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The Anachronistic Shrews

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of explication and emendation around this crux demonstrate how profoundly textual criticism remains a project of foreclosing ambiguity and doubt in the service of a definitive text.

“Sincklo” refers to the actor John Sincklo or Sincler, a member of the Lord Chamberlain’s Men, whose name appears in several other dramatic texts.² “Sincklo” appears as one of the gamekeepers in 3.1 of the Folio’s 3 *Henry VI*, as a beadle in the 1600 quarto of 2 *Henry IV*, and in the Induction to Marston’s *Malcontent* in 1604 (where Sincklo performs a bit of metatheater with his senior partner William Sly). His name also appears in the manuscript plot of *The Second Part of the Seven Deadly Sins*, now preserved in Dulwich College, where one of his roles is again a “keeper” who interacts with King Henry VI.³ Sincklo’s appearance in *The Seven Deadly Sins* plot has become a key piece of evidence for assigning *The Taming of the Shrew* a very early date. Sincklo never appears in lists of principal players or on the company’s royal patents; nor does he figure in any of his colleagues’ surviving wills. Some have proposed that “dem kleinen engelender Dinckenclo,” who received a payment from the Landgrave of Hesse in 1596, was Sincklo;⁴ if so, his only appearance in a household payment book gets his name wrong.

There are records of at least two John Sinclers, Sinckleys, or Sinclairs in London during the relevant period; at least two women (Elizabeth and Mary) recorded as the wife of John Sincler, Sinckley, or Sinclair; and a widow named “Bettrice Sinckloe” who might be the aforementioned Elizabeth, but whose husband’s personal name is unknown.⁵ None of the court or tax records ties any of these Sinclers to the profession of playing, or to any other figure from the London theaters. The obvious inference is that “Sincklo” was not a sharer but a hired player, too low in the company hierarchy to be listed in royal charters or named as a principal player. This fits with the handful of minor roles to which he can be conclusively linked. Some scholars have tried to cast Sincklo in the company’s other plays, based on jokes about his stature in 2 *Henry IV*, but those attempts are built more on supposition than on fact. The only evidence for John Sincklo’s acting career is preserved in the margins of the plays themselves.

“Soto” evidently refers to a character in Fletcher’s *Women Pleased*, a servant of that name who dresses in his master’s clothing in an abortive attempt to woo a

² Some scholars give preference to the spelling “Sincler,” but I will use “Sincklo” in this essay, as this spelling or some phonetic variant of it consistently appears in contemporary printed texts.

³ See the First Folio, 3 *Henry VI*, TLN 1396–1499; *The Second Part of Henrie the Fourth*. . . (London, 1600), sig. K3v; and John Marston, *The Malcontent*, in *English Renaissance Drama*, ed. David Bevington et al. (New York: W. W. Norton, 2002), 545–613, ll. 17 sd–135. The plot of *The Second Part of the Seven Deadly Sins* is Dulwich College MS 19.

⁴ Mark Eccles, “Elizabethan Actors IV: S to End,” *Notes and Queries* 238 (1993): 165–76, esp. 168.

⁵ Eccles, 168–69.

gentlewoman, Lady Belvidere, on his master's behalf. The reference in *The Shrew* is singularly apt, although its aptness has gone unremarked: the Lord, planning his trick on Christopher Sly, recalls another moment of class-conscious travesty, with another clownish character failing to sustain an aristocratic disguise with appropriate bearing. The players respond to this prompt by offering—as the main action of *The Shrew*—an entertainment with a successful class disguise, with Tranio impersonating Lucentio in order to woo on his master's behalf. However, this bit of intracompany intertextuality links the manuscript underlying the Folio text to a play whose cast list suggests a date between 1619 and 1623, long after the date most scholars seek to fix for *The Shrew* and clearly after William Shakespeare's death. (There is also a character named Soto in Middleton and Rowley's *Spanish Gypsy*, but this play is even later than Fletcher's and so presents the same complications in dating the Induction of *The Shrew*.) Few critics have been eager to pronounce the manuscript underlying *The Taming of the Shrew* a posthumous text. Moreover, a late Jacobean date for the printers' manuscript undermines claims that the Folio text precedes the 1594 quarto text.

Inconveniently, the reference to Soto comes in the one line ascribed to Sincklo, a speech heading used as evidence for an early date and often, but mistakenly, for a date of 1592 or earlier. The actor whose presence is taken for an early provenance speaks evidence for a later one; the speech has an apparent *terminus a quo* much later than the speech prefix's *terminus ad quem*. This moment in the Folio text derives from some period after the King's Men began acting Fletcher's *Women Pleas'd* but before the end of Sincklo's performing career. Using the currently accepted ranges of dates proposed for those events, this means sometime after 1620 but before 1606 or so. Proper respect for the evidence means some of these accounts must be revised.

II. "TWO ALTERNATIVES"

The mainstream approach to the Sincklo/Soto problem was established by Arthur Quiller-Couch and John Dover Wilson in their 1928 New Cambridge Shakespeare *Taming of the Shrew*, the first edition of the play to promote Peter Alexander's argument that *The Shrew* preceded *A Shrew*.⁶ In part, Quiller-Couch and Dover Wilson work by selectively decoupling the spoken name from the speaker's name. They freely discuss Sincklo, whom they use to support an earlier date, without raising the issue of Soto—but Soto is only discussed in conjunction with Sincklo. The name construed as a sign of "earliness" is not undermined by the "lateness" suggested by the other name; instead, the evidence of lateness is strictly controlled and limited by

⁶ Arthur Quiller-Couch and John Dover Wilson, eds., *The Taming of the Shrew* (Cambridge: Cambridge UP, 1928), 114–15.

the other name's purported earliness. Sincklo's name is treated as evidence in its own right, and Soto's as merely potential evidence, as far as Sincklo's name permits. Quiller-Couch and Dover Wilson use Sincklo as Soto's discursive chaperone.

