A RESPONSE TO GABRIEL EGAN’S NYWES 2017

Darren Freebury-Jones

In his ‘NYWES 2017’, Gabriel Egan extends his annual tradition of attacking and misrepresenting anyone and everyone who disagrees with the conclusions reached in The New Oxford Shakespeare: Authorship Companion (2017), co-edited by Egan and Gary Taylor and more or less universally panned by attribution specialists outside of the New Oxford Shakespeare team. As will be clear to anyone who reads Egan’s contributions to the SHAKSPER forum or who has read my annual responses to Egan’s egregious reviews, he in turn celebrates the work of his colleagues, irrespective of quality. For example, according to Egan, ‘Gary Taylor, John V. Nance and Keegan Cooper’, in a piece containing extremely dubious arguments, as has been exposed by Brian Vickers in a recent article titled, ‘Kyd, Edward III, and “The Shock of the New”’ (forthcoming, 2020), ‘show that Kyd did not write the Mariner speech from Edward 3 (4.137-184) and that we have no reason to agree with Brian Vickers’s claim that he wrote the rest of the play (“Shakespeare and Who? Aeschylus, Edward III and Thomas Kyd”)’. But Taylor et al. do nothing of the kind and Egan repeatedly shows that he is prepared to celebrate questionable scholarship provided it stems from his colleagues, those scholars whose ‘fine literary-critical remarks’ provide a sharp contrast to the ‘Poorly performed computational work on Shakespeare’s writing’ conducted by others.

There are 44 references to ‘Freebury-Jones’ in Egan’s latest instalment. His reviews of two of my articles are designed to cast doubt on my scholarly competence, but anyone who scratches away at the surface of Egan’s remarks, or takes the time to read the articles Egan critiques, will see that Egan’s piling up of imaginary errors merely exposes either the ineptitudes or biases of the reviewer. Here I draw attention to Egan’s misinformation. I limit my observations to Egan’s comments on my work, but readers should know that his accounts of other scholars’ works in these annual self-published reviews are equally unreliable.
My article, ‘Kyd and Shakespeare: Authorship Versus Influence’, establishes that verbal links between Shakespeare plays and Arden of Faversham are unimpressive when Shakespeare’s borrowings from other plays attributed to Thomas Kyd are taken into account.

It seems that one of Egan’s favourite pastimes is to claim that URLs I present in my articles are broken when they are not. Egan writes the following:

To trace Shakespeare’s recall and reuse of others’ phrases in his plays, he turns to Martin Mueller’s dataset called “Shakespeare His Contemporaries” for which Freebury-Jones gives an URL that at the time of review (December 2019) returns a “page not found error”. Even searching for “Shakespeare His Contemporaries” (with and without Mueller’s name) in the popular search engines Google and Bing does not turn up the material Freebury-Jones depends upon, so there is no way to replicate his work although one can use EEBO-TCP to check the parallel phrasings he claims.

The URL I provide works fine at the time of my writing this response (December 2019) and readers are welcome to check it here: https://scalablereading.northwestern.edu/?p=312. The web page also contains a link to the dataset I rely on, so there is no need for Egan to use a different dataset (EEBO-TCP) in an attempt to replicate my work. Put simply, this is poor scholarly practice on Egan’s part. He continues:

Freebury-Jones finds phrases that are unique to Kyd’s Soliman and Perseda and Shakespeare’s 3 Henry 6. Some of these phrases are indeed unique to Kyd’s play and Shakespeare’s, but “I [would|will] not hence till” that Freebury-Jones claims is unique also turns up in Edward 3: “Ye will not hence, till” (STC 7501, B2r); I assume that if “would”/”will” are allowed to stand for each other, then “I”/”ye” are too.

‘Would’ is the past tense form of ‘will’ and the words are therefore semantically equivalent in Martin Mueller’s lemmatized texts. ‘Ye’ is the archaic plural form of ‘thou’. The word ‘I’ is a first-person singular nominative case personal pronoun. ‘I’ and ‘ye’ are not the same, and the pentagram (five-word unit), ‘I would/will not hence till’, is a longer and therefore different match than the tetragram (four-word unit), ‘will not hence till’. Egan might as well be saying that the phrase, ‘You struggle with grammar’, is the same as the statement, ‘I struggle with grammar’.
The study in question revolves around unique tetragrams plus (contiguous sequences of four or more words that occur in only two plays) in Mueller’s corpus. One of the examples I cite is ‘The sweet glances of’. When Egan writes that ‘Freebury-Jones turns to phrases shared by Kyd’s Solimon and Perseda [sic] and The Two Gentlemen of Verona, and one of them, “sweet glances”, seems to be such a commonplace--Greene used it in Arbasto, the Anatomy of Fortune, Heywood used it in Troia Britannica, and so did more than half a dozen other writers’, he is citing a bigram (two-word unit) that is statistically irrelevant to my arguments. Egan continues:

Freebury-Jones revisits Vickers’s attempt to expand the Kyd canon and Jackson’s demonstration (in an essay reviewed in YWES for 2009) that this method is a “one-horse race”. Freebury-Jones tries to undermine Jackson’s work by pointing out that Jackson finds more phrases shared by the non-Shakespearian (according to Jackson) rather than the Shakespearian parts of Arden of Faversham and 2 Henry 6 and shared by the non-Shakespearian (according to Jackson) rather than the Shakespearian parts of Arden of Faversham and The Taming of the Shrew.

