A BROKEN REED?: Early Drama Records, Politics, and the Old Historicism

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The issue of the utility, even the validity, of studying the archival records of early drama has become a more urgent concern since the publication of two articles by Theresa Coletti.[1] These articles, one ('Reading REED') a general reflection upon the nature and direction of the REED project, the other ('Fragmentation and Redemption: Dramatic Records, History, and the Dream of Wholeness') an extended review of David Klausner's REED edition of the Herefordshire/Worcestershire records, do not in themselves amount to anything very significant in the historiography of early English drama. What may prove more significant, however, is the opportunity which they provide for reflection upon the state and direction of archivally-based dramatic scholarship. What I wish to do in this paper is offer a minor contribution to that reflection from a position somewhat removed from the chief interests of most *Medieval English Theatre* readers, a low moan from the haunted East Wing of that mansion of many rooms which is early drama studies, where lie the dusty chambers in which political history and dramatic history occasionally slip into bed together and wonder whether they have enough in common to start going steady. What I propose to do is to examine briefly a number of the claims which Coletti makes in the course of her critique of REED before moving on to consider the broader implications which this debate might have for the future of dramatic historiography, and of historiography more generally, both within and without the REED project, finishing with a brief example of how, I think, the two disciplines might more fruitfully interact.

There are two broad thrusts to Coletti's critical assessment of the REED project. First there is criticism of the strategic and tactical editorial decisions made by the REED editors, and of the consequences of those decisions for the value of the material gathered and published: criticism concerned largely with the problems suggested by the first term in the title of her review article: fragmentation. Then there is a wider critique of the whole archival-historical enterprise which REED represents, and an attempt to point out its alleged limitations in the light of the theoretical approaches to literary history practised by the New Historicism and post-modern cultural studies: a critique based upon what Coletti perceives as an attempt implicit in the REED project to redeem a comprehensive, whole, and true archive of dramatic records from the scattered sources at its disposal. As this second level of criticism seems to have less to recommend it I will look at it first and more briefly before moving on to the more important questions suggested in the remarks about editorial practice.

In concluding her account of what she sees as the limitations of the 'Old Historicism' Coletti sketches a model of the recent past and implied future of medieval studies:[2]

> With increasing frequency scholars of medieval literature and history are analysing the impact of post-modern theory on the fundamental historical dimension of medieval studies and, in the process, substantially revising the discipline's prior understanding of such things as historical objectivity, the independent status of documents, the neutral quality of evidence. As a result of this revision, which has come lately to medieval studies, medievalists have begun to approach their work from positions that accept their own historical contingency, the textualized nature of historical data, and the hermeneutic construction of evidence. They have begun to articulate the provisional nature of our knowledge of the medieval past.

But all this begs too many questions for comfort. The model of the progress of historical scholarship which Coletti offers is, of course, a travesty. It surely has not taken 'post-modern theory' to prompt historians to an awareness of the provisional status of their knowledge of the past, the slipperiness of archival evidence, or the crucial role of the historian in both constructing and interpreting the archival evidence upon which their accounts are based. Medievalists of all people do not need help from 'theory' to understand the patchy, inconsistent, and anything but neutral nature of the fragmentary evidence they study. It may be that literary scholars have only slowly come to an appreciation of the way historical evidence, and hence historical enquiry, works, (although this hardly seems true of the medievalists I know) but it is as patronising as it is naïve for Coletti to displace the lateness of that appreciation onto the historians themselves, who have long understood and superseded the 'discoveries' which she seeks to reinvent. Despite her apparent assumptions to the contrary, history does not think of itself as a science, nor, I believe, has it done so (except in a few of its more statistical branches) for the better part of a century. So accusing REED of so doing seems an unlikely basis for a penetrating critique of its practices.

That history is a tricky business, that evidence is never neutral, that the past can never be understood in its totality, that no historical account can offer the Truth, or even the last word on a subject, that all reports from the archives are necessarily provisional, and that historiography is as much persuasive advocacy as it is objective analysis: these are the bedrock of the historical discipline. Any student in the archives is made aware of them by the very nature of the enterprise they undertake. Such observations hardly render up the head of the Old Historicism on a plate to post-modern cultural theory. REED's dream of wholeness would thus seem to be a delusion of Coletti's own devising, a misreading, as Peter Greenfield has pointed out, of the desire for as exhaustive as possible an account of the available records to enable future scholarship to build upon the soundest available archival base.[3] It would thus seem premature to abandon the fundamental tenets of historical research in favour of New Historicist theorising until we are quite sure that we all know what we are talking about.