This one-way uncoupling of Sincklo from Soto is abetted by the standard editorial practice of emending speech prefixes, so that Sincklo's name does not appear in the text itself, and the two proper names can only be joined in the paratext (where they are always, of course, more than a scant three syllables apart). Although Sincklo is banished from the text, he is prominent in the Cambridge editors' textual note, where the bare fact of his name is marshaled as evidence. Soto's name appears nowhere in Quiller-Couch and Dover Wilson's extended note on the text or in their introduction; the name is excluded from their formal discussion of dating and provenance and instead exiled to the endnotes. Quiller-Couch and Dover Wilson do not withhold information about Soto, but provide it only to readers following up on the reference. Their note calls the name "important, if puzzling . . . as regards stage history,"⁷ but apparently they do not rate its importance highly enough for anything more than a marginal discussion.

When Quiller-Couch and Dover Wilson do at last turn to Soto, they are acutely and explicitly aware that the name's obvious reading works against their thesis and even that "Sincklo" is a relatively weak guarantor of an early date. "[T]he reference to Soto," they write, "points at first blush to a play of Fletcher's which is dated as late as 1620," and they admit that "if the evidence we have about him were taken at its face value" Sincklo "would have been on the stage for over 30 years."⁸ The goal of their explication, then, is to exchange the evidence at hand for something other than its face value.

The New Cambridge editors' influential but peculiar solution is to supply two mutually exclusive arguments without actually choosing between them, although they favor one over the other:

The passage, therefore, leaves us with two alternatives: either (i) that ll. 82–87 are an insertion (they could, in fact, be omitted without any injury to the text) made after Shakespeare's death at some revival of *The Shrew* shortly before the publication of F; or (ii) that *Women Pleased* was itself based upon an earlier text belonging to Shakespeare's company and that Sincklo was playing Soto in this earlier version sometime about 1591–2. We incline to the second alternative.⁹

⁷ Quiller-Couch and Dover Wilson, eds., 131.

⁸ Quiller-Couch and Dover Wilson, eds., 131–32.

⁹ Quiller-Couch and Dover Wilson, eds., 132.

Quiller-Couch and Dover Wilson's "two alternatives" remain the standard gloss on Soto's name today. The *Norton Shakespeare* footnote on Soto, almost identical to that of the *Riverside Shakespeare*, illustrates the persistence of Quiller-Couch and Dover Wilson's dual explanation: "The reference must be a late addition to Shakespeare's text or else refer to a character in an earlier play, now lost."¹⁰ Most editors still propose both of the now-traditional hypotheses at once, although H. J. Oliver (about whom more below), argues exclusively for the Cambridge editors' preferred second alternative.¹¹ The appeal of the two alternatives, however diametrically opposed their internal logic, seems to lie in the combination itself. These arguments seem more influential as a pairing than as arguments per se.

Although Quiller-Couch and Dover Wilson clearly prefer one hypothesis to the other, they are not confident enough in their hypothetical earlier text to stake the play's date and provenance upon it. Their reluctance is perfectly understandable, since there is no evidence that any such text existed, unless one counts as evidence the very problem that the hypothesized text is meant to explain. The editors suggest that the allusion is not to a known play, but to a lost play (similar in many respects to the known play) to which no reference survives except for the case in question. This is hypothesis as scholarly wish, imagining a piece of additional evidence one would like to have in order to make a case one would like to make. And while this specific hypothesis of convenience has now become a traditional gloss, such hypothesizing is not widely accepted as editorial practice. Could, for example, a scholar finding a reference to Shylock in the text of an early modern play but hoping to date that play before *The Merchant of Venice* postulate a hitherto-unknown source play for *The Merchant of Venice*, with a hitherto-unknown *Ur-Shylock*, whose existence is corroborated only by the reference in the undated play itself?

One should not even begin to speculate about lost *Ur*-texts as the Cambridge editors do until one knows, beyond reasonable dispute, the date of the reference upon which the speculation is built. But Quiller-Couch and Dover Wilson know nothing of the kind. In 1928, the date of *The Shrew* was not even a matter of scholarly consensus, let alone incontrovertible fact. Indeed, it was a question which had newly been thrown into dispute, and the New Cambridge edition was actively furthering the controversy by championing the upstart position. The previous orthodoxy took the publishing chronology of the 1594 and 1623 texts at face value, presuming that order of publication reflected order of composition unless positive evidence proved otherwise. Peter Alexander's argument that the 1623 Folio text preceded the 1594 quarto was still an "explosive suggestion"

¹⁰ Greenblatt, gen. ed., 144n6.

¹¹ H. J. Oliver, ed., *The Taming of the Shrew* (Oxford: Clarendon Press, 1982), 5.

to Quiller-Couch and Dover Wilson, so recent that they write of Alexander making the argument "the other day."¹² The very early date proposed for the Folio version was not something everyone knew, but something a few energetic people claimed. The idea of quarto texts as derivative reconstructions was still far more of a minority position, and indeed more of a novelty, than the so-called "disintegrationist" approach which preceded the New Bibliography. The New Cambridge editors cannot use an established date of the Sincklo/Soto crux to argue the logical necessity of an early Soto play. Instead, Quiller-Couch and Dover Wilson need to protect their conclusions from the most obvious implications of Soto's name. They keep Soto entirely out of the discussion on dating, and then approach the problem of the *Women Pleased* reference as if the question of the date were already established. The fact is made to fit the theory.