(Here we might note the use of the word ‘attempt’ to describe studies conducted by Brian Vickers, who exceeds my mentions in ‘NYWES 2017’ by a total of one, but ‘demonstration’ for a study conducted by Egan’s New Oxford Shakespeare colleague.) Egan claims that ‘Obvious objections to Freebury-Jones’s observation are that the authorship of 2 Henry 6 is itself disputed (many investigators think it co-authored) and that the sole-authorship of The Taming of the Shrew is under suspicion too’. The investigators in question are Egan’s New Oxford Shakespeare colleagues, and the arguments that these plays are co-authored are, in my view, utterly unconvincing. Either way, these so-called ‘objections’ could in turn be aimed at MacDonald P. Jackson’s article, but Egan does nothing of the kind.

The next paragraph of Egan’s review is confusing:

Although Freebury-Jones refers to his adjustment of the counts of shared phrases to compensate for the texts being different lengths, it is not clear from his phrasing that he properly allowed for the non-Shakespearian parts of Arden of Faversham being four-fifths of the play and having, as it were, more opportunity to match with other plays.
Readers of my article will see that I adjusted the figures for Shakespeare matches with both (according to Jackson) ‘Shakespeare’ and ‘non-Shakespeare’ portions of Thomas Kyd’s (by my argument) *Arden of Faversham* according to composite word counts. All I can say is that it is not clear from his phrasing what Egan means here or if he has a better suggestion for the kind of statistical control used by most attribution scholars, including his close colleagues Taylor and John V. Nance.

Egan then writes:

> Freebury-Jones considers the influence of *King Leir* on Shakespeare, citing a series of phrases-in-common that Mueller’s dataset tells him are unique. It is not clear what Freebury-Jones thinks this indicates.

It indicates that, in terms of quantity and quality, Shakespeare matches with *King Leir*, attributed to Kyd, are more impressive than Shakespeare matches with *Arden of Faversham*. I don’t think I can articulate this any more clearly than I do in the article: ‘The plays with the most pervasive influence on Shakespeare’s dramatic language in this table are *The Spanish Tragedy* and *King Leir*, not *Arden of Faversham*’.

Egan also writes:

> Looking for 4-grams common to *Arden of Faversham* and all other plays, Freebury-Jones reports that in Mueller’s dataset “the play with the most unique matches is Kyd's *Soliman and Perseda*, with a total of eighteen” (p. 18). Mueller puts the odds of so many shared 4-grams coming up by chance as smaller than one in 10,000. Freebury-Jones does not mention by how much *Solimon and Perseda* [sic] has “the most” matches, and it would make a great difference to the significance of his discovery if the next highest play had, say, only half as many matches. On the other hand if *Solimon and Perseda* [sic] has 18 matches and five other plays each have 17 matches, the significance of the finding is much less. Without such details, a mere rank order (“the play with the most”) does not really tell us anything and Freebury-Jones is overplaying his hand in claiming to have refuted the evidence that *Arden of Faversham* is not by Kyd.

This is a reviewer who lavishes praise on the method of ‘microattribution’ (see my response to Egan’s NYWES 2018a), proponents of which sometimes rely on just a couple of verbal matches to decide who the author of a sample is. My study provides a comparison between Shakespeare and Kyd’s dramatic corpora. Kyd’s *Soliman and Perseda* shares 18 unique
matches with *Arden of Faversham* in Mueller’s spreadsheet, whereas the top-ranked Shakespeare play, *Richard III*, shares 8. Thus, when Egan writes, ‘Freebury-Jones finds parallels between *Arden of Faversham* (outside of Act 3) and *Richard 3*, but again the problem is in the data’s presentation since to see the significance one would require comparison between plays from multiple canons and *Arden of Faversham*,’ he indicates that he doesn’t understand the work he is critiquing, including the very title of the article, ‘Kyd and Shakespeare: Authorship versus Influence’. Egan then cites the co-occurrence of a phrase with a play by William Davenant, which perhaps should have been included in Mueller’s database but has little effect on my arguments. Shakespeare is more likely to have been influenced by a phrase used by Kyd when he wrote *Richard III* than a phrase adopted by a dramatist born in 1606 and whose first play was composed over three decades later.

Using his different dataset, Egan contests another of the matches I cite, ‘in his bed’, followed by ‘He is’, because it can be found in two works ‘which ought to be in Mueller’s dataset’. Egan might very well be right. This is one of just 2 unique matches in Mueller’s corpus between *Richard III* and the portions that he and his *New Oxford Shakespeare* colleagues assign to Shakespeare in *Arden of Faversham* (whereas 6 occur in the ‘non-Shakespeare’ portions), so Egan’s elimination of it does more harm than good to Jackson’s case.