Where Coletti raises more valid objections, however, is over the editorial decisions which REED inevitably has to make in creating its records. In choosing to include certain records because they refer directly to secular drama, music, or ceremony, and excluding others because they do not (even though they may illuminate the cultural context in which such activities took place) inevitably means that the story created by the *REED* volumes is a partial and potentially misleading one. There is a valid selectivity at work here: REED's aim is to collect specifically *dramatic* records after all. But if, as REED continually avows, its volumes are intended to be of value to social and cultural historians as well as scholars of drama, then the removal of the wider context may well prove a disabling limitation.

If we consider a recent volume, J.A.B. Somerset's *Shropshire*, the problems become clear.[4] There is an inevitable intellectual parochialism to the selection of many of the entries (of the 'KING BEHEADED: LOCAL ACTOR PRESENT' variety) which is a little disturbing. A case in point is the story taken from Ludlow in 1622–23, involving the theft of a purse during bear-baiting at Bridgnorth. The depositions of witnesses and suspects are recorded, and footnotes help us to a better understanding of the local topography. But quite what the case adds to our understanding of bear-baiting, beyond the rather obvious suggestion that it might provide an occasion at which purses might be stolen is less easy to determine. What we are presented with is simply an account of a petty theft which just happened to take place at a bear-baiting; it adds little to our knowledge of bears, their baiting, or crime. Stripped of its archival context it becomes simply a story.

It is not possible to answer any of the questions which might give it significance as a record of social history. Were crimes of this sort more common at bear-baitings than at other outdoor events or less common? And, if more common, was periodic official hostility to such events indeed partly motivated by their capacity to provoke lawlessness? Or was it part of a general antipathy to popular entertainments based upon more aesthetic, bureaucratic, or moral grounds? Was this particular crime evidence of local criminals preying upon wealthy visitors, or transient criminals preying upon wealthy locals? Were the majority of such local crimes committed by 'professional criminals' or by otherwise uncriminalised individuals driven to deviant behaviour by poverty or desperation? We cannot know if we cannot compare this particular record with the generality of entries in the document from which it came.

Such objections are, in small, a reflection of wider difficulties with the *REED* volumes as historical source materials. In extracting material from the archives one is inevitably cutting it off from much of the collateral evidence which helps to place it in its cultural context. The exclusion of the evidence of religious ceremonies and music, and important parts of the documentary evidence associated with the feast of Corpus Christi is, as Coletti points out, a serious limitation upon the value of the York volumes as source material for the cultural history of that city.[5] But more mundane objections might be made for other exclusions too. By listing the payments made only to minstrels and players from household or municipal account books, for example, it is not possible to appreciate the status and relative importance of such payments (and hence such entertainments) to the towns or households which they visited. When a troupe was paid 20s for a performance, was this a generous payment, or a meagre one? What proportion of the ready resources were being expended upon such things? Were, for example, visiting entertainers costing a nobleman more than visiting painters, clerics, or lawyers, or less? Was he spending more or less on his revels than on his hawks? Was a town more concerned with regulating its visiting players or its indigenous rats?

On a separate but related issue: we can see in the *REED* volumes evidence of individuals and crowds flocking to dances and interludes rather than attending divine services, but did the same townsfolk also flock to hear the preaching of a visiting friar or post-Reformation sermoniser rather than attend regular services, and did this arouse similar concern among the parish clergy? And did they also skip sermons to attend guild meetings, and vice-versa? The bald nature of the records calendared by REED implies a

Bahktinian binary opposition — a conflict — between popular ludic misrule on the one hand, and sober clericist authority on the other: church or play, church or dancing? A wider view of the archival records might present a rather more pluralist culture in which religion and popular culture interacted in rather less predictable, oppositional, ways.

In other words, we can gain glimpses of a rich and varied world of dramatic activity from these records. But what did it all mean? What was its contribution to the wider cultural history which contained it? And, by implication, what is the contribution of the historiography of drama to the wider historiography of culture around it?

