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The Ilchester Prologue, the Penultimate Passus of Corpus 201 (F),
and a New School of Piers Plowman Textual Studies

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In 1906 John Matthews Manly notoriously argued that a leaf was lost from the ancestor of all extant manuscripts of Piers Plowman A, a loss that “accounts for the unaccountable omission of the confession of Wrath and for the abruptness of the end of the confession of Envy.” To be specific, it was a “lost double leaf” or bifolium, “the precise number of lines to the page” of which was determinable as thirty-one: 122 lines total, plus spaces between the confessions of Covetousness and Gluttony, and between those of Gluttony and Sloth.¹ This fanciful argument provided the basis for Manly’s belief, enormously influential in the first half of the twentieth century, that A and B were written by different poets. Even if a few holdouts still maintain the plausibility of his multiple author theory, no one takes the “lost leaf” proposal seriously anymore.

Nevertheless, in late 2016 Ralph Hanna published an essay called “Loose Leaves, Lost Leaves, and the Text of Piers Plowman,” which opened by quipping that Manly was the “only begetter” of a “critical strain” in which the concept of “leaves mishandled in or lost from various archetypes/exemplars of Piers Plowman have played a disruptive role[.]”² The scholars who adopt this approach believe “that they are in touch with and recognize a poet who should not have perpetrated such a thing” as discontinuity in the poem; “the shifting

¹ John Matthews Manly, “The Lost Leaf of ‘Piers the Plowman,’” Modern Philology, 3 (1906), 365, 364.

² Ralph Hanna, “Loose Leaves, Lost Leaves, and the Text of Piers Plowman,” Journal of the Early Book Society, 19 (2016), 187.

arguments of B Version passus 15,” on which the Athlone editors George Kane and E. Talbot Donaldson, Robert Adams, and I have written extensively, “...have proved an irresistible site for such interventions.”³ The other critic cited as having succumbed to this temptation is Wendy Scase—the essays by her and by me “are particularly challenged, through the authors’ inability to distinguish readings that might be ascribed to an author and those produced by scribes[.]”⁴ Hanna had taken both Scase and me to task in earlier publications, though without bringing the irresistible Manly into the picture on those occasions.⁵ But none of these assaults mentions either his own earlier arguments concerning passages on “loose sheets temporarily separated from copy” or John Burrow and Thorlac Turville-Petre’s recent ones, nearly identical to my own, regarding “revisions entered [into the B-version archetype] ... on loose sheets.”⁶ Indeed Hanna calls the latter critics’ edition “certainly the most significant contribution to Piers Plowman textual studies in the last twenty years, perhaps in the nearly sixty years since George Kane’s A-text.”⁷

In late 2016 Sarah Wood, too, published an essay called “Langlandian Loose Leaves and Lost Histories,” which opened by quipping that “in spite of its implausibility, Manly’s ‘lost leaf’ has continued, at some level, to fascinate scholars.”⁸ That qualifier is necessary since no one endorses Manly’s theory; the scholars supposedly nevertheless fascinated by it are E. Talbot Donaldson, Robert Adams, Wendy Scase, and, especially, me.⁹ Wood had

³ Hanna, “Loose Leaves,” p. 187. He identifies these “avatars of similar arguments” at pp. 193–94, n. 1.

⁴ Hanna, “Loose Leaves,” p. 195, n. 12.

⁵ Ralph Hanna, “On the Versions of Piers Plowman,” in Pursuing History: Middle English Manuscripts and Their Texts, *Figurae: Reading Medieval Culture* (Stanford: Stanford Univ. Press, 1996), pp. 204–14 (against Scase); and Hanna, “George Kane and the Invention of Textual Thought: Retrospect and Prospect,” Yearbook of Langland Studies, 24 (2010), 14–19 (against Warner).

⁶ Hanna, “On the Versions,” p. 217; Piers Plowman: The B-Version Archetype (Bx), ed. John A. Burrow and Thorlac Turville-Petre, PPEA print series, vol. 1 (Raleigh, N.C.: SEENET, distributed by University of North Carolina Press, 2018), p. 7. See also note 18 below for Hanna’s advocacy of the early circulation of loose matter attesting C material. Burrow and Turville-Petre’s edition originally appeared online in 2014; it and all other volumes of the Piers Plowman Electronic Archive cited below are available at [<http://piers.chass.ncsu.edu>].

⁷ The B-Version Archetype, ed. Burrow and Turville-Petre, back cover blurb.

⁸ Sarah Wood, “Langlandian Loose Leaves and Lost Histories,” The Library, ser. 7, 17 (2016), 371.

⁹ Wood, “Langlandian Loose Leaves,” pp. 371 n. 4 (list of culprits), 372 and n. 5 (Warner).

earlier written an essay, published in this journal, arguing that Scase's suggestion that some authorial passages had been recorded on loose sheets "surely represents a case of wishful thinking";¹⁰ Scase, she claims, was unable to recognize manifestly scribal materials. "Langlandian Loose Leaves" turns its guns on me, asserting that my proposal that the postulated passages might be early Langlandian material "simply ignores the textual evidence that points . . . overwhelmingly against such a conclusion" and that "[t]he seductive idea of the Langlandian loose leaf distracts Warner from undertaking any systematic analysis of the actual variants of the passages that he would propose as authorial draft."¹¹ She does not take note of Hanna's or Burrow and Turville-Petre's remarks regarding Langlandian loose leaves; indeed "Langlandian Loose Leaves" in effect serves as an apology for Turville-Petre's textual work.

Neither Hanna's nor Wood's essay mentions the other, so it is unclear whether this steady stream of identical claims is the result of an extraordinary coincidence, unacknowledged collaboration, or some other mode of convergence. It is not just the arguments and assumptions that are identical: more important, I want to suggest, is the common methodology employed by these critics in their assaults on the "loose leaf" approach. For their critiques of Scase and me are not purely rhetorical, delicious though that rhetoric undoubtedly is. Both of them also offer alternative interpretations of the textual phenomena in question, on which depends our entire understanding of not just the early production and transmission, but the very identity, of Piers Plowman. One of this essay's main goals is to bring this approach into the open, an approach that loosely follows this model: begin by citing the "inabilities," "wishful thinking," and so forth of the critic whose work is to be undermined; isolate one element of a multifaceted argument, offering a simple

¹⁰ Sarah Wood, "Nonauthorial Piers: C-text Interpolations in the Second Vision of Piers Plowman in Huntington Library, MS HM 114," JEGP, 114 (2015), 483.

¹¹ Wood, "Langlandian Loose Leaves," p. 374.

alternative explanation, usually a statement that a few readings might be scribal; and announce that therefore the opponent's interpretation of the multifaceted phenomenon has been proven impossible. The crucial and final move is still subtler and more effective: in the final paragraph or two, present one's approach as a "demonstration" and conclude with an acknowledgment of the most compelling evidence for the opponent's claim that nevertheless dismisses that evidence as a large-scale coincidence since the argument has already been settled. By these means, manuscript affiliations that by conventional standards of textual criticism provide clear evidence of genetic relations, that is, descent from an exclusive mutual ancestor, are announced to be the results of convergent variation (coincident substitution, consultation of another copy), while agreements that by definition can only be evidence of convergent variation are opportunistically rescued and rendered meaningful.¹²

This reversal of the logic of convergent variation makes for a remarkable chapter in the history of Middle English scholarship, worthy of notice in itself. My second goal is to correct two of the many specific textual arguments that rely on this reversed application of the concept of convergent variation. The first of these is Hanna's purported argument against Scase regarding the "Ilchester Prologue," together with Wood's essay reviving Hanna's approach, which draws me into the mix, on the basis, however, of a sequence of errors of fact and a fundamental misunderstanding of how convergent variation works, of which readers ought to be made aware quite apart from the fact that these missteps are so consequential. The second is Hanna's influential argument that MS F of Piers for a crucial passage attests not the B version of the poem but rather C, despite the fact that it has no readings that must have come from C, and forty-six agreements with B against C, including at every point of

¹² "Convergent variation was produced in three ways, by coincident substitution, by memorial contamination, and by consultation of another copy." The latter two are easy enough to understand; the first, and most pervasive, coincident substitution, "occurred because every piece of text in its context contained inducements to certain classes of substitution, from its content or language or metrical form" (Piers Plowman: The C Version, ed. George Russell and George Kane [London: Athlone, 1997], p. 59).

major divergence. At issue are not just the Ilchester Prologue and MS F, important though those texts are in their own right, but also the status of the earliest production and transmission of Piers Plowman, and in particular whether the received account of integral B and C versions can withstand the many counterindicators that have arisen in the last thirty years.

THREE LINES NOT IN THE ILCHESTER PROLOGUE

In 1987 Wendy Scase pointed out that both the Ilchester Prologue in Senate House Library, University of London, MS S.L. V.88 (MS J) and the conflated text of Piers in San Marino, Huntington Library, MS HM 114 (MS Ht), in its Passus 6, “include unique versions of the lengthy passages on false hermits or lollers,” C 9.66–163, 189–280, “and of the passage usually associated with the C-text Prologue, on prelates and idolatry (the Ophni and Phinees passage, C Pro 91–127).”¹³ These insertions share striking similarities in shape, content, pattern of omissions, and substantial agreements that differ from the readings of all other C-text manuscripts. The most telling similarity is that these are “two passages new in the C-text, lacking the B-text lines found interlineated with this material in the received C-text.”

Addressing the question of whether these passages might have originated as excerpts from the C tradition, Scase observes “that in the exemplar the two C-text passages were associated with a few lines of contextual material,” concluding:

Anyone excerpting chosen passages from a C-text might easily include some B-text lines, as it is highly unlikely that he would wish to excerpt only material new in the C-text, and equally unlikely that he would have any means of identifying it. But we would not expect excerpts consistently to follow this pattern, of new C-text material preceded and followed by a few lines of B-text context. When this pattern is linked

¹³ Wendy Scase, “Two Piers Plowman C-Text Interpolations: Evidence for a Second Textual Tradition,” Notes and Queries, n.s. 34 (1987), 456. Here and throughout, I follow the line numbers of Russell and Kane, eds., The C Version.

with the absence of the interlineated B-text material, it seems even less likely that these fragments were simply excerpts from the received C-text.¹⁴

Her argument is thus that HtJ attest a “second textual tradition,” which is to say, that these passages were circulating “in an unfinished condition” prior to the final version found in the rest of the C tradition.¹⁵

In 1996 Hanna responded to this argument on a number of fronts. As for what to my mind is the strongest element of Scase’s argument, the absence of the new interlineated B lines, its existence is not acknowledged until the final sentences of his eleven-page discussion, on the assumption that his alternative explanation of a portion of the evidence (discussed in a moment) amounts to a “demonstration that the HtJ archetype cannot reflect an authorial draft[.]” “Given” this demonstration, he says, the absence of C 9.164–88, taken over from B, perhaps “represents a deliberate suppression by the archetypal scribe, reflecting a perceived desire for hermit, not beggar, materials.”¹⁶ “But this cannot be the answer,” objects Andrew Galloway, since the Ilchester Prologue in fact includes lines from C Passus 9 directly concerned with beggars. For Hanna, he observes, “it’s a matter of coincidence that this ‘desire’ followed the fault line between C and B,” even if he himself does not put the matter quite that way.¹⁷ Hanna’s belated explanation of the central issue is followed by only two more sentences, which suggest that the HtJ materials “formed a small booklet of C version lines” comprising “two bifolia in a 35-line format”: these loose leaves had four lines more per side than did Manly’s lost leaf.¹⁸

Hanna focuses note on Scase’s evidence but instead on the narration, based on the opening four chapters of 1 Kings (=1 Samuel), of Hophni and Phineas’s loss of the Ark of the

¹⁴ Scase, “C-Text Interpolations,” p. 460.