Quiller-Couch and Dover Wilson hedge their fairly shaky bet by leaving open the possibility of a late insertion. If an *Ur-Women Pleased* does not seem persuasive, the editors are willing to concede exactly five lines of revised verse later than *Women Pleased*. The reason for the concession is perhaps obvious; the limit placed upon the concession is merely reflexive. Quiller-Couch and Dover Wilson trade in exceptions here; the five-line "insertion" is presented as an exception to the otherwise integral text. The possibility of "a late addition to Shakespeare's text,"¹³ in the Norton edition's phrase, and the potential for contamination by other agents are strictly confined to the single, otherwise inexplicable, instance. Why a play that is imagined as being revised around 1620 would be altered in only one place, for five trivial lines, is not explained. Rather, the urge to enclose the revised text as closely as possible arises from an unspoken principle: that Shakespeare's authority is to be assumed everywhere that it is not manifestly disproved. A reference to a play written after Shakespeare has died may be conceded as a single posthumous addition, but the rest of the play is tacitly presumed to be free of tampering. The reasoning here cannot withstand scrutiny; the argument works backward from the desired conclusion, rather than forward from the evidence.

Both hypotheses present serious difficulties: the late insertion theory might lead readers to seek other posthumous revisions, and the lost play theory is not much more than a wishful tautology. So Quiller-Couch and Dover Wilson present both theories together, joined in prophylactic uncertainty. The frailty of each argument is cushioned by the existence of the other; as long as neither is dismissed, both can survive, and further speculation can be avoided. Refusing to commit to one explanation of the Soto remark relieves critics from the need to defend the explanation they have chosen. If the critical discourse is carefully

¹² Quiller-Couch and Dover Wilson, eds., xiii.

¹³ Greenblatt, gen. ed., 144n6.

structured as a choice between rival explanations, any exposed flaw in one hypothesis can be presented as implicitly strengthening the others. Naturally, it does not follow that the weakness of one proposition strengthens another, but framing the crux as a binary choice preserves the unexamined and unwarranted premise that only two alternatives exist. As long as the critical focus remains on choosing between Quiller-Couch and Dover Wilson's two problematic explanations, speculation about other possibilities, including that of taking the evidence at what Quiller-Couch and Dover Wilson call its "face value," is foreclosed.

Editors' discussions of the text generally follow Quiller-Couch and Dover Wilson by presenting both theories, and sometimes more than two, as more or less plausible alternatives, although the *Ur-play* notion is often favored. In fact, the decision to offer multiple theories but not to choose between them intrinsically favors the *Ur-play* argument, which is too weak to stand on its own merits but can be made to seem credible when presented as one among a number of unconfirmed theories. In this case, the performance of academic doubt serves as a necessary prelude to the promotion of a dubious idea.

The editors of the *Oxford Shakespeare* give the fullest and most painstaking version of this performance, entertaining a wide selection of hypotheses without choosing among them, but favoring the idea of "a lost Elizabethan play later adapted by Fletcher."¹⁴ Stanley Wells considers the possibility of the Soto line as a late addition and even takes the unusual step of linking that possible addition to other signs of revision in the Folio text, although elsewhere in the same textual note he expresses a belief that those revisions were "undertaken during composition."¹⁵ He even briefly entertains the notion that the reference to Soto is a mere "hint" of Shakespeare's that Fletcher "expanded . . . into a character" some fifteen years later. This odd hypothesis, which Wells himself dismisses as "unlikely,"¹⁶ suggests that the Soto speech is a reference to nothing at all, and that fifteen years later another playwright created a play to which the earlier text only seems to allude. This too, would be difficult to defend as a general approach to dating literary allusions; if the principle that an allusion might precede the text and that the literary work might be a poetic response to the allusion itself were to be widely adopted, it would swiftly become impossible to use allusions for dating literary works.

¹⁴ Stanley Wells and Gary Taylor with John Jowett and William Montgomery, *William Shakespeare: A Textual Companion* (Oxford: Clarendon Press, 1987), 170.

¹⁵ Wells and Taylor, 169. Wells does suggest that if the Soto speech is indeed a reference to *Women Pleased*, then "the allusion might be a late interpolation, authorial or not" (170). An "authorial" interpolation in a Shakespeare play ca. 1620 surely cannot be what Wells intends to suggest.

¹⁶ Wells and Taylor, 170.

Wells thoroughly and rather scornfully dismisses Eric Sams's attempt to coordinate the standard dating of Sincklo and Soto when (in Wells's phrasing) Sams "arbitrarily dates" *Women Pleased* to 1604 and dates *The Taming of the Shrew* to succeed it.¹⁷ Wells finds this "wildly at odds with all the stylistic evidence, which points to a much earlier period of composition,"¹⁸ and he objects that there is no evidence of Fletcher writing for the King's Men, rather than for the boys' companies, so early in his career. While Wells's objections have some real merit, the charge of arbitrariness is at odds with Sams's good-faith attempt to reconcile the disparate dates in the crux without hypothesizing new evidence; far more arbitrary expedients, such as Dover Wilson's hypothetical lost play, are treated with more respect.

Like Quiller-Couch and Dover Wilson, the *Oxford* editors lean toward the *Ur-Soto* hypothesis but, like their predecessors, refuse to commit entirely: "We suspect this explanation [the lost Soto play] is correct, but in the nature of the case it could never be proven (or disproven)."¹⁹ The grounds of this suspicion, in the absence of corroboration or even the potential for corroboration, go unexplained. The evidence underlying the *Ur-Soto* notion is not given the rigorous examination applied to other hypotheses, since no evidence exists to be examined. But Wells's parenthetical appeal to the basic unfalsifiability of the claim, the impossibility of disproving it, shows how thoroughly the burden of proof has been misplaced here. A hypothesis that cannot be falsified, that is not subject to any test or examination, should be suspect for that very reason. It is true that no one will ever be able to prove that some early lost play about Soto did *not* exist, but this is not a reason to take the hypothesis seriously, let alone to endorse it by suspecting its correctness. Wells seems to imply, if only equivocally, that an "explanation" remains viable as long as it cannot be "disproven." The logical impossibility of proving a negative, therefore, ensures that the *Ur-Women Pleased* hypothesis will never die. The traditionally favored explanation for the Sincklo/Soto crux is impossible to prove, but has been enshrined by editorial tradition. The burden is now placed on challengers to disprove it. Indeed, it has become acceptable to confess the impossibility of proving the hypothesis even as one advocates it. The standards of scholarly proof and disproof have been suspended in the case of the *Shrew* plays, providing an untestable, unfalsifiable foundation upon which bibliographic orthodoxy can rest.

