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Not the Last Word

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Citation	Simpson, James. 2005. Not the last word. <i>Journal of Medieval and Early Modern Studies</i> 35(1): 111-120.
Published Version	doi:10.1215/10829636-35-1-111
Accessed	February 17, 2015 3:30:51 PM EST
Citable Link	http://nrs.harvard.edu/urn-3:HUL.InstRepos:2664290
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"Not the Last Word"

When the editors of *JMEMS* suggested a number dedicated to *Reform and Cultural Revolution*, I was naturally delighted. I remain deeply grateful to David Aers and Sarah Beckwith for their generosity in opening these prestigious pages to consideration of the book. I'm no less grateful to the reviewers for their generous (sometimes exceptionally generous) and unfailingly serious readings. I confess, however, that, on reading these review essays, there were moments when I felt a little like King Edmund after the Danes had finished with him. The midge-swarms of "discursives" stung badly.

Before reading the review essays, my instinct was not to reply at length: the book is already long, and I'd had my say. Point by point reply would indeed strain the patience of readers, but some response to the larger issues might nonetheless be productive.

Allow me to begin by taking a step back into the book's genesis. In planning it I went to my College library to consult those volumes in the prior Oxford History of English Literature series whose dates overlapped with mine. What most struck me was this: one of the volumes had never been borrowed from the library.

It's true that, by 1993, it had been there for only forty-six years; there was always a chance that it was going to be borrowed some day, but the odds on that had to be lengthening by the day. I determined then to write a form of literary history that people would want to read. These are the motives that drove that enterprise:

- (i) to question the periodic persuasions by which late medieval texts are habitually read, by setting those texts into dialogue with (early) Early Modernity. The distinction between "reformist" and "revolutionary" cultural practices emerged as a way of putting pressure on triumphalist accounts of sixteenth century developments;
- (ii) to craft a version of literary history that would connect with cultural history via formal categories. Consideration of mode and genre, for example, could be read off as a way of imagining jurisdictional freedoms;
- (iii) to devise a way of bringing the whole of later medieval English writing vitally into the story, given that too much of it had been seen merely as a foil to set off Chaucer's incontestable brilliance. Lydgate played a significant role in that strategy;
- (iv) to reconceive specific areas of Middle English literary historiography: the single, hopelessly elastic category of "romance," for example, seemed

to me to embrace texts that were more productively considered separately as elegy, romance and tragedy; and

- (v) to promote reflection on the historiography of late medieval and early modern English literature. Almost each chapter contains mini-histories of its subject. These underscore the point that memory and scholarship have themselves been conditioned by moments of cultural revolution. Scholarship is, then, implicitly a part of the book's subject.

The review essays here focus principally on (i). Some of them say I should, or could, have included more within the categories proposed. Suggestions of that kind are music to my ears. Reviewers often approach books in the way authors do before actually writing: they conceive of books as ideal forms. And so they should. I entirely agree with Bruce Holsinger that my story of post 1350 "freedoms" challenges us to look to the cultural politics of the reigns of Edwards I, II and III. And of course I'm very happy to see Tom Betteridge reading Elizabethan texts within my categories. David Wallace productively suggests ways of reading English reception of Continental texts within the terms I propose. These are all welcome suggestions. The literary history of two hundred years turned out, however, to be very much larger than I'd

naively supposed it would be on setting out. Derek Pearsall, who after all knows about such things, thankfully mentions "the real problems in writing a literary history." One can't do everything, especially outside one's set boundaries.

In a second response to the terms of (i), some reviewers accept the categories of "reformist" and "revolutionary," but point to shortcomings in my handling of the categories. Having praised my account of a "reformist" literary culture, Bruce Holsinger faults me for under-reading the "revolutionary" texts. That, with respect, is an under-reading of the book. We need to distinguish between the unbending constraints on "revolutionary" texts (especially their commitment to the exiguous and punishing confines of the literal sense) and the actual currents of the texts themselves. Those currents are very bent indeed. David Wallace, happily, recognises that my evident sympathies for late medieval heterogeneity do not lead me to "downplay the intrinsic interest or fascination of work from the later period." That fascination seems to me everywhere apparent, as I read, for example, Bale's texts very much against their grain. I see in Bale (in a passage cited by Holsinger as "smartly counterintuitive") the "profoundly divided sensibility of the revolutionary thinker, undoing the new order of which he is a champion" (p. 19). To my mind,