¹⁵ Scase, “C-Text Interpolations,” p. 462.

¹⁶ Hanna, “On the Versions,” p. 214. I silently alter references to “HtI” here and in other essays quoted to “HtJ” for the sake of consistency.

¹⁷ Andrew Galloway, “Uncharacterizable Entities: The Poetics of Middle English Scribal Culture and the Definitive *Piers Plowman*,” *Studies in Bibliography*, 52 (1999), 74.

¹⁸ Hanna, “On the Versions,” p. 214.

Lord, and the death of their father Eli (or Heli), who had failed to rein them in. The HtJ version of these lines, by not attesting lines 110b–13a, found in the received C text, does not indicate that Eli fell and broke his neck upon learning of both the loss of the ark and his sons’ deaths. I present it here in Ht’s version (fol. 42v):¹⁹

For <u>Offines</u> synne alpirfirst and <u>Finees</u> his broþer	107
Betyn were in bataylle and lostyn <u>archa domini</u>	
And for her owne syre sawe hem do þe synne	109a
Suffrid hem to do ylle chastise hem ne wolde	109b/110a
Per he fil for sorow from a chayer þer he satte	113b/114a
Breke his nekkebone in two & alle for veniaunce	114b/115a
And for þei were prestes & men of holy chirche	116

The absence of lines 110b–13a from HtJ, Hanna believes, “can successfully be explained as yet another example of the J scribe’s response to his derived C version archetype,”²⁰ which, he proposes, had been mislineated thus:

For Offines synne and fines his brother	107
Thei were discomfited in batayle and losten <u>Archa domini</u>	
And for here syre sey hem synne	109a
And suffered hem do ille and chastised hem nocht þerof	109b/110a
And nolde nocht rebuken hem, anon as it was tolde hym	110b/111a
That þe children of Israel were disconfit in batayle	111b/112a
And <u>Archa domini</u> lorn and his sones slawe ther	112b/113a
Anon he ful for sorwe fro his chayere þer he sat	113b/114a
And brake his nekke atwene and al was for vengeance	114b/115a
He bet nocht his children	115b
And for þei were prestis and men of holy chirche	116

According to this account, the scribe of HtJ’s exclusive common ancestor “returned to copy at a line ending þer(of) but did so at the wrong one, the second such, not the first. In this process, the scribe was materially aided by another repetition . . . , the phrase ‘Archa domini lorn,’ which the scribe would, of course, have remembered copying” from line 108. This

¹⁹ This is my transcription from the digital image of this folio included in Patricia Bart, “The Whole Book: Textual, Codicological, Paleographical and Linguistic Artifacts in Huntington Library Hm 114 (Ht) of Piers Plowman,” (PhD diss., Univ. of Virginia, 2007). In quoting manuscript material I provide capitalization of line-opening terms and silently expand abbreviations. Another transcription, with a few small differences, is printed in Kathryn Kerby-Fulton, “Langland ‘In His Working Clothes’? Scribe D, Authorial Loose Revision Material, and the Nature of Scribal Intervention,” in New Directions in Later Medieval Manuscript Studies: Proceedings from the 1998 Harvard Conference, ed. Derek Pearsall (York: York Medieval Press, 2000), p. 166.

²⁰ Hanna, “On the Versions,” p. 209 (altering his “I’s [sic] scribe’s” to “J scribe’s”).

possibility, which had been just that a few pages earlier (HtJ's text "can be explained" thus), turns into unequivocal fact: "Thus, rather than the traditional Pro. 111–13 being an intrusion into the received C version, these lines are genuine, and their absence in J is a complicated example of scribal error."²¹

On its own terms all this, save the conclusion drawn, is perfectly reasonable. But those terms are not as well considered as one might expect from such a major plank of an argument. The most obvious problem is that Hanna does not explain how it shows that Scase's argument "is flatly wrong," or indeed what relevance it has at all to its intended target.²² Readers of his chapter might assume Scase argues that Prologue lines 110b–13a must be recognized as a later authorial addition to the passage. To be sure, Galloway has since pointed out that the repetitive character of received C at this juncture ("Thei were discomfited in batayle and losten Archa domini ... Were disconfit in batayle and Archa domini lorn") is characteristic of Langlandian revision, but Scase herself never mentions these lines.²³ Indeed her argument easily accommodates Hanna's proposal. "Doubtless," she writes, "many features [of HtJ] are attributable to subsequent corruption."²⁴ In such a scenario the initial Hophni and Phineas passage, including lines 110b–13a, were inscribed on a loose sheet or wax tablet. A scribe was commissioned to turn copy this material onto a loose sheet for the revision copy, but he mislineated it, on account of the lack of alliteration

²¹ Hanna, "On the Versions," p. 211.

²² Hanna, "On the Versions," p. 206.

²³ Since the missing lines are of "extraordinarily repetitive, as well as completely non-alliterative" matter in the C archetypal tradition, Galloway asks: "Is it not possible that the archetype of HtJ appeared first, as Scase suggests, and that the repeated phrases in the main C tradition in this passage appeared in the course of inserting a version of the HtJ materials?" ("Uncharacterizable Entities," pp. 73, 74; he discusses this phenomenon on pp. 74–76). The lines must be included among the "omissions detailed by Pearsall in his description of the Ilchester text [that] are matched in Ht" but are not singled out by Scase ("C-Text Interpolations," p. 458).

²⁴ Scase, "C-Text Interpolations," p. 462. Likewise Kathryn Kerby-Fulton: "HtJ share several patterns of smaller variation and omission, and interpreting these is exceedingly tricky: they may or may not have been in Langland's 'loose revision material'. If not, they were obviously the work of an extremely early scribal editor, someone editing even before the Ilchester redactor" ("Langland 'In His Working Clothes'?", p. 156; her emphasis).

and messy state as draft material. This is the copy Hanna invents. Other copies were needed as well, perhaps for insertion in various other exemplars, or for promulgation as a standalone text. One of these, using the mislineated copy as exemplar, became the HtJ archetype, whose scribe omitted the lines via eyeskip.

Another difficulty with the idea that the HtJ archetype is “representative of the accepted C version” rather than, as Scase suggests, a separate C tradition, is the inability to assign that source to any known genetic group.²⁵ Scase identifies dozens of substantial agreements between Ht and J that set them apart from the received C text, and those readings that do align with received C “do not seem genetically assignable,” as George Russell and George Kane observe in their edition of that version.²⁶ Hanna attempts to accommodate this situation to his argument: “Ignoring once again minor errors, substantial variation in the reconstructible archetype [of HtJ] reflects readings associated with both textual families of the C version,” x and p. “But probable p readings predominate, and a sizeable minority of deviant readings reflects those Skeat reports from British Library MS. Cotton Vespasian B.xvi (sigil M), perhaps most notably C Pro. 119b,” where for received C “and worshchipe maumettes,” Ht reads “mawmetrie to wurship,” and M, “mawmettes honoure.”²⁷ I am not convinced by the assumption that one can determine a text’s affiliations in ignorance of minor errors,²⁸ but let us instead focus on the supposed “reflection” of M by Ht. That choice of term is apt, since there is in fact no Ht-M “agreement” here. Hanna must be thinking of their respective transpositions, yet this minor scribal error is pervasive, appearing, for example, over the following eighty lines alone in MS F’s “parceyued I” for received “I

²⁵ Hanna, “On the Versions,” p. 207.

²⁶ Russell and Kane, eds., The C Version, p. 193. See Scase, “C-Text Interpolations,” pp. 458–59, on their substantial “agreements in error.”

²⁷ Hanna, “On the Versions,” p. 207.

²⁸ “No rule about the reliability of one or another kind of variant as evidence of genetic relation survives application to the manuscripts of the A version of Piers Plowman ... The inadvisability of choosing any particular type of variant as the evidence for classifying these manuscripts will have appeared” (George Kane, ed., Piers Plowman: The A Version, rev. ed. [London: Athlone, 1988], p. 60).

parseyued” (l. 128), MS L’s “asentud to his reson” for “to þis resoun assentide” (l. 192), and the p family’s “it hongid” for x’s “hanged it” (l. 196). If the HtJ archetype is indeed descended from the C archetype, then it is not impossible that it does so alone, constituting a separate family apart from the main two, as MS H seems in the B tradition on account of its “indeterminable” genetic position,²⁹ but it was the inability to assign it to x or p that led Scase to argue that HtJ attest a second (that is, non-archetypal) textual tradition in the first place.

Hanna presents as the “fullest proof” for his claim “nearly 40 readings in which J has embellished nonrhyming lines in the archetype,” but it is Scase who brought this body of data to light and it is difficult to see how it undermines her conclusion.³⁰ This body of readings only supports his case if one begs the question, as Hanna does throughout this discussion: “Here Ht palpably copies the C archetype,” which the J scribe ““corrected””; “An archetypal error, recorded in Ht, has left a line metrically deviant, and J restores a standard aa/ax alliterative pattern,” and so forth.³¹ The issue is not J’s normalization of the text represented by Ht, which took place two generations after the production of the copy in question, but rather whether Ht’s readings must be taken as palpable copies of the archetypal text. Perhaps

²⁹ George Kane and E. Talbot Donaldson, eds., Piers Plowman: The B Version, rev. ed. (London: Athlone, 1988), p. 61. This is London, British Library, MS Harley 3954, which attests B to about 5.127, and which “may represent a third line of descent.” The other possibility would be that the HtJ archetype was corrupt to a degree found nowhere else in the manuscript tradition of Piers Plowman, and became so over the space of an extraordinarily narrow window given that (so Hanna has it) C “must post-date 1388, if not 1390” according to “Anne Middleton’s detailed demonstration” (“Invention,” p. 14) and that Ilchester’s decoration, in the judgment of A. I. Doyle and M. B. Parkes, “seems more appropriate to the late fourteenth century than that which appears in D’s other manuscripts, and this manuscript might therefore be taken to represent one of D’s earliest efforts” and thus among the earliest extant copies of the poem (“The Production of Copies of the Canterbury Tales and the Confessio Amantis in the Early Fifteenth Century,” in Medieval Scribes, Manuscripts and Libraries: Essays Presented to N. R. Ker, ed. M. B. Parkes and Andrew G. Watson [London: Scholar Press, 1978], p. 195).

³⁰ Hanna, “On the Versions,” p. 207. Scase lists instance after instance in which “the Ilchester line becomes explicable when seen as a rewriting of something close to the unmetrical line found in Ht. These comparisons of Ilchester with Huntington HM 114 confirm Pearsall’s suspicion that the Ilchester Prologue has been ‘improved’, and help to explain why it often seems none the less inferior to the received text. Ilchester is better explained not as an improvement of the received C-text, but of something close to the Ht text” (“C-Text Interpolations,” p. 459, citing Derek Pearsall, “The ‘Ilchester’ Manuscript of Piers Plowman,” Neuphilologische Mitteilungen, 82 [1981], 181–93). As Kerby-Fulton wryly observes, Hanna “does helpfully (re)demonstrate her view that J’s lines were metrically ‘improved’ in scribal transmission, and that Ht’s are clearly deficient” (“Langland ‘In His Working Clothes’?,” p. 140, n. 3).