This superficially prudent hesitation about Soto—the refusal to choose between the unproved and unprovable possibilities—also has the practical effect of excluding the name from evidentiary status. The name "Soto" is not

¹⁷ Eric Sams, "The Timing of the *Shrews*," *Notes and Queries* 230 (1985): 33–45.

¹⁸ Wells and Taylor, 170.

¹⁹ Wells and Taylor, 170.

treated as a building block used to construct a larger argument about the provenance of the text. Instead, general accounts of the text are used to divine the proper interpretation of the anomalous fact. Many scholars proceed as if neither of the two standard but contradictory explanations of Soto's name has any implications for the date of *The Shrew* as a whole. The question of whether the Induction refers to an actual play from around 1620 or to a hypothetical play from the late 1580s or early 1590s is treated as irrelevant to the business of dating the play, so that modern editors may declare themselves open to both possibilities for dating "Soto" without proposing any adjustment in their dating of *The Shrew*. Textual critics can be publicly agnostic on this specific piece of evidence and firmly convinced of their general conclusion; they may not feel confident explaining how Soto's name got into the text, but they are certain that it doesn't change anything.

Repetition has made Quiller-Couch and Dover Wilson's twin hypotheses seem obvious and commonsensical to later editors; modern textual critics seem to have far less awareness than Quiller-Couch and Dover Wilson of their arguments' vulnerabilities. More importantly, the "explosive suggestion" of the 1920s has now become the standard account of the *Shrew* plays' relationship, and that account does not allow for a Soto. Scholars who come to the Sincklo/Soto crux already convinced of the Folio text's early date, rather than still making the case as the New Cambridge editors did, have been trained to expect that "Soto" will fit into the established dating.

Ann Thompson, Quiller-Couch and Dover Wilson's successor as editor of the New Cambridge Shakespeare *Taming of the Shrew*, pronounces that "it is now generally agreed that *A Shrew* is some kind of memorial reconstruction of *The Shrew* itself," vigorously champions that position, and presses for a date "as early as 1590."²⁰ Brian Morris begins his discussion by saying, "Much of the earlier scholarly speculation about the date of *The Shrew* can be disregarded once it is accepted that *A Shrew* is a Bad Quarto and therefore later than its original."²¹ Stanley Wells and Gary Taylor profess themselves agnostic about the relationship between the quarto and Folio texts but edit the play as if it had an extremely early date.²² The Norton Shakespeare apparatus firmly endorses the conventional orthodoxy about *The Shrew*. The claim that *The Shrew* preceded *A Shrew* is presented as a "growing consensus," with Leah Marcus's groundbreaking arguments to the contrary described as an "interesting" minority position; Eric Sams's infuriated and wide-ranging dissent, "The Timing

²⁰ Ann Thompson, ed., *The Taming of the Shrew*, rev. ed. (Cambridge: Cambridge UP, 2003), 2, 3.

²¹ Brian Morris, ed., *The Taming of the Shrew* (London: Methuen, 1951), 50.

²² Wells and Taylor, 169.

of the *Shrews*," is ignored.²³ The date of *The Shrew* is limited to 1592 or earlier, on the explicit grounds of its precedence to *A Shrew*; and the Folio text is held to derive, directly or indirectly, from "Shakespeare's 'foul papers'" because of "certain features of the text."²⁴ Why an authorial manuscript would include a reference to a play written after the author's death is left unexplored; rather, the detailed exploration performed in the *Oxford Shakespeare's Textual Companion* is treated as moot. Moreover, the *Oxford* and *Norton* texts arrange Shakespeare's works in "chronological" sequence, placing *The Taming of the Shrew* earlier than any of Shakespeare's works, except for *The Two Gentlemen of Verona*, and before the other plays published in 1594. The polemic implicit in the editors' tables of contents shores up the establishment position and quarantines the Folio *Shrew* text to a date at least thirty years before its publication, safe from the three intervening decades of its theatrical history.

Inconveniently, one of the "features of the text" upon which the orthodox narrative about *The Shrew* is based is the embarrassing detail of Sincklo's name.

III. HOW TO WRITE FOUL PAPERS

The notion that actors' names in printed texts might derive from an authorial manuscript, and that such names might even be construed as positive evidence of the author's hand, dates from the early 1930s, when R. B. McKerrow, "busy generating editorial theory," in Paul Werstine's phrase, invents his influential account of Shakespeare's foul papers, singling out certain types of oddity in printed texts as reliably legible signs of Shakespeare's drafts.²⁵ "One of the reasons for the badness of dramatic texts was that they were often set up from the author's original manuscript and not from a fair-copy," McKerrow argues.²⁶ His approach identifies certain kinds of "badness" as merely bad, unfortunate artifacts of the playhouse or the printing house, but embraces other kinds of "badness" as good, as Shakespeare's own personal mistakes and hurried

²³ Greenblatt, gen. ed., 140–41, esp. 140.

²⁴ Greenblatt, gen. ed., 140. Wells is more cautious in *William Shakespeare: A Textual Companion*, noting "a combination of the characteristics of a [theatrical] transcript and foul papers" (Wells and Taylor, 170); cautious and detailed editorial theory is simplified into far less cautious practice.