this deconstructive reading aptly characterises my fundamental posture towards many (perhaps, I concede, not all) of the "revolutionary" texts with which I deal. It certainly applies to my account of Bale as bibliographer and as dramatist. It also applies to my readings of *Utopia*, of Tyndale's hermeneutics, and of the poetry of both Wyatt and Surrey, for example. Less than "persistent unwillingness" to read against the grain, this is evidence, rather, of persistent inability to resist doing precisely that. Holsinger himself wants to read Lollard texts against their grain. I concede that I have been able to resist that temptation, provisionally persuaded as I am that Lollard claims to exclusivist community formation produce exclusivist formal practices. I look very much forward to Holsinger's readings, as I admire the first taste of it given here.

Betteridge, too, is unhappy with my characterisations of the "revolutionary," but in a different way. He welcomes my categories, but would have it that the reign of Henry VIII is the repressive exception to an otherwise decentralised early modernity. The editors also ask about how the book's thesis will look when extrapolated beyond the mid-sixteenth century. I reply to both points thus: the book, whose term is 1547, isn't itself answerable to such questions. It is nevertheless *a-priori* unlikely that positions formulated

with regard to a moment of cultural revolution will hold steady for long. Such moments characteristically try to hold a very rigid line before confronting the sheer impossibility of containing historical energies within such tight bounds. That said, the ideological formulations of cultural revolutions have a way of surviving in interesting forms. Betteridge's position would have been yet stronger had he addressed the relentlessly centralising tendencies of, say, an evangelical theology of grace, which certainly survived the reign of Henry VIII.

A third response (by Pearsall) to the terms of (i) was to square my book with Eamon Duffy's *Stripping of the Altars*, in proposing 150 years of stasis followed by twenty years of moving history. The editors, too, refer to the book as "almost a secularized mirror image of Eamon Duffy's book." Despite my admiration for aspects of Duffy's book, I strenuously oppose the strict parallel with mine. While it's true that, by definition, change in a "reformist" culture occurs in smaller gradations, it is not true that I flatten any sense of micro-cultures within the period prior to the Act of Supremacy. Literary conditions in the reigns of Richard II, Henries IV and V, Henry VI, and Henry VII are delineated in, respectively, Chapters 4 (The Elegiac), 5 (The Political), 8 (Moving Pictures), and 6 (The Comic). These differing climates

are not treated in chronological order (the book's structure disallowed that), but they are treated. And while I think the effects of Arundel's "Constitutions" have been exaggerated in scholarship over the last decade, they are amply treated in the book, as a quick glance at the Index will confirm.

Neither do I see why my book should be called a "secularized" mirror image, when religion, as the editors note, plays every bit as powerful a role as secular practice. And within both areas, I describe heterogeneous, competitive medieval cultures very unlike Duffy's. The spirituality and social imagination of the mystery plays and of *Piers Plowman* in particular dominate my account of what was most dynamic in pre-Reformation religious literary culture. Neither figure significantly in *The Stripping of the Altars*.

Since the sixteenth century two paradigmatic postures have been taken with regard to pre-Reformation culture: either it's rejected, or else it's the focus of nostalgia. Duffy's book is an expression of the second of those positions (nostalgia), while many of the attacks on Duffy were reflexes of the first. Both Duffy and some of his detractors are replaying a five hundred year agon. My tactic in that agon was to draw early modernists into the medieval field and vice versa, rather than replaying the fight from entrenched lines. The structure of *Reform and*

Cultural Revolution, unlike that of *The Stripping of the Altars*, constantly traverses the medieval/Early Modern line.

A final objection to the terms of (i) was that I treated "reformist" and "revolutionary" practice in too consecutive a way, first one then the other. Richard Emmerson makes this charge in his discussion of Chapter 10. I agree that the book lacks enough detailed discussion of the complicated and often-tragic manoeuvres by which a reformist culture engages with a revolutionary culture. The Biblical (Chapter 9) does that to a certain extent in dealing with More's address to Tyndale, but the interactions of Lollardy and "orthodoxy" remain under-explored. That said, I'm surprised that Emmerson should choose Chapter 10 to lay this charge, since that is precisely the chapter in which I do acknowledge the punishing disciplines of some branches of pre-Reformation drama (the *Croxton Play of the Sacrament*, for example). I also refuse to make the amateur/professional distinction a watertight periodic distinction in theatre history.