³¹ Hanna, “On the Versions,” p. 208.

Ht's text strays from the original, as Hanna assumes, but he presents no evidence that that original must have been the archetype and not the matter Scase posits on loose leaves.³² In Scase's account, both that and Cx attest stages of the same passage, after all.

As we have seen, critics who "believe they are in touch with and recognize a poet who should not have perpetrated such a thing" as discontinuity expose themselves to Hanna's ridicule in "Loose Leaves," but the foundation of his argument here against Scase, too, is the belief that the passage as attested in HtJ is marred by an absence that "appear[s] to obliterate the very point of the passage."³³ He follows Derek Pearsall, who had considered it "superfluous to point out how the story loses all sense if Heli's falling from his chair is made into an instant reprisal for his failure to bring up his children properly, without connection with the loss by his sons of the Ark of the Lord, which is the occasion for introducing the story in the first place."³⁴ It seems to me that that earlier line, "Betyn were in bataylle and lostyn archa domini," provides that reason. Furthermore, Kathryn Kerby-Fulton has noted that HtJ's passage as it stands "was perhaps originally between prelates and fathers—not prelates and sons,³⁵ while a still more plausible interpretation is that the passage concerns fathers (spiritual or literal) and sons. For late-medieval English sermons frequently link Eli's fall only with his sons' death, with no mention of the Ark of the Lord. Harsh correction, explains one preacher to his ecclesiastical congregation, is preferable to soft tolerance: "This is shown in Eli, who corrected his sons negligently, as 1 Kings 2 says: 'Why are you doing

³² Likewise Kerby-Fulton: "Ralph Hanna is the only scholar who has rejected Scase's view, asserting that 'the HtJ archetype cannot reflect an authorial draft' (p. 214), an assertion for which, however, he provides no incontrovertible evidence," followed by her comment quoted above, note 30, then: "But he offers no concrete evidence that the passages could not have been authorially derived at their point of origin" ("Langland 'In His Working Clothes'?", p. 140, n. 3; her emphasis).

³³ Hanna, "On the Versions," p. 209. My emendation assumes that Hanna intends to say that the omission of the lines, rather than the lines themselves (as he writes), appears to obliterate the sense.

³⁴ Pearsall, "The 'Ilchester' Manuscript," p. 191.

³⁵ Kerby-Fulton, "Langland 'In His Working Clothes'?", p. 158 (her emphasis). She observes that Pearsall was quite right to suspect this as scribal, since "J's alliteration destroys the analogy": "J obscures the paternal image 'fadris' in favour of the alliterating 'gyours'; Hanna's argument (Pursuing History, p. 211) that the omission of 111 and 112 is scribal accident ... neglects the fact that the Ht version (although not the J version) may actually make its own economical sense without these lines" (n. 41).

these things that I hear about from the people, very bad things? It is not a good report I hear about you.’ And for that he lost them by their death in the war. And he himself fell from his chair and died with his neck broken.”³⁶ Another preacher, on Palm Sunday in front of a mixed lay and clerical audience, exhorts curates to chastise those in their care who sin and adds a lesson for the parents in the crowd:

unless he amends himself of this fault in this life, he will at last be condemned in hell [or grievously] without end. In evidence of this we read in 1 Kings 3 and 4 about Eli, who was a priest. As he knew that his sons were doing wrong and did not correct them, God threatened that he would send him vengeance for his neglect. And so God did, as the biblical text says right afterwards, for on one day his sons were killed and their father broke his neck. This danger and mishap similarly befalls fathers and mothers who out of negligence do not chastise their children.³⁷

In sum, there is nothing amiss with HtJ’s connection of Eli’s falling from his chair and his paternal negligence, and so no need to reconstruct a lost original that attested lines 110b–13a.

Sarah Wood’s JEGP essay seeks to rehabilitate Hanna’s argument in the wake of Galloway’s and Kerby-Fulton’s objections as cited above. The collations in Russell and Kane’s edition of Piers C, she claims, “reveal at a glance the prescience of Hanna’s hypothetical reconstruction of the mislineation that resulted in the dropping of lines from HtJ’s exemplar. For four C-text manuscripts, P²OLB, share in part (at ll. 112–16) the very mislineation that Hanna suggested lay behind HtJ’s text” and that caused the eyeskip he postulated.³⁸ I here present the text as found on folio 2r of London, British Library, Additional MS 10574 (MS L; Russell and Kane erroneously reverse O and L in the apparatus), bolding the terms in question:³⁹

For <u>Offines</u> synne and <u>Fines</u> his broþer	107
Pei weren disconfitid in bataile and losten <u>archa domini</u>	
And for here sire say hem synne and suffrid hem do ille	
And chastised hem not perof and nolde not rebuke hem	

³⁶ Siegfried Wenzel, “Eli and His Sons,” Yearbook of Langland Studies, 13 (1999), 142, Wenzel’s translation of a visitation sermon in Hereford Cathedral Library MS O.iii.5, fol. 43r-v.

³⁷ Wenzel, “Eli and His Sons,” p. 148, Wenzel’s translation of a sermon in the monastic collection Worcester Cathedral Library MS F.10, fol. 126r.

³⁸ Wood, “Nonauthorial Piers,” p. 489.

³⁹ I consulted Additional MS 10574 in person.

Anon as it was told hym þat þe children of Israel
 Were disconfit in bataile and archa domini lorn and his sonnes slawe 112/113a
Pere anon he fel for sorwe fro his chaier þer he saat 113b/114a
 And brak his necke atweyne and al was for vengeance 114b/115a
 He bet not his children and for þei weren prestes & men of holichirche 115b/116

This, Wood claims, constitutes “the textual evidence—some of it unavailable to Scase and Hanna and overlooked by subsequent commentators—[that] points overwhelmingly to the likelihood that the materials Ht shares with J ... reflect exemplars available in the London book trade, exemplars removed at many stages from any original Langlandian composition.”⁴⁰ The “willingness to overlook” this evidence by Galloway, Kerby-Fulton, and me⁴¹ has blinded us to “the very brilliance of Hanna’s set-piece reconstruction.”⁴²

We did not overlook this evidence, though, because there is no evidence to overlook. According to Wood, “Hanna’s reconstruction proves inaccurate in only one small respect,” that is, in its placement of **pere** at line-ending 113a rather than line-beginning 113b as in these manuscripts.⁴³ But this is hardly a small inaccuracy: the placement of **pere** in terminal position is the whole point of Hanna’s reconstruction. And neither is **perof** in the terminal position necessitated by Hanna’s logic: “And chastised hem not **perof** and nolde not rebuke hem.” P²OLB has nothing to do with the eyeskip that Hanna postulated by the scribe of the

⁴⁰ Wood, “Nonauthorial Piers,” p. 485.

⁴¹ “What prompts Scase’s and Warner’s willingness to overlook such infelicities in the individual readings of the HtJ interpolations is the allure of their apparent ‘shape’ as passages new to C that could be accommodated on loose leaves” (Wood, “Langlandian Loose Leaves,” p. 390); “both Galloway and Kerby-Fulton apparently overlook a piece of evidence that one might have expected them to consider” (“Nonauthorial Piers,” p. 489; cf. Hanna: “Scase avoids one examination she should logically have undertaken” [“On the Versions,” p. 206]), that is, P²OLB’s reading. My “view that N², Ht, and J might all share access to early authorial drafts circulating on loose leaves simply repeats Scase’s original error of failing to take proper account of the readings of the passages in question,” referring both to the individual errors Hanna cites as manifestly corruptions of the archetype, discussed above, and the “now-published C-text collations [that] confirm his analysis of the scribal omission of lines 110b–13a in the C Prologue by showing that four C-text manuscripts, P²OLB, share in part the same mislineation (in lines 112–16) that Hanna argued had caused the ancestor of HtJ to drop several lines” (“Langlandian Loose Leaves,” pp. 389–90). She picks up on a sentence in Lawrence Warner, The Lost History of “Piers Plowman”: The Earliest Transmission of Langland’s Work, The Middle Ages Series (Philadelphia: Univ. of Pennsylvania Press, 2011), p. 31, which provided external suggestive support for the viability of the notion that loose leaves containing early C matter were in circulation. The book’s argument stands on its own terms “even if N² is not to be included in this picture” of HtJ loose matter (Warner, “Impossible Piers,” Review of English Studies, 66 [2015], 226).

⁴² Wood, “Nonauthorial Piers,” p. 491.

⁴³ Wood, “Nonauthorial Piers,” p. 489.

HtJ archetype. But even if we ignore that fundamental problem, these manuscripts’ readings still would not confirm his approach. “If the mislineation of lines 112–16 in P²OLB were unrelated to the mislineation and omission in lines 109–16 in HtJ, it would be, to say the least, an extraordinary coincidence,” Wood claims, going on to endorse the major alternative to coincident substitution, descent from a mutual common ancestor, as the explanation for this proposed relation: “Since the misdivision in lines 112–16 appears only in four manuscripts of the x family, it must have occurred after the division of the C archetype into the two great families,” x and p, with “the further mislineation of lines 109–12 that apparently led to the dropping of lines in the HtJ archetype requir[ing] at least one further stage of copying[.]”⁴⁴ In this account HtJ are indeed far from any authorial stage of production. Because the remaining three x manuscripts, XUD, share correct lineation with p (which she does not mention), the stemma she is narrating looks like this:

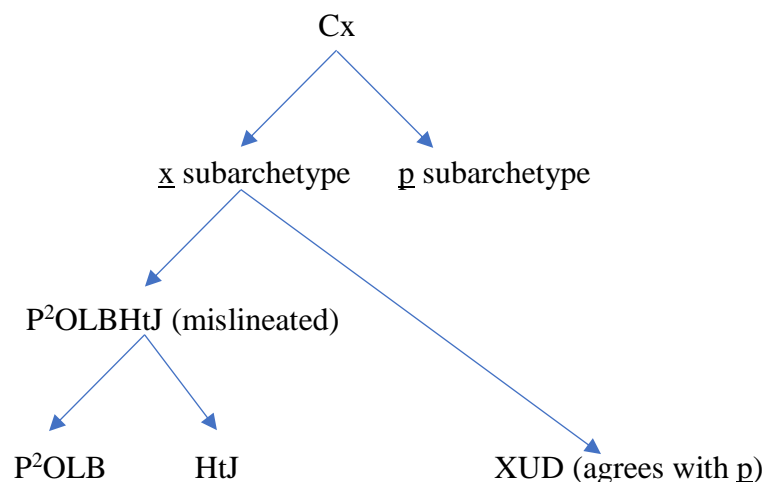


Figure 1: stemma for C Prol.112-16 necessitated by Wood’s theory

But since P²OLB do not form a genetic group within the x family, their agreement for 112–16 must be coincidental, Wood’s surprise at that fact notwithstanding. Let me substantiate this claim. Its basis is that two manuscripts, P² and [OLB], not four, agree with

⁴⁴ Wood, “Nonauthorial Piers,” p. 489.

the mislineation, OLB making up one of the most well attested genetic groups.⁴⁵ These two witnesses' sole other shared agreement is the very minor lich for received lith at Prologue line 137, brought about by the common confusion of c and t; for example, at A 11.2 the scribe of MS K succumbed to the error in reverse, substituting lithe for correct lich.⁴⁶ For every other reading, P² and OLB either appear together with the remaining x or Cx witnesses, or clash with each other, P² agreeing with XUD against OLB, or OLB with XUD against P².⁴⁷ The only explanation is that these two witnesses descend, independently of each other, from the x subarchetype, [XP²OLBUD] itself.⁴⁸ And since XUD agree with p in lineating lines 112–16 correctly, the error in question was not in those manuscripts' ancestor, this same x subarchetype. The scribes of P² and of the OLB archetype thus mislineated these lines separately.⁴⁹ This coincidence is not particularly extraordinary. The “prolix and repetitive” lines 108–24, as Russell and Kane observe, “must be the roughest of Langland’s drafts” in that “only four lines (109, 117, 119, 123) alliterate normatively,” which renders it by far the

⁴⁵ OLB agree in error fifty-two times over the 584 lines from the Prologue through 2.131, a number “equivalent to 600 over the whole poem” (Russell and Kane, eds., The C Version, p. 40).