There is inherent in the very idea of being an historian of drama an implied danger that one will become ghettoised, excluded from the mainstream of cultural history by a mixture of self-denying ordinance (our interests are parochially dramatic, wider issues are of concern only when they relate directly to dramatic entertainment) and the indifference — even the ludophobia — of other historians who, because we have not made the case strongly enough that drama is an important element in cultural history, feel able safely to ignore it. We, like the REED project (in its first phase at least) risk becoming merely antiquarian hoarders, the hunter-gatherers of the historical world, amassing material of interest to us, and assessing, collating, and publishing it in a form which is useful to us, rather than immediately using that material in ways which draw out the vital interconnections between dramatic activity and political, social, and religious activity.

In the area of religious history — and especially in the study of the practices, beliefs, and institutions associated with the festival of Corpus Christi, these barriers — this divide — no longer prevents fruitful conversations taking place.[6] But in my own field, political history, the case has still to be made that drama records have much to tell us about political culture. Since the pioneering work of David Bevington in the 1960s, it has been clear that there is a political dimension to early dramatic activity which is in urgent need of exploration.[7] But that fact is, in itself, of little interest to the mainstream political historian. The drama scholar is still left in the position of parasite in this relationship, the intellectual debtor, learning from the study of politics the information which can shed light upon the dramatic texts or records, but offering little in return. The crucial step is to move from the demonstration that drama might be *about* political activity to the demonstration that drama itself is, in many cases, a political activity, and that this observation has considerable consequences for the study and appreciation of both drama and politics, not just in the theorised generalities favoured by the literary New Historicism, but in terms which historians will recognise and find useful. Hence it is vital that the noble self-denying ordinance enshrined in the informal REED charter that one first gathers the data and then — and only then — seeks to analyse it, continues to be abrogated with greater and greater regularity by editors and readers alike, to enable a continuous dialogue between records and analysis to develop. Work such as Marie Axton's on the Elizabethan succession debates and Gordon Kipling's on ceremonies and royal receptions has continued the process, but much more could be done.[8] When records are uncovered which have a wider significance for political and cultural history it is important that this significance be drawn out in articles in mainstream journals and papers at mainstream conferences, not limited solely to in-house publications and dedicated gatherings of specialists only.

Work such as John McGavin's exposition of a case of 'rough music' at the court of Robert III of Scotland, in which he traces the attempt by the granger of Scone Abbey to seek redress for damage done to crops by guests at the coronation, serves to place the activities of a REED editor at the centre rather than the margins of debate about political culture.[9] It shows how a reading of a single, given, example of popular ritual might reveal much, not simply about popular culture and agrarian folklore, but about court culture, the nature and exercise of late medieval kingship, and of the delicate negotiations and interpenetrations between the élite and popular spheres. Such work spans the historical disciplines and demonstrates compellingly the value of work on drama records to historians of politics and culture alike.

A further final example of how the histories of drama and politics might interact productively can be seen in the discovery of a documentary account of a performance of Sackville and Norton's infamous tragedy *Gorboduc*.[10]

Among the Yelverton papers in the British Library is a collection of the papers of Robert Beale, the Elizabethan courtier and administrator. Among these are what appears to be the working notes for a chronicle, concentrating in part upon matters associated with Robert Dudley.[11] And it is here that one can find an eye-witness account of *Gorboduc* as performed in 1561–2.

Scholars have long realised that the composite entertainment of *Gorboduc*, and an attendant mask of *Beauty and Desire*, offered a direct intervention in the political controversy surrounding Elizabeth I's marriage plans (or lack of them) and the uncertainty of the succession. But now with this document it is possible to be more definite about the specific political context of the play, and its reception by the courtiers, administrators, and lawyers who made up its first audiences.