I am more than happy to concede any genuine errors pointed out by Egan and correct them in future work. Indeed, I would be very grateful if this were the case. But having scrutinized his arguments, I can only conclude that Egan’s review of my article, ‘Kyd and Shakespeare: Authorship versus Influence’, is of no value.

Unfortunately, I can’t offer praise or gratitude for his review of my article, ‘Did Shakespeare Really Co-write 2 Henry VI with Marlowe?’, either. In this article I argue that the evidence suggests that 2 *Henry VI* was written by Shakespeare alone. Egan of course is an
advocate of Marlowe’s co-authorship. His one externally peer-reviewed contribution to attribution scholarship thus far is the Word Adjacency Networks method, which apparently convinced the *New Oxford Shakespeare* team to credit Marlowe with large parts of the *Henry VI* trilogy. However, the WAN method has been shown in recent studies by Pervez Rizvi (‘Authorship Attribution for Early Modern Plays using Function Word Adjacency Networks: A Critical View’) and Rosalind Barber (‘Function Word Adjacency Networks and Early Modern Plays’) to be useless for distinguishing Shakespeare and Marlowe’s styles.

It is worth quoting Egan’s remarks on Marlowe’s use of feminine endings in full:

Freebury-Jones returns to the work of Philip Timberlake on feminine endings to verse lines, pointing out that early Shakespeare’s rate of feminine endings is much higher than the rate in Marlowe’s plays, and by Freebury-Jones’s counts the parts of 2* Henry 6* that have recently been attributed to Marlowe have feminine ending rates much too high to be his. Freebury-Jones quotes Marlowe’s mean average rate of feminine-endings of under 4% (of verse lines, by Timberlake’s “strict” count), and Shakespeare’s mean average of over 10%.

But Marlowe’s low mean conceals a wide variation in short sections, hitting 12.5% in *Doctor Faustus*, 9.6% in *The Jew of Malta*, 11.1% in *Edward II*, and 12.5% in *The Massacre at Paris*, as tabulated in the pages of Timberlake’s book that Freebury-Jones refers his reader to. The problem with short sections of plays is in deciding where to draw the boundaries: a single scene of 8 lines containing one feminine ending comes out as a 12.5% scene, and a better measure of variation across a play would be to ignore dramatic structure and use a rolling window taking in a given number of lines.

Egan has an impressive knowledge of statistics, so I find this section of his review truly alarming. In the article, I write the following: ‘In total, the scenes Hugh Craig would remove from Shakespeare (1.1-2; 2.3; 4.2-10; and 5.2) average 10.7 percent feminine endings, which is far too high for Marlowe, but commensurate with Shakespeare’s practice’. We are not talking a tiny sample of 8 lines here, as in *Doctor Faustus*; nor 31 lines as in *The Jew of Malta* scene Egan mentions; nor 18 lines as in the *Edward II* scene; nor 8 lines as in *The Massacre at Paris* scene Egan highlights (making a grand total of 65 lines). We are talking 1194 lines in *2 Henry VI* that reach a figure for feminine endings vastly higher than anything Marlowe ever attained for a comparable sample in his dramatic work. It appears that Egan is
willing to feign ignorance in order to protect an authorship theory that strikes me as most unlikely.

Egan concludes his annual diatribe on my work with the following:

Freebury-Jones shows that if we use Ants Oras’s pause-pattern measurements, the parts of 2 Henry 6 supposed to be Marlowe’s and the parts supposed to be Shakespeare’s test alike. This tells us nothing since Freebury-Jones has not established that Shakespeare and Marlowe (or anyone else for that matter) were distinctive in their use of these pause patterns. Freebury-Jones finishes by returning to those who do not accept Marlowe’s contribution to 2 Henry 6, quoting from private correspondence from Marcus Dahl and an essay by Mueller that, as is unfortunately usual in Freebury-Jones's publications, is not at the URL he provides, which points to a location within Vickers’s personal website.

Throughout the article, I demonstrate the homogeneity of portions that Egan would assign to Shakespeare and Marlowe. There is simply no internal evidence to suggest that this play is the product of two different authors’ hands. As Egan himself does not deign to establish whether ‘Shakespeare and Marlowe (or anyone else for that matter) were distinctive in their use of these pause patterns’ the section in his review on Ants Oras’s pause pattern data is unenlightening.

Finally, as for Egan’s claim that the ‘essay by Mueller that, as is unfortunately usual in Freebury-Jones’s publications, is not at the URL he provides’, readers can test the URL I provide in the article here: http://www.brianvickers.uk/wp-content/uploads/2016/05/Martin-Mueller-on-Brian-Vickers-and-the-Kyd-canon.pdf. Again, at the time of writing (December 2019) it works fine for me.

As Lady Bracknell might put it: ‘To claim that one URL does not work can be regarded as a misfortune; to claim that two URLs do not work looks like carelessness’.