⁴⁶ See apparatus for Prol.137 in Russell and Kane, eds., The C Version. They preface the reading with a question mark, but L is definitely lich, and P² (London, British Library Additional MS 34779, consulted in person) liche, at the least. This exemplifies one of the “large number of variants originating from a more confidently determinable cause” than conscious substitution, “the similarity of various letters in the handwritings, whether formal or intermediate, of the fifteenth centuries,” including “Confusion of t and c,” which Kane exemplifies by, among others, K’s at A 11.2 (Kane, ed., The A Version, pp. 119–20).

⁴⁷ **XP²UD] OLB** readings, all from C Prologue, include: 2 shroudes] a shrowde. 95 cussed] accusid. 101 for it profiteþ 3ow] 3ee for it profiteþ; 3e prelati] prelati it. 155 vt] quod. 185 line om. OLB only. 191 roume] shonye. 219 reed] reik. **P²] XOLBUD**: 64 shut] choppe. 77 iset] ysent. 78 bischops leue] bischop y leue XOLBD (U: bishopes y leue). 82 line om. P² only. 125 Her] 3oure. 150 folwe &] zero. 174 cache] vs halde. 180 bendes al] beyus. 206 seue] seune. 207 nis] is; lewed] elynge.

⁴⁸ See Russell and Kane, eds., The C Version, pp. 42–43, on XJP²OLBUD as a genetic group in Passus 1; in the Prologue, where J is not collated, XP²OLBUD agreements occur in ll. 46, 49, 61, 64, 84, 105–6, 127, 136, 146, 147, 166, 184, 189.

⁴⁹ Those resistant to such a conclusion might point to Russell and Kane’s remark that a similar misdivision, XYJ’s of 9.71–79, “seems too extensive to be coincidental error” (Russell and Kane, eds., The C Version, p. 43; their “XI 71–9” is an error for “IX 71–9,” correctly presented in their tally of XYJ’s agreements in error on that page). Yet quite in opposition to P²OLB, “XYJ can be presumed to have an exclusive common ancestor” on the basis of a substantial body of evidence: thirty-three exclusive errors including this mislineation, as well as dozens of XY, XJ, and YJ agreements, which came about via either the third manuscript’s further error or “sporadic consultation and ‘correction’ in a single copying centre” (pp. 43, 44). Also, that passage, unlike 108–24, is not unalliterative (see following).

most susceptible to such mislineation of all passages in Langland's poem.⁵⁰ Indeed, this enabled Hanna to invent his copy, and forced the medieval scribes to turn instead to sense and grammar to guide their presentation. The results in P² and OLB look as viable as the received text: the past participle slawe ("slain") in line 113 seems, like disconfit and lorn, dependent on 112's opening Were: hence 112/113a, in which the brothers "were routed in battle, and the ark of the Lord (was) lost, and his sons (were) slain." Each of the following three lines, too, makes good sense on its own: he fell from the chair where he sat; he broke his neck and all was for vengeance; he did not beat his children for they were priests. That HtJ and P²OLB are not very close in either their lineation or their respective texts confirms the point that the absence of alliteration lent itself to this error.⁵¹

On the assumption that her bombshell has done its damage, Wood turns to the question of how this material originated, referring in passing to the major difficulty facing her argument. She endorses D. Vance Smith's recent argument that the HtJ archetype "was either a digest of some significant changes that had been made to the B Text, edited, to a certain extent, to stand alone or alongside a B Text; or it was something analogous to a software patch, a short text or set of pages that would enable a scribe to update—at least partially—an

⁵⁰ Russell and Kane, eds., The C Version, p. 87. Their claim that these lines "would read like prose if so written out," though, is perhaps overstated; Galloway says that this passage "is not 'prose'; it has the right metrical stresses" ("Uncharacterizable Entities," p. 77; misquoted by Wood, "Nonauthorial Piers," p. 492, n. 33). But the general point that this set of lines lacks the alliteration that distinguishes the rest of the poem certainly stands. Wood argues that the HtJ version of line 9.107, "Suche as lunatyk and lepers aboute," which does not have the received C term "lollares" ("The whiche aren lunatyk lollares and leparas aboute"), is a scribal corruption of the authorial original: "To accept that the line in its HtJ form might be Langland's original, later revised to include the missing term 'lollares,' one must also suppose that the poet's draft composition lacked not only alliteration but even the required number of stresses to qualify as verse" (p. 492). But as she in effect acknowledges, no one has ever insisted that this must be authorial; Kerby-Fulton, the only critic to discuss the line in this context, attributes the absence to either the poet, who had not settled on a meaning of the term, or an early redactor ("Langland 'In His Working Clothes'?", p. 158). I tend to agree with Wood that this is simply a mechanical error; in any case its status has no bearing on Scase's argument.

⁵¹ Ht's Offines synne (l. 107) is shared by OLB (P² reads Offines cam), but also by QSF of p, while J's þe synne of Offyn is attested by that family's PEVA group; both wordings are reflected as well in N's offynes synnes and D's Offines is syn. Ht's to do ylle (l. 109b/110a; J spurious), too, is in QS. The manuscripts D of x and RM of p join Ht in omitting wel (l. 117; J spurious). Russell and Kane observe that HtJ's insertions here "agree about three dozen times with C copies in variation from the adopted text. Not one agreement is even relatively persistent" (The C Version, p. 193, n. 7)

older version of the poem without the expense of producing or acquiring a full C Text manuscript to use as an exemplar.”⁵² This is put forward as a correction of Scase, but other than the crucial term “digest” it reads like a repetition of it. The theory that they are stand-alone passages matches her “theory that the C-text passages were in circulation, and available to the compilers of the Ilchester Prologue, and the Huntington text, in uninterpolated form”; the alternative, that they were a software patch, is another way of saying that they circulated “with a few lines of contextual material” which “facilitate insertion.”⁵³ His conclusion, that is, reads like a description of the reasons Scase rejects it: standalone passages of new C material, perhaps intended as easy ways to update a complete copy, are the products of a poet (just as software patches come from Microsoft or Apple) not of a later reader (or user of Word for Mac).

As for the fact that the B lines are not carried over in this supposed digest of Piers Plowman, Smith offers a single observation: “But neither are the C lines that expand those lines within the section,” which also serves as Wood’s only answer to the problem.⁵⁴ This does not address the problem. What Scase deems unlikely is not that someone excerpting material from a C manuscript would include all lines new to that version, even among those close to the passages in question, but that this individual would choose only new C lines, with a few contextual lines carried over from B on either side of the new material, omitting the interlineated B lines, revised or not. Her point also adheres more closely to historical precedent than does his: while it is very difficult to imagine a poem like Piers Plowman C coming into being without substantial use of loose matter,⁵⁵ the first evidence of such

⁵² D. Vance Smith, “The Shadow of the Book: Piers Plowman, The Ilchester Prologue, and Inhumane Revision,” in Yee? Baw For Bokes: Essays on Medieval Manuscripts and Poetics In Honor of Hoyt N. Duggan, ed. Michael Calabrese and Stephen H. A. Shepherd (Los Angeles: Marymount Institute, 2013), p. 217.

⁵³ Scase, “C-Text Interpolations,” pp. 460, 461.

⁵⁴ Smith, “The Shadow of the Book,” p. 213. See Wood, “Nonauthorial Piers,” p. 490, where she says that, while Hanna’s explanation is weak, “neither can Scase’s argument be accepted uncritically” (see note 57 for Smith’s earlier use of the identical phrase).

⁵⁵ Russell and Kane posit “well more than a dozen separate single leaves or bifolia of new material, interleaved or loose” in the reviser’s copy (The C Version, p. 89), but A. V. C. Schmidt considers it “at best

excerption of Piers's English passages from a manuscript source does not arise until the seventeenth century.⁵⁶

Given the near-identical character of Smith's and Scase's respective accounts of HtJ's purpose, and Smith's absence of any explanation for the most compelling piece of evidence in support of Scase's proposal, why does Smith nevertheless "find it difficult to accept uncritically that the HtJ archetype is descended from loose revision material before it was integrated into a text in the process of becoming the C version"?⁵⁷ The answer is that the excerpts "fall into sections of remarkably similar length," so he claims. "Three of the five blocks of text from the C Text"—he refers to 9.75–87, 9.96–163, 9.189–255, 9.256–80 (separated from the previous by A matter), Prol.91–159—"take up exactly 66 lines each in the Ilchester Prologue, although they correspond to passages that are longer or shorter in the received text of C."⁵⁸ The omission of 9.164–88, the revised matter not carried over, he says, "gives a section of text from C Passus 9 that is 132 lines long, or two folios containing 33 lines," indicating that the compiler "creates the opening of the Prologue in a unit of 66 lines from these two disparate sources, which suggests that his manuscript was ruled at 33 lines per page throughout"—strong echoes of Manly's "precise number of lines to the page in the MS"

only probable (and to the present editor, no more than possible)" that Langland's process of revision proceeded as envisaged by Russell and Kane. He allows "only rare passages such as the Ophni-Phineas lines in Pr 95–124 (and not necessarily even these) remaining in enough of a draft state to lend credence to the Athlone editors' postulated 'physical processes of revision'" (Piers Plowman: A Parallel-Text Edition of the A, B, C and Z Versions, vol. 2: Introduction and Textual Notes, rev. ed. [Kalamazoo, Mich.: Medieval Institute Press, 2011], p. 62). I cannot infer from his comments the manner in which he thinks the new passages were composed if not on new material.

⁵⁶ The seventeenth-century antiquarians Richard James and Gerard Langbaine are the first known readers to make collections of excerpts in English. Three early excerpts of English lines from Piers occur (two Prol.1–4; the third either A 1.162 or C 1.184), and two prophetic passages are combined in a number of sixteenth-century compilations and commonplace books, but it is unlikely any of these was copied from a manuscript. Otherwise all excerpting over the poem's first two centuries was of Latin lines. See Lawrence Warner, The Myth of "Piers Plowman": Constructing a Medieval Literary Archive, Cambridge Studies in Medieval Literature, 89 (Cambridge: Cambridge Univ. Press, 2014), pp. 67–71 on the history of excerpting, and Chapter 4 on the prophetic passages, on which Eric Weiskott has built: "Prophetic Piers Plowman: New Sixteenth-Century Excerpts," Review of English Studies, 67 (2016), 21–41; and "More Prophetic Piers Plowman," ANQ, 30 (2017), 133–36.

