61[2014] 265–8), finds evidence of Ben Jonson's dramatic writing not always being appreciated. Edward Alleyn retired from the stage between 1597 and 1600, and during that period the Admiral's men did not play *The Spanish Tragedy*, which they had often performed before. Alleyn returned to the stage in 1601, and the play was revived with Jonson paid to write additions to it, although these seem not to be the additions that survive in the quarto of 1602. The addition of the Painter's Part at least must have existed by 1599 because it is parodied in John Marston's *Antonio and Mellida* of that year. Richard Burbage must have played Hieronimo because *2 Return from Parnassus* mimics him doing it and his famous funeral elegy recalls his performance in the role. Thus the Chamberlain's men must have played *The Spanish Tragedy* during Alleyn's retirement from the stage.

When Alleyn revived the role at the Fortune, he probably felt he had something to prove, and perhaps Jonson's revisions are connected to that. The boys' company play *The First Part of Hieronimo* (published in 1605) is a prequel to *The Spanish Tragedy* and it seems to contain an allusion to Jonson's additions for the revival of *The Spanish Tragedy* when Hieronimo says of the news that Lorenzo is honest 'Go, tell it abroad now; | But see you put no new additions to it'. Blamires reads this as 'evidence that Jonson fulfilled his task, but that his additions did not find favour, at least amongst the Blackfriars cognoscenti' (p. 268). This seems a lot of weight to put on a small allusion, but then Blamires cheerfully admits that indeed it is.

2. Shakespeare in the Theatre

Shakespeare's canonical prominence has tended to augment the profile of the Chamberlain's/King's men at the cost of other acting companies. Moreover, Shakespeare's attachment as writer-in-residence to this single troupe serves to occlude the extent to which, as Lawrence Manley and Sally-Beth MacLean assert, the circumstances surrounding such companies were in flux in the early 1590s. *Lord Strange's Men and Their Plays* demonstrates that, in many ways, Shakespeare is the exception rather than the rule, and the stability associated with his middle and later career wholly atypical. As Manley and MacLean insist in this assiduously researched book, '1589–93 was marked by exceptional fluidity and volatility (as well as artistic ferment) in the theatrical profession'.

At the heart of their project is the repertory and writers associated with Strange's men, including Robert Greene, Thomas Kyd, Thomas Lodge, Christopher Marlowe, Thomas Nashe, and George Peele. In addition, Manley and MacLean argue for the inclusion of plays by Henry Chettle, Anthony Munday, and Shakespeare. Fortunately the Diary of Philip Henslowe is able to assist them here though not without ambiguities and omissions. The arguments in favour of the inclusion of *Sir Thomas More* and *1 Henry VI*, for instance, rely on hypotheses. They date the former play nearly a decade earlier than its latest editor, John Jowett, whose proposal of 1600 postdates the dissolution of the company by seven years, and they assume that Shakespeare authored rather than revised the latter.

32:ii[2014] 185–206), of Feste's character attempts to work 'at the intersection of performance practice and literary analysis' (p. 186), foregrounding the interdisciplinary methodology as a means of debating character. Proposing a 'strategic essentialism' (p. 188) that is born out of historical stage practice, literary analysis, and actor preparation, Hutchison and Jellerson use the type of character exploration that is typical of the modern rehearsal room as a means of discovering the mysterious 'something' that Feste claims to care for in III.i of *Twelfth Night*, referring several times to the rehearsal processes of unnamed actors. The collaborative work between the performance practitioner and literary scholar yields an admirable close reading, although I suspect the main value of such an article is pedagogical, as it indeed does rely on a textual essentialism that can only work in a more traditional production context; that is, assuming that no directorial concept reconfigures the balance of characters, their relationship to one another, and the setting. Even if the stability of such a practice feels somewhat overstated, this well-researched and diligent essay makes a salient case for academic investment in the 'emotional life' (p. 191) of characters.

In a careful close reading, 'What they Will: Comic Grammar in *Twelfth Night*' (*Shakespeare* 10:ii[2014] 158–70), Rikita Tyson argues that the use of modal auxiliary verbs, particularly 'will', suggests that the play ultimately rests 'hope' on the active use of 'will'. Tyson resists the urge to unpack all instances of 'I will' in *Twelfth Night*, carefully choosing instances that illustrate the verb's association with wish and desire. This modal utterance, which she links with 'can', 'may', 'must', and 'might', 'bring a being into being, demonstrating the thinking, evaluating, and judging that characters are always doing by means of these declarations' (p. 160), locating activity in *Twelfth Night* in small verbal choices made by various characters. Viola's language, for example, posits her will that her brother will survive the shipwreck, creating hope 'through an act of will and wordplay' (p. 163). Hope, in turn, creates the will to seek out the opportunity that leads Viola to Orsino. Tyson carefully traces out the interplay of assertion of, and submission to, the wills of the self and others, to outline how these utterances create a space for action. Ultimately, she argues that Viola's 'actions show us the paradox of will suggested by the fluidity of modal meanings: obligation and volition, inference and demand, can be revealed by, compressed into, and even enacted by these pinpricks of words in the smallest of sentences' (p. 167); this sort of paradox shapes the comic energies of the play, and gives direction to the desires of the characters.

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