> Ther was a Tragedie played in the Inner Temple of the two brethren Porrex and Ferrex K[ings] of Brytayne betwene whome the father had devyded the Realme, the one slewe the other and the mother slewe the manquil[e]r [ie. the man-queller or man-killer]. It was thus used. Firste wilde men cam[e] in and woulde have broken a whole fagott, but could not, the stickes they brake being severed. Then cam[e] in a king to whome was geven a clere glasse, and a golden cupp of golde covered, full of poyson, the glasse he caste under his fote and brake hyt, the poyson he drank of, after cam[e] in mourners.[12] The shadowes were declared by the Chore[us] first to signyfie unytie, the 2 [ie. second] howe that men refused the certen and toocke the uncerten, wherby was ment that yt was better for the Quene to marye with the L[ord] R[obert] knowen then with the K[ing] of Sweden. The thryde to declare that cyvill discention bredeth mo[u]rning. Many thinges were handled of mariage, and that the matter was to be debated in p[ar]liament, because yt was much banding but th[at] hit ought to be determined by the councell. Ther was also declared howe a straunge duke seying the realme at dyvysion, would have taken upon him the crowne, but the people would none of hytt. And many thinges were saied for the succession to put thinges in certenty. This play was the [blank] daye of January at the courte before the Quene, where none ambassadors were present but the Spanyshe.

Now clearly this is a significant document in the history of drama in the Inns of Court and the Royal Household. A number of the observations it contains supplement and in some cases alter our perceptions of *Gorboduc*. But what it is crucial to note in the present context is that it is also an important document for political and diplomatic history too, and a powerful reminder of the interconnections between the two kinds of historiography.

Whereas previous accounts of the play have placed it in the general context of the succession debate,[13] this account makes clear the precise moment and nature of that intervention, and does so in ways the printed versions of the text would not lead us to expect. The crucial issue for this witness was clearly whether the Queen should marry Robert Dudley or the King of Sweden. The latter's candidature has largely been dismissed by historians as hopeless, but clearly it was for a time a live and disturbing matter in court and legal circles (and, once alerted to it, one can find a lot of documentary interest in it at the time). According to the printed text, the play's second dumbshow, in which a king is offered a glass full of wholesome drink and a golden goblet full of poison, was intended as an allegorical warning against heading the seductive lures of flattery instead of the clear honest counsel of truth. This new document suggests that it was read very differently by its first audience: as a warning against the unknown perils of a seemingly attractive foreign marriage. Indeed this account suggests strongly that the play as performed was very different from the text as printed — for the 'many things' that were handled of marriage, and the Parliamentary debates concerning it, are not to be found in the printed version.

More generally, what this new evidence demonstrates, of course, is the close relationship between drama and politics in this period. It proves that *Gorboduc was* read by its first audience as a direct commentary upon, and intervention in, contemporary political debates: and not just in general terms but in the specific context of the Swedish suit for Elizabeth's hand. Drama and politics did not inhabit separate spheres of operation at this time. When they wished to make a political point and influence policy on an issue as important as royal marriage and the succession, political figures of the stature of Norton and Sackville chose to do so, not in Parliament only, but through a play as well. Similarly it shows that contemporary educated audiences were accustomed to reading the most direct political relevance into dramatic representations, even where the subject matter did not immediately suggest it.

The play was, then, in both its acted and its printed form, a political document. Its performance in the Inner Temple and at court, and its subsequent publication in amended form in print, were political acts intended to influence politicians and affect royal policy. This documentary account of its performance, recorded in a political chronicle with equal — and indeed greater — care and detail to that afforded to Parliamentary proceedings and encounters at court, serves to confirm and extend our appreciation of those facts. Such a record thus spans the histories of drama, politics, and culture, contributing to each with equal weight. It, like so much of the material examined and collected by REED editors and other historians of drama, deserves a wider hearing.

It is to be hoped, then, that in reacting to the criticisms voiced by Theresa Coletti and others, historians of drama do not simply retreat further into self-examination or mutually supportive seclusion within their own specialism, but seek to locate the questions that they ask and the issues which they address within the wider debates of historical studies generally, whether of the older or the newer varieties, for our voices can have influence there.