²⁵ R. B. McKerrow, "The Elizabethan Printer and Dramatic Manuscripts," in *Ronald Brunlees McKerrow: A Selection of His Essays*, comp. John Phillip Immroth (Metuchen, NJ: Scarecrow Press, 1974), 139–58, esp. 149. McKerrow does not use the term "foul papers" in this 1931 essay—he refers to "author's manuscript" and "author's rough draft"—but he cites W. W. Greg's 1925 essay in which Greg reports having found the term in a letter from Edward Knight, bookkeeper for the King's Men in the 1620s and 1630s. See Paul Werstine's "McKerrow's 'Suggestion' and Twentieth-Century Textual Criticism," *Renaissance Drama* (1988) 19: 149–73, esp. 150.

²⁶ R. B. McKerrow, "Elizabethan Printer," in Immroth, comp., 149.

orthography, which are to be treasured. McKerrow's assertions naturally cannot be tested by examination of Shakespeare's foul papers themselves, but his detailed and entirely speculative claims about their features have become part of the foundation of Shakespearean bibliography.

McKerrow builds his imaginary model of Shakespeare's foul papers by negative definition, carefully enumerating the features of theatrical promptbooks and taking the lack of such signs, or of those he finds sufficiently persuasive, to indicate Shakespeare's autograph draft. "It thus seems to me," McKerrow argues, "that the origin from prompt-books of the texts . . . to which we have been referring is far from being proved, and that until this is done we may continue to assume that they were printed from the author's own manuscript, or at any rate from a rough copy of some sort or another."²⁷ The "at any rate" is a nice piece of rhetoric, suggesting McKerrow's openness to other possibilities without actually postulating any other species of "rough copy." Yet McKerrow abandons this caveat by the end of the paragraph in which he makes it, proposing "an author's rough draft much corrected"²⁸ as the typical printer's copy for plays. Most of McKerrow's followers have embraced the proposed rule and not the equivocal exception; since McKerrow, an editor's rule of thumb has been to construe roughness as authorial.

McKerrow effectively constructs, while purporting to disown, a false dichotomy that allows for only two sources of copy text: if the copy does not come from the prompter, it must come from the author's rough draft. McKerrow simultaneously (and perversely) shifts the burden of proof onto anyone not embracing the foul-papers hypothesis. The copy text shall be assumed to derive from Shakespeare's authorial manuscript unless proved otherwise. By the same token, Shakespeare's authorial manuscript will be presumed to exhibit certain specific features until proved otherwise, which cannot be done in the absence of any such manuscripts. Assertions that are neither proven nor subject to falsification are presented as necessary starting points for any discussion of the text. Subsequent editing practice has adopted McKerrow's speculative axioms as first principles, and their near immunity to disproof has transformed them into virtually inevitable conclusions.

McKerrow's article makes a point of claiming actors' names as a sign of authorial, rather than theatrical, manuscripts, specifically rebutting Dover Wilson and A. W. Pollard's arguments to the contrary. McKerrow does not deny (and could not deny, in the face of material evidence) that promptbooks included actors' names, but he creates a novel and artificial distinction between the demonstrable ways that such names appear in promptbooks (which can

²⁷ McKerrow, "Elizabethan Printer," in Immroth, comp., 156.

²⁸ McKerrow, in Immroth, comp., 156.

be examined) and the imagined way they appear in Shakespeare's foul papers (which cannot be):

A third mark of prompter's copies is the mention of actor's [sic] names as a *gloss*. This is important. So far as I have noticed, the name of the actor in a prompt copy always appears *in addition* to the name of the character, not substituted for it. . . . I believe that there is no case in any play having the clear marks of being, or being printed from, a prompt-copy, of an actor's name being given *alone* without that of the character whom he was to represent.²⁹

McKerrow is having it both ways here. His argument is premised on the idea that bookkeepers routinely removed authorial idiosyncrasies while preparing promptbooks. Therefore, by McKerrow's logic, actors' names are never substituted for characters' names in promptbooks because the bookkeeper would remove any such substitution. McKerrow imagines the prompters removing actors' names from texts and also as adding them "as a gloss." Having cast doubt on the objection that actors' names appear in promptbooks, McKerrow proceeds to offer, through a vividly imagined narrative of Shakespeare at work, his own hypothesis about how actors' names might enter the authorial draft:

Even the occasional mention of the name of an actor seems to me far from unnatural in the manuscript of such a dramatist as Shakespeare, who was writing for a particular company with which he was closely connected. Psychologically it is, I think, just what we should expect. To a man with a good power of visualization such as every successful dramatist must have, and who knows in advance what actor will fill each of the more important roles, the actors themselves must have been more or less constantly present in his mind as he wrote. I suspect, indeed, that this fact is responsible for the extraordinary vitality and vividness of . . . some of Shakespeare's minor characters. . . . [W]hat more natural than that Shakespeare . . . should momentarily forget the names which he had assigned to the characters and put down instead the much more familiar names of the actors instead?³⁰

McKerrow's gifts for storytelling and characterization carry his argument here. However slyly McKerrow slips in the reference to the "fact" that the actors were "constantly present in [Shakespeare's] mind," this is not a fact but a supposition or, more accurately, a wish. We do not know what was in Shakespeare's mind as he wrote, and cannot know for all of our scholarly longings; this is simply what McKerrow would like Shakespeare to have been thinking.

For all its air of facticity, McKerrow's argument includes virtually no verifiable facts. While it is true that after 1594 Shakespeare was writing for a particular company with which he was closely connected, it is much harder to

²⁹ McKerrow, "Elizabethan Printer," in Immroth, comp., 153–54.