⁵⁷ Smith, "The Shadow of the Book," p. 217.

⁵⁸ Smith, "The Shadow of the Book," p. 215.

that lost its leaf (Smith's are two lines longer).⁵⁹ He concludes that this compiler "fit his material according to the spatial constraints of the manuscript he was producing, or to the folios he had on hand, rather than work from loose sheets."⁶⁰ It was a compiler who did this, rather than a poet inscribing the matter (or directing it to be inscribed) onto loose sheets, because "the meticulous blocking involved in copying these extracts suggests work that comes well after the stage of composition."⁶¹ On this basis Wood announces that "Vance Smith has ... comprehensively demolished this argument [of Scase's] about the 'shape' of the passages as reflecting loose leaves of C-text draft."⁶²

But Smith's logic is circular, the meticulous blocking involved in copying these extracts showing that meticulous blocking was involved in copying these extracts. That aside, his proposal comes up against two serious problems: that the thirty-three-line-ruled C copy from which this compiler excerpted this matter belonged to neither x nor p and thus not to Cx either; and more devastating, that the figures on which Smith bases his argument are not accurate. The three longer sections of the Ilchester Prologue's C matter are not "exactly sixty-six lines each": in fact, none of them is. Of the ten blocks of C text shared by Ht and J, only the former's 9.189–255 is sixty-six lines. Here are the correct numbers:

9.75–87: J fourteen (?orig. 9.70–87, nineteen, plus "some improvised transitional lines") // Ht 9.66–87: twenty-one
 9.96–163: J sixty-eight // Ht sixty-five
 9.189–255: J sixty-five // Ht sixty-six
 9.256–80: J twenty-two // Ht twenty-five (plus one spurious line at end)
 Prol.91–159: J sixty-three // Ht Prol.91–127 thirty-one⁶³

⁵⁹ Smith, "The Shadow of the Book," p. 216. For Manly, see above, note 1.

⁶⁰ Smith, "The Shadow of the Book," p. 216. In his discussion of Galloway's response to Hanna, Smith does not mention Prol.110b–13a, focusing only on lines present in J but not Ht. He says that Galloway's observation that repetition "could also be the result of the insertion of new material while the C Text is in the process of being revised," while possible (see above, note 23), "still does not explain how Ilchester reflects readings that appear in the revised text of C but not in HM 114" (p. 211). But it would only need to do so if it were known that Ht were an absolutely accurate copy of the HtJ archetype, which is manifestly not the case. The HM 114 scribe might have omitted these by either mistake or strategy: picking and choosing what to include from his sources is his most singular characteristic, after all.

⁶¹ Smith, "The Shadow of the Book," p. 216.

⁶² Wood, "Langlandian Loose Leaves," p. 390.

⁶³ For these figures, my methodology was to count the lines in Kerby-Fulton's transcriptions, "Langland 'In His Working Clothes'?", pp. 162–67, check those for Ilchester against Russell and Kane's

If not for this mistake at the heart of his argument, this might have provided a viable alternative to Scase's proposal. Smith's engaging essay intriguingly interprets the HtJ materials in the form in which they reached the J and Ht scribes: it is "an elegant and canny compilation" of passages on ecclesiastical abuses of office, one which, however, omits "precisely those passages that describe intractable frailty, the unavoidable (as opposed to the willing) failure of a human to maintain its own life."⁶⁴ Even if his conclusions do not all stand up, Smith's attention to the codicological indicators that portions of the poem might have taken on lives of their own, even in the poem's earliest years, is salutary.

Another recent proposal along such lines, picking up on the fact that the final two passus of C, which are "lightly revised in places" and possibly "unrevised" from B,⁶⁵ suggests that they might have joined the HtJ passages as part of "a program that associates friars, illicit sexuality, and the question of fyndynge (that is, livelihood or endowment) in a series of passages first inscribed on loose sheets that could be read or copied independently and then incorporated into the C" version of the poem, as well as into the B archetype.⁶⁶ Central to this new proposal of mine was the steady stream of agreements between the alpha group of B, comprising manuscripts R (most of which is Oxford, Bodleian Library, MS Rawlinson Poetry 38) and F (Oxford, Corpus Christi College MS 201), and the C text in those passus—many more than earlier in the poem, and many more than beta has in these passus. The simplest explanation, I suggested, was that the copy of these lines provided to the

transcription (The C Version, pp. 186–92), and repeat to ensure the figures were accurate. Russell and Kane remark that the faded "first folio contained Prologue 1–54, some improvised transitional lines, and C IX 70–4" (p. 186), so those five plus the fourteen of 75–87 on the verso make for nineteen lines; cf. Smith: "the opening of the Prologue ... contains 18 lines from C Passus 9" (The Shadow of the Book, p. 216). Other than his remark that the three longer passages "take up exactly 66 lines each," his only comment regarding length is that "[t]he material imported from a C Prologue is also 66 lines long in Ilchester" (p. 216). And where I count twenty-two in 9.256–80, he has twenty-four (p. 216).

⁶⁴ Smith, "The Shadow of the Book," pp. 214, 218.

⁶⁵ Respectively, Schmidt, Introduction and Textual Notes, p. 166; Russell and Kane, eds., The C Version, p. 91.

⁶⁶ Warner, Lost History, p. 62.

B archetype (Bx) had been updated with individual authorial corrections between these scribes' respective copyings of it. Ralph Hanna has sought to undermine this argument, as well, in his 2010 essay in the special section of The Yearbook of Langland Studies commemorating the work of George Kane. The remarkable and unprecedented way in which he attempted to do so is the subject of the rest of this essay.

PASSUS B 19 OF CORPUS CHRISTI COLLEGE MS 201 (MS F)

From B 18.411 through 20.26, where MS R is absent, MS F of B agrees with Cx against beta of B for some fifty-eight readings, an extraordinarily high number, which forces editors into impossible situations.⁶⁷ Bolstered by the fact that, as Hanna remarks, these are “very frequently readings more persuasive than all other B copies,” A. V. C. Schmidt accepts their authenticity as a matter of course. He adopts no fewer than fifty-three of them, with another two deemed possibly authorial.⁶⁸ But he does not explain why the beta scribe's facility as a copyist utterly collapses at the very moment he reached Passus 19. Kane and Donaldson help the beta scribe out a bit by accepting only thirty-three of these F-Cx agreements as authorial, but the twenty-five errors in agreement they assign to those witnesses over these 537 lines are

⁶⁷ Of these fifty-eight readings, both Athlone (Kane and Donaldson, eds., The B Version; and Russell and Kane, eds., The C Version) and Schmidt (Parallel-Text, vol. 1: Text, 2nd ed.) adopt thirty-three: in Passus 19/21, lines 43, 56–59, 60, 73, 77, 94, 118, 130 (x2), 140 (Athlone: Russell-Kane; Kane-Donaldson adopt beta), 145 (x2), 149, 151, 152, 179, 181a, 208, 236b–37a, 273, 274, 283, 284, 330, 334, 336, 394, 422, 446, 453, 457, 463, 479. Schmidt alone adopts twenty: Passus 19/21, lines 24, 91, 109, 120, 142, 154, 223 (x2), 228, 267, 280, 295, 311, 479a; and Passus 20/22, lines 1, 3, 7, 9, 11, and 13. Neither adopts those in Passus 19/21, lines 12, 39, 134, 339 (x2). These figures are updated and corrected from Lawrence Warner, “The Ending, and End, of Piers Plowman B: The C-Version Origins of the Final Two Passus,” Medium Ævum, 76 (2007), 237–38. For discussion of many of these see Schmidt, Introduction and Textual Notes, pp. 453–65; Kane and Donaldson, eds., The B Version, pp. 155–58, 165, n. 86, 172, n. 92, 175; and Russell and Kane, eds., The C Version, pp. 119–27. I here include only those where the C archetypal reading is not in serious question; I omit for instance 18.424/20.467 carolden F, x family of C; dauncede beta, p family of C, and 19/21.63 his cros F, x family of C; cros beta, p family of C. These do include a few readings in which a single B witness far down the stemma agrees with F (e.g., 19/21.39 & fre OF, Cx; fre beta. 77 knowelechid GF, Cx; knowelichynge beta), though they exclude those where a single beta witness high up the stemma does, as in: 190 elles LF, Cx; elliswhere rest of beta, 4 C MSS. 216 kan LF, Cx; han beta. 249 pacience HmF, Cx; penaunce beta, D[H²Ch]. 306 pe lawe LF, Cx; lawe beta, DG. 20/22.6 coudist pou LF, Cx; Kanstow beta, MVAW.

⁶⁸ Hanna, “Invention,” p. 16. See previous note for the readings rejected by Schmidt; he thinks that the additions of & and the (B 19.39, 134), though, might not be errors (Introduction and Textual Notes, pp. 454, 455).

still equivalent to over 340 over the course of the poem. This rate is matched only by the most pervasively attested genetic groups, far more than those editors are willing to attribute to coincidence anywhere else, but which, they believe, occurred by coincidence only where MS R would one day become deficient. Likewise Burrow and Turville-Petre's approach, which at the least recognizes the problem, is forced to claim that F was thoroughly contaminated by C only where R, again by pure coincidence, would happen to lose a gathering at a later date.⁶⁹ This editorial dilemma, together with the fact that the rate of RF (alpha)-Cx agreements is maintained over the following passus,⁷⁰ prompted my recent argument that the alpha manuscript had been corrected against Langland's final revisions to C 21–22 (which had already been copied as B 19–20 by beta) before its use by the R and F scribes.

All three of those approaches to F's Passus 19 assume that it still attests alpha, as have Elsie Blackman and the editors of F for the Piers Plowman Electronic Archive (Hanna among them).⁷¹ In 2010, however, Ralph Hanna announced that F instead follows C where R is absent. The case goes like this. It begins with a rather leading reference to F's provision of "a steady stream of C readings," phrasing that announces his conclusion.⁷² Rather than

⁶⁹ The B-Version Archetype, ed. Burrow and Turville-Petre, pp. 31-32. They attempt to downplay the coincidence by citing "sporadic indications" of such contamination elsewhere (p. 31), by which they must mean those readings where F's "is also the reading of Cx" (their 5.17, 7.59, 8.43, 8.124). All of these, however, are easily explicable as standard coincident variation, and the editors use this same phrase, with no suggestion that it indicates C contamination, regarding other manuscripts: R 3.67, 8.6, 8.43; Hm 17.142; W 20.83; WHm 5.31; WHmF 11.140, 20.117. The one "notable instance" they cite is their 13.361-68 (p. 31), lines that "are in the first person, as they are in Cx where they are part of the confession of lechery (R[ussell]-K[ane].6.176.84). In [line 363] F's & summe y gan is a C-text reading and suggests contamination" (p. 239). And yet the passage in Bx begins one line earlier, in the first person: "I wayted wisloker . . ." (360), with F's partner R going on to add another I, "perhaps prompted by the alpha reading my for his in the next line," as Burrow and Turville-Petre say. As observed by F's PPEA editors, among whom is Turville-Petre, its scribe's change in pronouns in lines 362-63, 368 is "influenced by alpha"; they do not mention C (The Piers Plowman Electronic Archive, vol. 1: Corpus Christi College, Oxford MS 201 (F), ed. Robert Adams, Hoyt N. Duggan, Eric Eliason, Ralph Hanna, John Price-Wilkin, and Thorlac Turville-Petre [2000], note to F 10.353). See also F's change of pronouns in Prol.151, 13.393, and especially 14.82, and its shortening of phrases akin to summe for sum-tyme at, for instance, 13.385 F myse for rest mysdedes, 13.392 sprws for Prucelond; 14.114 meke for neiȝ meke (lines numbers in this sentence are those of The B Version, ed. Kane and Donaldson).