The essays pay some attention to (ii), though only, in my view, do the editors and David Wallace accurately explicate what I was up to here. The editors point to the book's modal categories. They recognize in them an implicit critique of tightly-defined post Reformation generic categories as too limited for the late medieval

objects to which they are habitually applied. Wallace recognises in his first paragraph that I write cultural history as a literary critic, not as a historian *manqué*. That is precisely what I aimed to do. What I avoided doing was to practise a nominalistic literary criticism on works as worlds unto themselves, without reference to more abstract, cultural categories with which formalist literary terms could resonate. I have no objection to more purely formalist analysis, but our discipline must recognise a grave problem with that, which is that it's stopped selling. Publishers are no longer interested. While we might continue to focus in class on the specifically literary as the locus of most intense illumination, we need to invent ways of writing literary history that will engage people who have not read the texts. Above all, I needed to avoid the intellectually inert formula of the literary inventory seasoned with bellestristic comment that characterised literary history for much of the twentieth century.

Derek Pearsall pays most sustained attention to (iii), arguing that I left questions of literary value bobbing in the wake of my literary historical argument. The previous paragraph begins to answer this charge. A fuller answer would be to concede, instantly, the greater vitality of Chaucer's verse at the level of diction, syntax and structure than that of any of his

contemporaries or followers. One would equally concede the importance of such a discrimination. From there on the defence of my procedure would go as follows: I was writing literary history rather than literary criticism in itself, and crafting categories within which focus on Chaucer's poetry would not simply and immediately extinguish interest in that of other late medieval English poets. Further, I defined vectors within the historiography of Middle English studies that prompted us to keep focussing on Chaucer's exceptional superiority and only on that. Besides, talking of what's interesting in writers brings out the best in them and, I contend, in us. Nothing looks more pompously condescending to my eyes than the discussion of a poet's "faults" that can be found in most earlier literary history. Why bother with faults, when they'll be obvious to any reader? I don't argue Lydgate's apotheosis, but I would argue that his apocolocytosis, or "pumpkinification" (to use Seneca's term) gets us very close to nowhere.

Some reference was made to (iv), though the review essays do not engage with the redistribution of works that are, in Middle English literary history, routinely treated as "romances." Derek Pearsall says that after *Reform and Cultural Revolution* two centuries of literary history look like a demolition site. There are strains of destructive energy in the book, but (as in the case of

romances) I also began the process of reconstruction where demolition had taken place.

There is no discussion of (v) in any of these responses.

There remain some significant criticisms unrelated to the book's aims as defined above. Derek Pearsall argues that I misread a Lydgate's *Danse Machabré* in an "extraordinary" way. I have re-read the *Danse Machabré*, and hold to my account of it as "minimally religious," or, at least, minimally soteriological. Certainly the Carthusian monk's final soteriological admonition sets the inadequacy of all other responses to Death into relief, but that in itself underlines my argument: the very many other responses to Death do not fall back on God's grace. Instead they stress regret for the transience of *worldly* powers in the face of Death. Literary criticism focuses on proportions, not merely on a poem's stated "message," and the weight of emotion in this poem is on the side of regret for worldly powers. Pearsall also takes issue with my account of Lydgate's *romans antiques* as oppositional, by saying that they offer the commonplaces of *Mirror of Princes* moralising that England's military rulers "were likely to find acceptable." This too underlines my point: if England's military leaders did find this representation of outright rebuke of military enterprise acceptable, then we simply

have to take account of a wider range of discursive possibilities in this period than anything we find in Henrician writing. New Historicist accounts of Power (themselves generated from reflection on sixteenth-century texts) turn out not to apply well here.

The wider range of discursive possibility also pertains to the dating issues. We do not need to be sure of what turns a war took to feel certain that Lydgate could or could not say something about it: it was clearly acceptable to say unpalatable things at any point. If, for example, the *Thebes* was written before 1422, then it was astonishingly prophetic about the dark future that awaited a brilliant present; if it was written after 1422, then it is less prophetic but more forceful in its warnings of civil war between Henry V's surviving brothers. The central point is that it (and *The Troy Book*) is savagely critical of militarists (even morally impressive ones) who go to war against prudential advice.