³⁰ McKerrow, "Elizabethan Printer," in Immroth, comp., 155–56.

establish which playing companies Shakespeare may have belonged to before joining the Lord Chamberlain's Men, the identity of his fellow actors in those earlier companies, or the duration of his working relationship with those allegedly "familiar" actors. To use Sincklo's name to date the play before 1594, as editors from Quiller-Couch and Dover Wilson to Ann Thompson have done, sets McKerrow's fanciful narrative in the years when his story is least plausible. Neither is it established that Shakespeare knew all of the casting of his plays in advance; we simply don't know the details of the casting procedure, and don't have enough information about the casts to reconstruct it. The most we can say is that Shakespeare knew his company's casting process, as we do not; to say that he could necessarily predict, let alone dictate, the outcome of that process is to speak more than we know. McKerrow's careful-sounding negative construction, his "far from unnatural," is a rhetorical masterstroke, mimicking scholarly caution while framing the question so that the burden of proof seems to fall upon any doubters, as if all that it took to support an argument were to prevent it from being proved palpably "unnatural."

By 1935, McKerrow had extended his imaginary reconstruction of Shakespeare's compositional process from the appearance of actors' names in the text to inconsistencies in the handling of character names in speech prefixes and stage directions. McKerrow's influential but unsubstantiated "Suggestion Regarding Shakespeare's Manuscripts" posits that "a play in which the names are irregular was printed from the author's original MS."³¹ McKerrow's reasoning has been thoroughly debunked by Paul Werstine, and by the evidence itself.³² The casual treatment of character names that McKerrow ascribes to authorial drafts is evident in surviving playhouse manuscripts. In fact, John Sinckler or Sincklo's first name is only known to us because it appears in a playhouse manuscript that mixes actors' and characters' names indiscriminately. But McKerrow's ideas have become so integral to Shakespeare editing that his principles frequently go uncited and unexplained, as if the characteristic features that McKerrow imagines for foul papers were as demonstrable as the distinction between octavos and quartos. Ann Thompson, for example, might doubt that the Folio *Shrew* derives from foul papers, but she takes the model of foul-papers copy texts, and the features of such texts, as perfectly straightforward.³³

³¹ R. B. McKerrow. "A Suggestion Regarding Shakespeare's Manuscripts," *Review of English Studies* o.s. 11 (1935): 459–65, esp. 464.

³² Werstine, "McKerrow's 'Suggestion.'"

³³ Thompson, ed., 164–68. Compare G. Blakemore Evans's assertion in the updated New Cambridge edition of *Romeo and Juliet* (Cambridge: Cambridge UP, 2003) that "there is universal agreement that the printer's copy for Q2 was derived in some way from Shakespeare's rough draft ('foul papers')" (208). Granted that Evans, like Thompson, is updating an edition rather than creating an entirely new one, the work of updating might include the acknowledgment that

It was not always so. W. W. Greg was initially skeptical of taking actors' names as signs of authorial copy. Greg wrote in 1931 that he "should not venture to deny that" such an interpretation "is possible," but that the evidence runs contrary: "In every instance in which an actor's name appears in a manuscript play it is written in a different hand from the text, or at any rate in a different ink and style, showing it to be a later addition and not part of the original composition."³⁴ By 1942, Greg had largely accepted his friend McKerrow's foul-papers model, and he confidently repeats the contention that inconsistent speech prefixes, which "would probably be tidied up by whoever prepared the prompt copy" are consequently signs of an authorial draft.³⁵ In the case of Folio *Shrew*, Greg still refuses to believe that "the parts have been cast in his mind by the author," but he exempts Sincklo from his refusals and considers the name a deliberate, rather than inadvertent, casting note by Shakespeare.³⁶

By the time he wrote *The Shakespeare First Folio*, which has served as an important guidebook to subsequent editors, Greg was blandly citing "the substitution of an actor's name" as one of the well-established features "characteristic of foul papers" and carefully distinguishing the way actors' names appear in promptbooks from the ways they are imagined to appear in "foul papers."³⁷ Greg holds up *The Taming of the Shrew*, with its reference to Sincklo, as an example of a text derived from authorial manuscript,³⁸ but remains skittish, if not downright ambivalent, about using actors' names to point to authors' papers. Greg takes Sincklo's name as a clear sign of Shakespeare's foul papers behind the first quarto of 2 *Henry IV*, but he also declares that Sincklo's name "cannot possibly be attributed to the author" in the Folio text of 3 *Henry VI*, on the grounds that Sincklo's name is also used consistently in the speech headings throughout the scene and therefore cannot be a slip of the authorial pen.³⁹ The apparent inconsistency comes from Greg's scrupulous fidelity to McKerrow's original logic; an actor's name suggests foul papers because it is assumed that a prompter would remove the name. When summarizing general editorial principle, Greg presents the conclusion of McKerrow's syllogism (actors' names are a characteristic of foul papers), but when applying those principles, Greg keeps the original premises of the syllogism in mind.

old agreements are no longer universal. Clearly, Evans, like Thompson, feels no need to defend or justify the traditional foul-papers model.

³⁴ W. W. Greg, *Dramatic Documents from the Elizabethan Playhouses*, 2 vols. (Oxford: Clarendon Press, 1931), 1:216.

³⁵ W. W. Greg, *The Editorial Problem in Shakespeare* (Oxford: Clarendon Press, 1942), 102–103.

³⁶ Greg, *Editorial Problem*, 73 et passim.

³⁷ Greg, *The Shakespeare First Folio* (Oxford: Clarendon Press, 1955), 142, 116–17.

³⁸ Greg, *Shakespeare First Folio*, 212–14.

³⁹ Greg, *Shakespeare First Folio*, 266, 183.