⁷⁰ See Warner, "Ending, and End," pp. 230, 238–39.

⁷¹ Elsie Blackman, "Notes on the B-Text MSS. of 'Piers Plowman,'" JEGP, 17 (1918), 502, n. 39, the first substantial discussion of its text; Corpus Christi College, Oxford MS 201, ed. Adams et al. (2000).

⁷² Hanna, "Invention," p. 16.

substantiate that conclusion and explain why these readings were not in alpha (whether by correction from C or via descent from the author's sole original), he points out that the argument for C 21–22's intrusion into Bx is hampered by its reliance on these F-Cx agreements, for "we essentially have no assurance that we are looking at readings of the archetype a usually shared by both books" and "cannot be sure that F may not be a representative of the C version, having divagated, for whatever reason, from its customary textual supply."⁷³ We will return to this surprising statement in a moment, but for now we might observe that this logic, insofar as it is valid, equally undermines any claim that F follows C, for we essentially have no assurance that we are looking at readings of Cx and cannot be sure that F may not be a representative of the B version. But back to Hanna's case: he next cites "some external evidence [that] might imply a very good reason for F's seeking an additional manuscript version here for its text": the "possibility ... that R and F determine a separate B-version archetype a precisely because the second is a copy constructed mostly by consultation of the former, along with other copies," and that R was lacking B 18.411–20.26 when F copied it.⁷⁴ After some comments regarding the difficulties this hypothesis would present to George Kane and E. Talbot Donaldson's approach to this passus, Hanna

⁷³ Hanna, "Invention," pp. 16, 16–17.

⁷⁴ Hanna, "Invention," p. 17. Wood says that here "the alpha reading cannot be known for much of the section in question" ("Langlandian Loose Leaves," p. 376, n. 22), and Simon Horobin, that R's deficiency "complicates the picture" regarding the pattern of F-Cx attestation, since in Passus 19 "evidence for RF relies entirely upon F, a manuscript demonstrably conflated with material from A and C elsewhere" (review of Warner, *Lost History*, in *Yearbook of Langland Studies*, 25 [2011], 207). Hanna's point, though, concerns not what alpha read but whether it existed. If it did, then according to my argument R's agreements with F would be confirmed as those of alpha; those with C against beta-F, or with beta against F-Cx, would be part of a pattern of such in which "the RF scribe recorded some or all of these new C readings as corrections to his manuscript, sometimes leading one of this exemplar's subsequent scribes (e.g., R) to attest the correction where the other (e.g., F) preferred the original" (Warner, *Lost History*, p. 47); and its unique readings would be erroneous. Pace Horobin, F is not demonstrably contaminated by C elsewhere. The only suggestions to the contrary are by Burrow and Turville-Petre (note 69 above) and Sean Taylor, who relies on a single reading capable of being interpreted otherwise: "F seems to reflect" C by reading "dey3neþ not vs to here," says Taylor (B 10.80; also C 11.59 JPEMFN² [not vs] vs nat XD; nou3t P²; vs YURVAQSZKGN]), where R reads "deyneth his heres to opne" ("The F Scribe and the R Manuscript of *Piers Plowman B*," *English Studies*, 77 [1996], 531, n. 7). The problem is that, as with Hanna's claim to have discovered intrusions into F by C, Taylor's single instance is where a major witness, in his case beta, is absent and so the reading might well be Bx's reading from which beta has strayed, as Kane and Donaldson have it.

announces his conclusion: “On the whole, outright consultation of a C manuscript, prompted by disruption of the scribe’s usual exemplar, seems to me most likely to account for F’s readings here.”⁷⁵ He does not explain the reasoning behind the pronouncement.

The absence of the apparatus or analysis that customarily accompanies such counterintuitive textual claims, the essay’s emphasis on the failings of Kane and Donaldson and its positing of my essay as the prime example of “forgetting Kane, failing to comprehend what [Hanna] should think the writings reveal that he meant (and had sometimes obscured),”⁷⁶ and his willingness silently to reject what would appear to be a fact that undermines his entire premise, might make one wonder whether Hanna is in earnest. The suspicion that this might indeed be a joke, a test of some sort, is only bolstered by the fact that in the Cambridge Companion to “Piers Plowman”, regarding “importation of C at a point where F’s source lacked eight leaves,” Hanna cites not his own essay, the only publication that has ever made such a claim, but mine, its target.⁷⁷ I have never encountered a scholarly note in this ironic mode before, but if such exists it surely wears its irony openly. Yet many critics have endorsed Hanna’s proposal, or at least praised the essay of which it is the centerpiece, which suggests that such doubts are not widespread. His co-authors on the Penn Commentary series, so Hanna claims at least, believe that Scase’s and my arguments “rest only upon an inability to conceptualize appropriately the vicissitudes of texts in manuscript

⁷⁵ Hanna, “Invention,” p. 18.

⁷⁶ Hanna, “Invention,” p. 8.

⁷⁷ Ralph Hanna, “The Versions and Revisions of Piers Plowman,” in The Cambridge Companion to “Piers Plowman”, ed. Andrew Cole and Andrew Galloway (Cambridge: Cambridge Univ. Press, 2014), p. 230, n. 8, pointing to my “Ending, and End.” Perhaps something went wrong in the production process, but this would entail accepting both that the correct reference disappeared and that “cf.” was replaced by “see” prior to the citation of my essay.

transmission”;⁷⁸ Wood cites his claim regarding F’s penultimate passus approvingly;⁷⁹ Kerby-Fulton calls it a “persuasive refutation” of my proposal;⁸⁰ Simon Horobin remarks on his authority (having unwittingly undermined it, however) that F “records a number of readings taken from the C Version”;⁸¹ Ian Cornelius cites the essay propounding it among those five, by only Kane, Adams, and Hanna, with which readers might begin to study the textual complexities of Piers Plowman;⁸² and Anne Middleton, whom Hanna thanks for reading the piece in draft, includes it among his work “on the textual and codicological forms of the poem, and their implications for understanding its chronology and methods of production” to which she is indebted.⁸³

Let us consider Hanna’s argument, both what it includes and what it leaves out, to see whether such endorsements rest on firm ground. The only “evidence” cited by Hanna—better characterized, perhaps, as a reason that would explain why F followed C rather than evidence that it did—is the possibility that F’s usual exemplar was R. One could wish as much

⁷⁸ Ralph Hanna, The Penn Commentary on “Piers Plowman”, vol. 2: C Passus 5–9; B Passus 5–7; A Passus 5–8 (Philadelphia: Univ. of Pennsylvania Press, 2017), p. xix, citing his own “Invention” and Robert Adams and Turville-Petre, “The London Book-Trade and the Lost History of Piers Plowman,” Review of English Studies, 65 (2014), 219–35, as ripostes (for a reply to the latter see Warner, “Impossible Piers”). The others writing for this series are Stephen A. Barney, Andrew Galloway, Traugott Lawler, and Anne Middleton. Middleton almost certainly belongs here (see, e.g., note 83 below), but Barney’s comments on my 2007 essay do not suggest any such belief (Penn Commentary, vol. 5: C Passus 20–22; B Passus 18–20 [Philadelphia: Univ. of Pennsylvania Press, 2006], p. 98). Galloway’s review of Lost History concludes: “Warner deserves credit for pursuing these possibilities in lucid, dynamic terms” (Choice, 49.3 [2011], 508; see above for his defense of Scase against Hanna). And Lawler’s review of that book observes that Hanna’s objection to my theory regarding the final two passus “does not, however, confront” what is “perhaps his major evidence for” it, concluding: “the careful argumentation is impressive, but I am not quite ready to see it as the only interpretation of the evidence” (Studies in the Age of Chaucer, 34 [2012], 447).

⁷⁹ Wood, “Langlandian Loose Leaves,” p. 376, n. 22.

⁸⁰ Kathryn Kerby-Fulton, “Confronting the Scribe-Poet Binary: The Z Text, Writing Office Redaction, and the Oxford Reading Circles,” in New Directions in Medieval Manuscript Studies and Reading Practices: Essays in Honor of Derek Pearsall, ed. Kathryn Kerby-Fulton, John J. Thompson, and Sarah Baechle (Notre Dame: Univ. of Notre Dame Press, 2014), p. 510, n. 5.

⁸¹ Simon Horobin, “Oxford, Corpus Christi College MS 201 and its Copy of Piers Plowman,” in Middle English Texts in Transition: A Festschrift Dedicated to Toshiyuki Takamiya on his 70th Birthday, ed. Simon Horobin and Linne Mooney (York: York Medieval Press, 2014), p. 26. See below, note 87 on Horobin’s undermining of Hanna’s assumptions, on which his claim is based, about the F scribe’s exemplar.

⁸² Ian Cornelius, Reconstructing Alliterative Verse: The Pursuit of a Medieval Meter, Cambridge Studies in Medieval Literature, 99 (Cambridge: Cambridge Univ. Press, 2017), p. 182, n. 9.

⁸³ Anne Middleton, “The Prologues and Ends of Piers Plowman A,” in Pursuing Middle English Manuscripts and their Texts: Essays in Honour of Ralph Hanna, ed. Simon Horobin and Aditi Nafde, Texts & Transitions, 10 (Turnhout: Brepols, 2017), p. 199, n. 1. See Hanna, “Invention,” p. 20, n. 40.

justification say that the possibility that F followed C constitutes “evidence” that its usual exemplar was R. Both are Hanna’s inventions. Whatever the case, this is impossible. As Blackman wrote a century ago in JEGP, “Certain errors of R, which do not appear in F, show that F is not descended from R,” eighty-one of which through Passus 7 Robert Adams has catalogued, to which can be added its straying from these correct beta-F terms in bold:⁸⁴

- 8.72 What art þou quod y **þo** þat þowh3 my name knowist] om. R.
 10.151 So 3ee **kenne me** kendely to knowe what ys Dowel] om. R.
 13.120 I have **sevene** sones he seyde þat seruyn in a Castel] om. LR.
 13.187 **þat** Pacyense þat pylgrym parfy3tly knewh **neuere**] om. R; euerre HmR.
 13.226 I am a waferer **wil** 3ee wete & worshepe manye lordis] well Cr¹²R.
 14.67 þat no reyn rey3nede þus men **rede in** bookys] rett ... on R.
 14.72 Amongis cristene creaturis if **cristis** wordys þei take] criste R.
 14.73 But welthe is so myche a maister a-mongis **cristene** peple] cristes R.
 15.13 Oon with-owtyn tunge or teep told me **whidir** y sholde] wonder R.
 18.39 & alle **þe** cowrt on **hym** criede crucifige ful sharpe] her LR; iesu R.
 20.123 & he armed hym in Auerise & **vngryly** he lyvede] vngriseliche R.⁸⁵

It was Sean Taylor who, based on the strong similarity between the F scribe’s hand and two small entries into R, first proposed that F followed R. He takes the fact that “Blackman does not enumerate the errors in R that do not appear in F” as justification not to engage with the problem.⁸⁶ Neither does Hanna mention it, though he was among the editors of F who pointed out that Taylor’s proposition “is consistent with neither the evidence of textual variation

⁸⁴ Blackman, “B-Text MSS.,” p. 502; Robert Adams, “The R/F MSS of Piers Plowman and the Pattern of Alpha / Beta Complementary Omissions: Implications for Critical Editing,” TEXT, 14 (2001), 131; list at 131–32. All beta-F lines are given in F’s version, cited from Corpus Christi College, Oxford MS 201, ed. Adams et al. F very often diverges from beta in these lines for the readings in these lines not at issue, of course; at 14.73 its a-verse is unique, for instance.