David Wallace reproaches the lack of sustained comparativist treatment of continental vernacular works, when so much attention was given to Ovid and Virgil. Let me say at once that the authors and General Editor of the series took a communal, if drawn-out, decision to treat insular and English literature as our primary focus, largely on pragmatic grounds; I wasn't unwillingly straitjacketed by that decision. It's also true that my

diachronic insular theme created a kind of slipstream in which lateral comparisons were difficult to work into the book's economy and word-limit. I'd argue, furthermore, that the *primary* profile of Ovid especially needed raising in Middle English literary history, before Wallace's excellent points about the mediations of Ovidian texts by vernacular authors could be discussed. All that said, I accept this criticism of the book, and look forward to studies that build on and challenge mine from this perspective.

Perhaps the severest criticisms come from Richard Emmerson's sustained, learned and thoughtful response. Here too I accept a shortcoming: as a relative newcomer to drama scholarship, I had missed studies that questioned the Wakefield/Towneley connection. What I do not accept is the following description of my own argument: that the amateur/professional distinction is an exclusively medieval/Early Modern distinction. That is indeed the basic shape of my chapter, but I go onto complicate it in both directions in, respectively, pp. 536-9, and Section VII. Neither do I accept that sixteenth century legislation was not concerned with the printing of plays: proclamations promulgated in 1542, 1551, 1553, each cited on p. 540, explicitly cite the printing of plays. Neither do I accept that I cite Gardiner's arguments (about central participation in

efforts to close down York, Wakefield and Chester in the 1560s and 70s) with approval simply because they suit my argument, without careful examination of the evidence. I was referring to Gardiner's evidence that Dean Hutton, a member of Her Majesty's Commission for Ecclesiastical Causes in the North, intervened decisively in the deliberations of the York town corporation not to have either Creed play or cycle performed in 1568. This was the last year before a reduced cycle was performed, for the last time.¹ Gardiner also cites the letter sent in May 1576 from the Diocesan Court of High Commission, banning "a play commonly called Corpus Christi" in Wakefield;² and he gives a documented account of the role of the Privy Council in banning the cycle in Chester.³ Local concerns may indeed have been involved in the closing of the cycles, evidence for which I cite (p. 536), but I remain persuaded of Gardiner's evidence for forms of active, centralised discouragement of the cycles. In short, I'm grateful for Emmerson's well-documented *aggiornamento* of some areas of scholarship, but unpersuaded that he has everywhere characterised my argument accurately, or that each of his empirical challenges carries conviction.

A final set of trenchant questions, posed by the editors, concerns my account of "freedoms." If "literary history is ancillary to the complex history of freedoms," can one write such history without reference to "peasant

and artisan communities"? One certainly cannot write a history of freedoms without such reference; I was writing literary history, and in the book's Introduction (p. 5) I signalled the modesty that a literary historian should exercise. "Literature" occupies a small proportion of the discursive landscape, and the discursive landscape is itself a small corner of the cultural landscape in a largely illiterate society. I should have made more reference to the English Uprising's aggressive treatment of certain forms of textuality, but almost all chapters (notably the Comic) offer sustained discussion of the ways in which non-aristocratic cultural models exert pressure on aristocratic cultural norms.

Even more trenchantly, "is centralisation always a bad thing?" I took it as axiomatic that the following pose challenges to triumphalist accounts of sixteenth-century modernity: the demolition of balances of power; the creation of historical vacuums; the belittling of human initiatives. On the other hand, European and American history teems with examples of vicious particularisms, some of them currently on the rise in a post-imperial age. Dante had good reason to write his *Monarchia* in a war-riven central Italy. This is a very big question, in response to which most of us will be divided in complex ways.

In the book's penultimate sentence I remark that the

book's case is "properly transitional." Not two years after its publication, I am fascinated to see so much work, most of it clearly begun before the publication of *Reform and Cultural Revolution*, that is effecting this transformation of Middle and Early Modern English studies, including brilliant work on many fifteenth century writers, on the medieval/Early Modern dialogue, and on the historiography of the discipline. The "period" is in visible ebullition, not least in the way it redefines its periodicity. David Wallace kindly offers me the last word; the good news is that my word isn't the last, and that the discussion has hardly begun.

¹ Harold C. Gardiner, *Mysteries' End* (New Haven: Yale University Press, 1946), pp. 72-74.

² Gardiner, *Mysteries' End*, pp. 77-8.

³ Gardiner, *Mysteries' End*, pp. 80-83.