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NOTES

1. Theresa Coletti 'Reading REED: History and the Records of Early English Drama' in *Literary Practice and Social Change in Britain, 1380–1530* edited Lee Patterson (University of California Press, Berkeley, 1990) 248–84; and '"Fragmentation and Redemption": Dramatic Records, History and the Dream of Wholeness' *Envoi 3* (1991) 1–13.
2. Coletti 'Fragmentation' 11.
3. Peter H. Greenfield '"But Herefordshire for a Morris-daunce": Dramatic Records and the New Historicism' *Envoi 3* (1991) 14–23; 15.
4. *Records of Early English Drama: Shropshire*, 2 volumes, edited J.A.B. Somerset (Toronto University Press, 1995). For further exposition of this case, see my review in *Midland History 20* (1995) 175–77.
5. *Records of Early English Drama: York*, 2 volumes, edited A.F. Johnston and M. Rogerson (University of Toronto Press, 1979); Coletti, 'Reading REED' 272–73.
6. See, for example, Miri Rubin *Corpus Christi: The Eucharist in Late Medieval Culture* (Cambridge University Press, 1991).
7. David Bevington *Tudor Drama and Politics: A Critical Approach to Topical Meaning* (Harvard University Press, Cambridge Mass., 1968).
8. M. Axton *The Queen's Two Bodies: Drama and the Elizabethan Succession* (Royal Historical Society, London, 1977); *The Receyt of the Ladie Kateryne* edited G. Kipling *EETS 296* (1990).
9. John J. McGavin 'Robert III's "Rough Music": Charivari and Diplomacy in a Medieval Scottish Court' *Scottish Historical Review 74* (1995) 144–58.
10. For a fuller exposition of the nature, context, and significance of this document, see Henry James and Greg Walker 'The Politics of *Gorboduc*' *English Historical Review 110* (1995) 109–21.
11. BL Additional MS 48023, fol 359^{v}.
12. I am grateful to Dr John R. Elliott of Syracuse University for palaeographical advice on this word.
13. See, for example, Axton *Queen's Two Bodies* 38–47, M. Levine *The Early Elizabethan Succession Question* (Stanford University Press, 1966) 30–44, L.H. Courtney 'The Tragedy of "Ferrex and Porrex"', *Notes and Queries, 2nd series, 10* (1860) 261–63. An alternative suggestion, that the play should be read in the context of the dispute between the Inner and Middle Temples was advanced in D.S. Bland *Three Revels from the Inns of Court* (Avebury Publishing, Amersham, 1984) 21–22. Dudley's connections with the Inner Temple, and his alleged role in the settlement of this dispute, chiefly concerning control over three Inns of Chancery, constitute an interesting story. For further details, including new

material drawn from the chronicle in BL Additional MS 48023, see Greg Walker *Early Renaissance Drama: The Politics of Performance* (Cambridge University Press, forthcoming).

2) Greg Walker is a seven-time multi-category semi finalist and finalist in the prestigious UK Songwriting Contest, including second place in the Jazz/Blues category of the 2002. His first album, Straight From The Source 1 gained success in the independent artists download charts at mp3tunes.com, where it held the No.1 position simultaneously in the overall Jazz chart, the Latin Jazz chart and the Brazilian Jazz chart - in the latter, almost without break for several months. Strâ€¦ read more. 5 Stars - Finest of Greg Walker, Vol. 6. Greg Walker. 2017. 20 Stars - Finest of Greg Walker. Greg Walker. 2017. 5 Stars - Finest of Greg Walker, Vol. 5. Greg Walker. 2017. 5 Stars - Finest of Greg Walker, Vol. 4. Greg Walker. 2017. 5 Stars - Finest of Greg Walker, Vol. 3. Greg Walker. 2017. 5 Stars - Finest Of Greg Walker, Vol. 2. Greg Walker. 2016. 5 Stars -Finest Of Greg Walker, Vol. 1. Greg Walker. 2016. CaffÃ¨ Gio. Greg Walker. 2016. Mosaic. Greg Walker. 2015. End of Summer. Greg Walker. 2012. A Joyful Noise. Greg Walker personifies a renaissance in individual artistic expression in his music. Gregâ€™s CD, Never Better, showcases his talents as a singer, songwriter, producer. ON his album, Sharing Christmas, Greg paints the ultimate holiday portrait with warm, visual, rhythmic grooves to touch your soul...the soundtrack of Christmas. The CDs are available at cdbaby.com/all/gwmusic. Gregory or Greg Walker may refer to: Greg Walker (academic), professor of rhetoric and English literature at the University of Edinburgh. Greg Walker (baseball) (born 1959), former first baseman. Greg Walker (footballer) (born 1967), former Australian rules footballer. Greg Walker (rower) (born 1964), American Olympic rower. Gregory T.S. Walker (born 1961), American composer, violinist, and guitarist. Gregory Walker (music), harmonic formula in the Renaissance period.