⁸⁵ F’s vngryly is a spelling variant of beta’s hungriliche (vngreliche C, honglelich Y). As Adams observes, “R’s form is not found in any C manuscript; moreover, the sole attestation for the word in MED, s.v. ungriseliche, is from this passage. MED offers a possible gloss of ‘Not hideously, sumptuously’ but also notes that it may be merely an error for the commonly attested form, hungriliche” (The Piers Plowman Electronic Archive, vol. 7: London, British Library, MS Lansdowne 398 and Oxford, Bodleian Library, MS Rawlinson Poetry 38 (R), ed. Robert Adams [2011], note to the line). So it is not impossible that the F scribe, if R’s form was in his exemplar, would have understood it and normalized the spelling, but the situation is usually reversed, and this is one of R’s odder spellings in any case.

⁸⁶ Taylor, “The F Scribe,” p. 530.

between the two witnesses nor the dialectal history of F.”⁸⁷ He does not offer an explanation for his new rejection of this body of data.

But this “is scarcely the only plausible explanation that might be offered” for F’s provision of C material, Hanna continues, his rhetoric seeming to suggest that the enumeration of explanations provides a sufficient substitute for evidence that it occurred: “R itself may be a page-by-page reproduction from a, in which case the archetype itself might either have been missing a quire, when F received it, or could have included a quire’s worth of C materials.”⁸⁸ In regard to the first possibility, Adams points to codicological evidence that “easily accounts for” the loss from MS R itself of the quire containing B 18.411–20.26, and states unequivocally that the leaves including this matter “were detached from R,” not its exemplar, “and lost long ago.”⁸⁹ We cannot know for sure that the removal of the first quire from the book—its inner two folios are now folios 77–80 of London, British Library MS Lansdowne 398; the rest is lost—is related to the absence of the putative quire that attested 18.411–20.26, but this phenomenon both proves that the book was unbound at some point

⁸⁷ Corpus Christi College, Oxford MS 201 (F), ed. Adams et al., section on “The Scribe” in the Introduction. This argument against Taylor is endorsed by Horobin, “Corpus Christi College MS 201,” p. 23 (see pp. 21–23 and see note 81 above for his endorsement of Hanna’s argument that F follows C here). Hanna acknowledges Taylor and reports that “[r]ecently, in the course of his Oxford D.Phil. researches, James Wright has reaffirmed Taylor’s view of the identity of notes in R with the hand of F” (“Invention,” p. 17, n. 36). Yet as Adams has written, “the immediate F scribe may well, at some unknown time, have had R in his possession and gone to the trouble of altering (or ‘repairing’ as he might have thought) the ... rubric. But this is a far remove from Taylor’s inference that R was his exemplar for copying F”; Adams rejects this inference on the grounds of R’s unique errors, “the Norfolk relict layer apparent in F but not present in R, which would imply, at the least, that some ancestor of F, rather than F itself, may have consulted R,” and the difficulty of understanding “why the F scribe would have bothered to ‘correct’ R’s Passus 8 rubric to reflect the more conventional four-part segmentation of the poem seen in copies like W at the same time that he was creating from scratch, for his own copy (or duplicating from an unknown conflational source), an entirely distinctive set of passus divisions that bears no resemblance to R’s pattern or W’s” (Adams, ed., MS Rawlinson Poetry 38 (R), Introduction, II.2.4, “R’s Relationship with F”).

⁸⁸ Hanna, “Invention,” p. 17.

⁸⁹ Adams, ed., Rawlinson Poetry 38 (R), Introduction, I.4, “Collation” (“easily accounts”) and note to R.18.422 (“detached”). Here is the codicological evidence: “Of the complete quires, only #2 and #13 lack boxed catchwords; and cropped letters at the bottom of fol. 8v indicate that catchwords were originally present in quire #2. They were presumably also present on fol. 95v. Comparative measurement of distance between the top of the last text line and the bottom of the leaf for these two pages indicates that the fol. 95v margin is now considerably smaller than that of fol. 8v (6.9 cm versus 7.6 cm). This difference easily accounts for the missing catchwords at the end of quire #13, and their absence may have caused the loss of quire #14 [which contained 18.411–20.26] during binding or rebinding” (I.4, “Collation”).

and provides a simple explanation for the disappearance of the latter. Finally, Hanna's suggestion that alpha "could have included a quire's worth of C materials" is like my own proposal in that both involve two early scribes using the identical exemplar, including Passus 19, which was altered between their respective copyings. I assume that his recent mockery of critics who speculate along these lines indicates his disavowal of this possibility.

The main reason none of this constitutes "evidence," though, is that, if indeed the F scribe was forced to source 18.411–20.26 from elsewhere, that source can only have been B, but not in its beta form. It can only, that is, have been alpha—which is what he was already following before and after the material that would go missing from R. There is no basis for Hanna's initial remark that one cannot be sure that F is not following C for this passage. All one has to do is to "examine F's behaviour critically, as one imagines textual critics are to do" (Hanna's barbs aimed at Kane and Donaldson).⁹⁰ To turn to that task: if the fifty-eight F-Cx agreements were the only sites of divergence between B19 and C21, then Hanna's conclusion that F followed C here might on its face seem reasonable, though it would have helped his case a bit if they were not, as I earlier quoted from his essay, "very frequently readings more persuasive than all other B copies." (Of course, even striking agreements with Cx, if such existed, could have come into F from alpha.) As it happens, the five F-Cx readings that Schmidt follows the Athlone editors in rejecting, though, are of a trivial character: the additions of & and the are possibly authorial (19.39, 134); line 12, "Quod conscience & knelede doun þese are **crístis** armes" (beta: Piers), is probably substitution to be "explained as by inducement of the alliteration or of" the following lines, "He ys críst with his cros ... Why calle 3ee hym críst" (ll. 14, 15); and those in line 339, "& sentyn forþ

⁹⁰ Hanna, "Invention," p. 16.

surquidoures were **sergawntys** of armes” (beta: “Surquidous his sergeaunt”), are among those F-Cx variants “of a character likely to have occurred coincidentally.”⁹¹

Let us look as well at the remainder of the relevant evidence, only a portion of which appears in Hanna’s essay. F also has a steady stream of B readings, in the form of forty-six agreements with beta against Cx. Both Schmidt and the Athlone editors find errors in sixteen of these beta-F agreements,⁹² a few of which are extensive, such as these two doozies (all readings here from B19/C21; terms in bold are those that diverge from Cx):

253	That all craft and connyng cam of my ʒefte	Cx
253	Pat he þat vseþ fayr craft to þe foulest y cowde a pyt hym Pynkeþ alle now quod grace þat grace comeþ of myn gifte	beta-F ⁹³
369	And a sisour and a sompnour þat weren forsworen ofte	Cx
369	& false men & flateris & vsereris & þevis Lyeris & qwest-mongeris þat ben for-swore ofte	beta-F

But we should not place too much stock in the question of whether beta-F agreements are erroneous by conventional standards. One of the sixteen lines in which both Schmidt and Kane-Donaldson identify fault shows why:

251	Ne no boest ne debaet be among hem alle	Cx
251	& forbad hem Debate þat noon were among hem	beta-F

⁹¹ On those in lines 39 and 134 as possibly authorial, see above, note 68. The quotation about line 12 is Russell and Kane, eds., The C Version, p. 123; for other possible explanations see Burrow and Turville-Petre, eds., The B-Version Archetype, note to their line 12; and Schmidt, Introduction and Textual Notes, p. 453. The quotation about line 339 is Russell and Kane, eds., The C Version, p. 122; on p. 123 they say the F-Cx reading “literalizes the personification allegory as earlier in the poem”; see also Schmidt, Introduction and Textual Notes, p. 460. I cite F-Cx agreements in F’s text, from Corpus Christi College, Oxford MS 201 (F), ed. Adams et al.

⁹² These occur in Passus 19/21 lines 97, 180, 229, 243, 251, 253, 298, 301, 303, 314 (x2), 343, 369, 429, 437; Passus 20/22 line 19. For beta-F doost in line 180, MS O of the beta group agrees with C’s seest; it is included here because, as Burrow and Turville-Petre observe, doost “must be the reading of Bx,” O’s agreement with Cx attributable to its scribe’s conjecture (The B-Version Archetype, note to their line 184). Kane and Donaldson discuss all save those in lines 180 and 229 in The B Version, pp. 90–95; Russell and Kane discuss those in lines 253 and 298 in The C Version, pp. 120–21; Schmidt discusses many of these in Introduction and Textual Notes, pp. 455–63.

⁹³ F adds four more unique spurious lines between these two lines. Its now is unique, and for its fayr beta reads þe fairest. Cx readings are sourced from The C Version, ed. Russell and Kane, in MS X’s spelling (once or twice MS X, the copy-text, errs from Cx).

Since Kane and Donaldson take C 21–22 to be unrevised from B 19–20, only one of these lines can be correct, and they choose Cx. Schmidt, though, finds revision here; it is on the list of errors solely because he rejects beta-F’s were in favor of Cx’s alliterative be.

But if F was in fact copying C, as Hanna posits, its line is suddenly wholly in error, as are the twenty additional beta-F errors that Kane-Donaldson identify, all but one of which are cases where Schmidt accepts both of the diverging B and C readings as authentic to those respective versions.⁹⁴ The issue is not whether Langland could have written this beta-F line:

230	To wynne with treuthe þat the world asketh	Cx
230	& wit to wynne here lyfloode [wip] as þe lond askep	beta-F ⁹⁵

but rather how, if F replaced B 18.411–20.26 with its C equivalent, it came to agree with beta against C here and at forty-five other lines, including at every site of substantial divergence between B and C. The same goes for beta-F’s agreements against Cx for a further ten readings for which the Athlone editors deem Cx in error (Schmidt accepts both the B and C readings for all except 373 as below where he grants Cx’s error).⁹⁶ Here are the two most substantial instances:

239	As here wit wolde when þe tyme come	Cx
239	To wynne with here lyfloode by loore of his techyng	beta-F
373	[zero]	Cx
373	Save shrewis oonly & swiche y spak of to-forehond	beta-F

⁹⁴ These are in Passus 19/21, lines 15, 101, 111, 148, 164, 174, 197, 230, 238, 241, 254, 271, 280, 292, 308, 335, 357, 362, 375; Passus 20/22, line 25. Kane and Donaldson discuss line 15 in The B Version, p. 152; line 238, p. 174; and all others except 111, 230, 241, and 375 on pp. 90–95; Russell and Kane discuss 101, 148, 271, 280, and 292 in The C Version, pp. 119–20. For all but 241, where he emends C to follow B, Schmidt allows both B and C readings to stand; see discussion in Introduction and Textual Notes, pp. 453–65. One member of beta agrees with Cx for a few of these: 111 lyf of] HmCrGYOC²CBLMF, lyf W, Cx (lyf of “seems clearly the reading of Bx”): The B-Version Archetype, ed. Burrow and Turville-Petre, note to their line 113); 375 thorw] WHmGYOC²CBLMF; by Cr, Cx (thorw is “secure for Bx” [note to their line 387]).