Greg's caveats and fine distinctions—and, for that matter, McKerrow's—tend to be simplified in later practice; the statement of general principle outweighs the discriminating application. The issue is not how delicately McKerrow or Greg or any of the editors who follow them use McKerrow's hypothesis, but the elevation of that unproven and unprovable hypothesis into a standard and uncontroversial tool of editing. The caution with which Greg and some later editors apply McKerrow's categories camouflages the profound flaws in the categories themselves.

H. J. Oliver's 1982 Oxford edition confidently asserts the appearance of actors' names in the text as primary evidence, indeed his leading evidence, for the foul-papers theory: "Shakespeare must have had Sincklo in mind when he wrote this part for the otherwise unnamed "Player"—and used the appellation in the speech prefix by an easy slip of the pen. (The name of an actor would hardly appear thus in a prompt-book, in one line and not in another, where it could cause only confusion.)"⁴⁰ These two sentences rely upon a number of suppositions: about the way Shakespeare composed, the way promptbooks were organized, and what would confuse early modern actors or prompters. But perhaps the most breathtaking assumption is Oliver's confidence that Sincklo got more than one line. Here, the textual critic takes his own emendation as evidence; the text gives "Sincklo" only one speech, but after Oliver has assigned that line to another speaking character, he points to the "inconsistency" with the other character's speech prefixes.

Brian Morris, who expresses reservations about the unmodified foul-papers theory, nonetheless calls the appearance of Sincklo's name as "one point . . . at which we can almost certainly detect Shakespeare's hand," and he pronounces it "very unlikely that a book-keeper or prompter could be responsible."⁴¹ The sway of McKerrow's theories can be seen in how even doubters must cede some ground to them. The assumptions here are counterintuitive, contrafactual, and taken as entirely self-evident. It has become a bibliographical truth almost universally acknowledged that the appearance of performers' names in a text excludes the possibility that it is a performers' text.

IV. SINCKLO AND NICKE

Of course, actors' names do appear in the relatively few surviving texts prepared by bookkeepers or prompters. Indeed, the specific actors' names in the 1623 *Shrew* appear in those texts. The "plot" of *The Second Part of the Seven Deadly Sins*, prepared to hang backstage in the tiring house, lists not only "I Sincler" and "Iohn Sincler" but also the names Harry, Vincent, Saunder, Will, Kitt, Ned, and

⁴⁰ Oliver, ed., 5.

⁴¹ Morris, *Shrew* (see n. 21 above), 6.

Nick. The Folio *Shrew* has a speech for a “Nicke” in Act 3, scene 1, after the stage direction “Enter a Messenger” (TLN 137–78). “Nicke’s” name, like “Sincklo’s,” is used to promote the “foul papers” idea, but the King’s Men’s prompter has written “Nicke” into promptbooks as well. A “Nick” appears in the manuscript of *Sir John van Olden Barnavel*t in 1619 (which is to say, around the time the company performed *Women Pleased*) and also figures in the 1631 manuscript of *Believe as You List*. Gerald Eades Bentley, who is strongly committed to the notion of an early date for *The Taming of the Shrew*, nonetheless gives “Nick” his own entry among the actors listed in *The Jacobean and Caroline Stage*.⁴² But no critic associates the “Nick” in a King Men’s manuscript from 1619 with the “Nicke” in a manuscript provided to the printer in 1623; scholars are interested in fixing the date of that manuscript as early as possible.

Nicke’s name is inconveniently common, but editors use Sincklo’s name to provide a *terminus ad quem*. The curious side effect of this procedure is that the editors must also provide a *terminus ad quem* for Sincklo. But since his career is never documented outside the plays themselves, scholars pronounce an end (or a likely end) to Sincklo’s career with no documentary evidence whatsoever. The standard technique is to argue from negative evidence: once Sincklo’s presence can no longer be confirmed, his absence is assumed. E. K. Chambers’s *Elizabethan Stage* puts cautionary question marks beside some of Sincklo’s presumed company affiliations but is blandly confident in assigning initial and terminal dates for his career. Chambers lists Sincklo as performing from 1590 to 1604, which is to say from the date Chambers prefers for the *Seven Deadly Sins* plot to the Induction of the *Malcontent*, and not a day later.⁴³ Andrew Gurr’s company history of the King’s Men is slightly more generous, extending Sincklo’s stage time to “c. 1606.”⁴⁴ Gurr gives Sincklo the extra years by speculatively casting him as various diminutive characters in plays from 1604 or 1605; this is interesting, but not quite evidence. (Sams’s proposed date of circa 1605 is founded on this consensual termination date for Sincklo’s career.) Ann Thompson even offers Sincklo’s absence from the list of “principal actors” in the 1623 Folio as a sign that Sincklo had retired,⁴⁵ although there is nothing odd about a minor actor being excluded from the list of principals, and the list has nothing at all to do with how recently the actors had left acting. It includes a number of players who had died or retired decades earlier, alongside some of the troupe’s current leadership. Thompson also argues that if Sincklo had not

⁴² Gerald Eades Bentley, *The Jacobean and Caroline Stage*, 7 vols. (Oxford: Clarendon Press, 1940–68), 3:432, 2:516–17.

⁴³ E. K. Chambers, *The Elizabethan Stage*, 4 vols. (Oxford: Clarendon Press, 1923), 2:339.

⁴⁴ Andrew Gurr, *The Shakespeare Company, 1594–1642* (Cambridge: Cambridge UP, 2004), 241.