⁹⁵ F alone among B witnesses omits wip after lyfloode.

⁹⁶ In addition to the two about to be quoted (239, 373; all in B.19/C.21), these are in lines 42, 63, 241, 255, 267, 363, 387, 423. Russell and Kane discuss 42, 63, 363, 373, and 387 (The C Version, pp. 121–22, 125).

For the claim that F substituted a C text for B 18.411–20.26 to stand, these beta-F agreements need to be convincingly explained as instances of convergent variation. That is to say, this group beta-F, WHmCrGYOC²CBLMF, needs to be proven random. Hanna attempts to go about that process thus:

I conclude this demonstration with a further point revealed in analysing the behaviour of F through B passus 19. ... [Kane and Donaldson's] discussion relies extensively upon the evidence of B.19, and it reveals twenty-five occasions when F concurs with the B archetype in a reading revealed as erroneous, when compared with readings of C. In context, one might assume this to mean that the scribe of F had access to more than one textual version.

However, closer examination throws up yet another example of those paradoxes I have considered through this essay. Kane and Donaldson's comparison relies upon juxtaposing scribal B readings—and they are universally perspicacious in having rejected them—with readings of the edited (and scribally purged) C version. Yet examination of the mass of C variants Russell and Kane provide reveals that 60 per cent of the examples of universal B error (fifteen of them) are reflected somewhere in the C tradition, sometimes very widely so.⁹⁷

In a note he explains what these instances of coincident substitution mean: “The presence of identical readings in the C tradition indicates that in the majority of instances, F's conflation from C may accidentally converge with the remainder of the B tradition. The ten remaining readings, given the total number of variants quite a modest showing,” are “capable of being explained either as convergence or spot-consultation of a B manuscript.”⁹⁸ He concludes the note by kindly thanking me for bringing his attention to the twenty-five F-beta errors he cites. Now, Hanna is absolutely right that, if F indeed follows C, its agreements with beta must be either coincidental or the products of consultation of another B copy. The logic above is based on the premise that he is concluding a “demonstration” to that effect. And yet there are three substantial difficulties, most obviously that he has not demonstrated that F followed C,

⁹⁷ Hanna, “Invention,” p. 19. These are (Cx] C MS agreeing with beta-F [brackets] indicating genetic groups): 97 comsed] gan to N (bygan). 164 to] for to FN. 174 zero / That] Thow [VA]N². 197 to] to the [(PE)(RM)]. 238 teche] dyche D. 254 as] alle as F. 271 folwede] folwen / folweþ [GN] (folweþ). 298 euene] euer D²D[RM]NN². 335 plow] plow and N². 343 that sire] that [TH²Ch]. 357 Preye] And preye [TH²Ch]WN. 375 bi] þoruþ P² (Cr agrees with Cx). 429 soudeth] sendith D²D[TH²Ch]. 437 So yblessed be Peres the plouhman þat peyneth hym to tulie] om. [TH²Ch] (F alone + [TH²Ch] also omit 435–36).

⁹⁸ Hanna, “Invention,” p. 19, n. 39.

or even suggested any reasons why anyone should accept that claim. That alone, in my judgment, should put an end to the discussion, but the widespread embrace of Hanna's proposal suggests a need to take a few paragraphs to sort out the second problem: that the level of convergence Hanna posits, even accepting for the moment that this is the extent of it, is unprecedented.

For these beta-F readings ought to have alerted him that F is a B text. The fact that some C readings converge to B has nothing to do with the issue; that happens everywhere and even if it did somehow call into question the characterization of WHmCrGYOC²CBLMF as a genetic group, one can only say that it is F that converges with WHmCrGYOC²CBLM by begging the question. A disinterested approach would allow that it might be, say, W that converged to HmCrGYOC²CBLMF, or O to WHmCrGYC²CBLMF, and so forth. The issue, in other words, is not that FN of C join beta-F of B in reading for to rather than to, but whether it is beta-F that is the genetic group, with the C agreements being the result of convergent variation to beta, or rather F-Cx that is the group, the F-beta agreements being as Hanna describes above. If a group's agreements display "relative persistence of agreement, distribution of agreement, and the congruency of variational groups presumed genetic," it is genetic; if not, it is random.⁹⁹ Relative persistence has it that group XY with 100 agreements in error is much likelier to be genetic than group XZ with fifty. In our situation, the figures are forty-six beta agreements and between zero (if they were in alpha) and fifty-eight (if Hanna's speculation is accurate) Cx ones. As for distribution, F's agreements with both groups occur throughout the text in question, while F also agrees with beta for readings in error everywhere else in the poem as well, including where R is deficient early on. Finally, the criterion of congruency has it that "in the case of two variational groups WX (50 agreements) and XY (50 agreements), all other things being equal, if WX form an element of

⁹⁹ Kane and Donaldson, eds., The B Version, p. 19.

a persistent larger group UVWX and XY do not occur in any larger group, there seems a probability that WX is genetic and XY random.”¹⁰⁰ The group WHmCrGYOC²CBLMF forms an element of the larger WHmCrGYOC²CBLMRF, but the group F-Cx is nowhere else attested.

Hanna points to a few readings that might possibly provide evidence that F continues to have access to a p manuscript after it has returned to alpha, but it can do so only if it is already following C, which is a different matter altogether, and which, it bears keeping in mind throughout our discussion, he never attempts to establish.¹⁰¹ This silence is not very surprising, since R’s deficiency means we cannot assume that any sign of C’s influence on F’s text—of which I am convinced there is a great deal, that is to say, the bulk of those fifty-eight readings—is a sign of influence on MS F itself. So we are here in the odd position of considering whether F followed C in the absence of any indication that can be confidently taken as evidence that it did, and in the presence of massive evidence that it did not. The notion that F stayed with its alpha exemplar encounters no problems. The C agreements were in that exemplar, as they are as well in alpha for the rest of the poem. By contrast, if F followed C then these forty-six beta readings, especially the ones quoted above, are suddenly a serious problem, forcing such acrobatics as when Hanna appeals to a few C copies’ convergence to beta-F for such readings as addition of terms and or alle, his remarkable and necessary concession that the F scribe did in fact have a B copy on his desk, and the like. Neither can this proposal explain RF’s agreements with Cx in the final passus. I see no viable alternative to the conclusion that F attests B 18.411–20.26.

¹⁰⁰ Kane and Donaldson, eds., *The B Version*, p. 20, n. 17.

¹⁰¹ Begging the question, he says that “such F dips into the C tradition do not end with the return of R (and some certainty about the readings of a),” and these “dips” include one reading “restricted to the p manuscripts” and several variants, of which he cites one, with N²W, which however is not a genetic group (Hanna, “Invention,” p. 18).

For what it is worth, the statistics Hanna pulls out to bolster the case for F's convergence to beta are far off the mark. It is true enough that 60 percent of the beta-F errors discussed by Kane and Donaldson are attested somewhere in the C tradition, but, to assume for a moment the relevance of this phenomenon, that is not the body of data at issue. We are talking instead about beta-F errors in toto, of which 39 percent, fourteen of thirty-six, appear somewhere in C. If we include the remaining beta readings that are by definition "errors" if F is following C, the percentage falls slightly to 35 (sixteen of forty-six).¹⁰² And where he says he need only account for ten remaining, "quite a modest showing," the accurate number is thirty. This is by way of identifying the third serious problem with his argument: his silence, for whatever reason, about half the evidence. As we have seen, he cites, at my prompting, twenty-five beta-F errors, two of which, however, I do not include as it is not clear that C in fact diverges from B.¹⁰³ But he mentions neither the remaining thirteen beta-F errors identified by Kane-Donaldson nor the ten F-Cx agreements where Kane and Donaldson take Cx to err, even though two of those are more substantial than nearly all of the errors. These are the very readings on which any conclusion must rely, but rather than analyze them critically Hanna expends his efforts on scolding George Kane for not analyzing them critically, "as one imagines textual scholars are to do," accusing him of being "downright misleading" in his treatment of F, and setting my argument up as an example of "forgetting" Kane.¹⁰⁴

To sum up my findings, which are really those of the Athlone editors. The HtJ passages are members of neither the x nor p families of the C version and pair with no extant

¹⁰² Of the ten F-beta agreements for correct readings (eight listed in note 96 above plus two in main text thereafter), two appear in C: 42 for] to [H²Ch]. 267 And sethe] And beta-F; E.

¹⁰³ On page 19 of "Invention" he cites twenty-five beta-F errors as discussed by Kane-Donaldson (see following), but I omit those in B19/C21 lines 453 and 477, cited in note 39 on that page. These are the ones to which Hanna refers in saying, as quoted above, that "sometimes" F-beta erroneous readings "are reflected somewhere in the C tradition, ... very widely so."

¹⁰⁴ For "downright misleading," see Hanna, "Invention," p. 19.

witness for more than a few readings, and so are not witnesses to the received C text. Neither P²OLB nor P²OLBHtJ of C make up genetic groups. WHmCrGYOC²CBLMF is a genetic group in B19. F-Cx is not a genetic group, anywhere. The ability to say the opposite about each of these cases does for Ralph Hanna and Sarah Wood what that loose leaf with thirty-one lines a side did for J. M. Manly: it serves as a massive escape hatch. Yet let no future critic dismiss the concept of convergent variation as the purview of those tainted with fantastic notions about Middle English manuscript transmission akin to those of J. M. Manly. Nor should anyone accuse Hanna, Smith, Wood, or Burrow and Turville-Petre, on account of the centrality of loose sheets and quires in their respective explanations of HtJ's, F's, and Bx's texts, of having been "seduced" or "distracted" by the "allure" of the loose sheet. That concept is neither erotic nor inherently fantastic. Hanna and Wood are not really talking about whether a given piece of parchment was sewn in or not, but whether the material on it descends from the archetype.

While this essay is not about loose sheets, it is very much about convergent variation, without a basic grasp of which one cannot understand texts like MS F or P²OLB. While it is rather startling to find how little impact that phenomenon, properly understood, has had in the most important recent essays in the field, we can take comfort in knowing that students of Langland have by far the most comprehensive demonstration and explanation of the phenomenon, indeed the only full-scale such study, at their fingertips in the Introduction to Kane's edition of the A version.¹⁰⁵ There are good grounds for hoping that future critics will be able to resist the allure of the invented "loose leaf" controversy of late 2016 and return to responsible engagement with the relevant topics as they determine whether, in light of the evidence of HtJ and F, the Piers Plowman we thought we knew can any longer stand up.

¹⁰⁵ Kane, ed., The A Version, pp. 53–114.