⁴⁵ Thompson, ed., 58n84.

retired, he would have been “too old” to play Soto in 1620,⁴⁶ but neither Soto’s age nor the upper age limit for playing comical servants has been established. And while Sincklo’s marginal presence in the Folio is often compared to the appearance of William Kemp’s name in Q2 *Romeo and Juliet* (1599) or Kemp’s and Richard Cowley’s names in Q1 *Much Ado About Nothing* (1600), those quartos were published soon after Kemp left the Chamberlain’s Men, and Kemp’s name is a relic of his recent tenure with the group. By 1623, Kemp’s name had been removed from the text. Sincklo’s name, on the other hand, is cast as a souvenir of the distant past and is presumed to have stayed in the script for three decades, and for almost two decades after he is imagined to have left the stage. In any case, the entire project of using Sincklo to set an end date for *The Shrew*, or indeed for any play, is manifestly illogical. That one cannot prove Sincklo still remained with the company cannot be offered as proof that he had departed.

It may well be, as Hans Walter Gabler puts it, that Sincklo “disappears from the Elizabethan dramatic records after 1604,”⁴⁷ but the statement is not quite fair, because Sincklo never figured in those records. Moreover Gabler’s formulation, which strikes me as typical for most critics dealing with this question, excludes the 1623 Shakespeare Folio as a “dramatic record.” It is more accurate to say that Sincklo’s name first appears in print in 1600, turns up again in 1604, and appears for the final time in 1623. Sincklo’s appearance in the Folio is taken as a relic of the 1590s, even when “Sincklo” alludes to a play from 1620. But this needs to be proven before it is deployed as proof.

Andrew Gurr rehearses the smoothly circular argument that Sincklo’s name in *The Shrew* and in 3 *Henry VI* is a survival from the time when those plays belonged in the Pembroke’s Men repertory, and he adduces Sincklo’s membership in Pembroke’s Men from his name’s appearance in their scripts.⁴⁸ There is no evidence, beyond this neat tautology, that Sincklo ever acted with the Pembroke company, and his name does not appear in the quartos that advertise *The Taming of a Shrew* and *The True Tragedy of Richard, Duke of York* as plays belonging to the Pembroke’s Men. Sincklo never appears in any version of a play presenting itself as the Pembroke’s Men’s, but always in versions presenting themselves as the Chamberlain’s.

At stake, again, is the precedence of the Folio texts over the “bad” quarto texts of 1594. If *The True Tragedy* and *The Taming of a Shrew* derive from the superior Folio texts, then those Folio texts must have existed before the formation of the

⁴⁶ Thompson, ed. (see n. 20 above), 58n84.

⁴⁷ Fredson Bowers, gen. ed., *The Dramatic Works in the Beaumont and Fletcher Canon*, 10 vols. (Cambridge: Cambridge UP, 1966–96), 5:444.

⁴⁸ Andrew Gurr, *The Shakespearian Playing Companies* (Oxford: Clarendon Press, 1996), 71–73.

Chamberlain's Men in 1594. Therefore, it becomes necessary to establish that the Pembroke's Men possessed the "good" texts. *The Taming of the Shrew* must be imagined as a text from 1592 instead of 1623. For even the best of scholars, the conclusions begin to drive the evidence. Since the Folio versions must have preceded the quartos, then the Folio versions must have been the Pembroke's Men's versions, and an actor named in the Chamberlain's Men's versions must have been one of Pembroke's Men.

The most important evidence for locating Sincklo in the early 1590s has been the *Seven Deadly Sins* plot, long considered to derive from the amalgamation of Lord Strange's Men and the Admiral's Men in that time period. (Even this account, however, places Sincklo with the Pembroke's Men's primary rivals during the relevant time period.) The primary basis for associating the plot with the Admiral's Men has been provenance, because the document survives in Edward Alleyn's archive at Dulwich College. However, David Kathman makes the case that the plot entered the Dulwich collection through a bequest from the bookseller William Cartwright, and that no connection to Alleyn is certain.⁴⁹ If one no longer presumes Alleyn's participation in *Seven Deadly Sins*, the principal actors seem to be a list of principal Chamberlain's Men from the mid- to late 1590s. Moreover, Kathman has positively identified one of the plot's boy players, "T. Belt," as the Thomas Belt apprenticed in 1595 to one of the Chamberlain's sharers, John Heminges.⁵⁰ The playhouse document which has been taken to prove Sincklo's pre-1594 career is in fact demonstrably later than 1594, and all of the surviving documents about Sincklo can now be seen to associate him with the Chamberlain's (and later, the King's) Men.

Whether by chance or design, Sincklo's name is only printed in plays which have somehow entered the Chamberlain's/King's repertory from that of another company. *The Shrew* and *3 Henry VI* had been Pembroke's Men's plays before they belonged to the Chamberlain's Men; the Queen's Men's *Famous Victories of Henry V* had staged Falstaff (under his previous name, Oldcastle) and the theatrical reformation of the future Henry V before the *Henry IV* plays did so; and the Induction of *The Malcontent*, in which Sincklo appears, explicitly comments on that play's previous ownership: "I wonder you would play it," asks William Sly in the character of a truculent spectator, "another company having interest in it."⁵¹ If Sincklo's name in a printed text bore any significance, it was diametrically opposed to the meaning scholars have constructed for it. Sincklo's name in a printed play has been taken as a trace from a play's existence

⁴⁹ David Kathman, "Reconsidering *The Seven Deadly Sins*" *Early Theatre* 7 (2004): 13–44, esp. 14–18.

⁵⁰ Kathman, 28, 30–31.

⁵¹ Marston (see n. 3 above), Induction, ll. 75–76.

before the King's Men acquired it; it might be better construed, to the extent that it will bear construction, as a sign of the Chamberlain's Men's possession and the consolidation of their ownership. If Sincklo's name suggests a date for *The Taming of the Shrew*, it suggests a *terminus a quo* in the mid- or late 1590s. Sincklo does not guarantee a pre-1594 text. He raises the possibility of a later one.

V. REVISION TROUBLE

The same impulses which have led scholars to push "Sincklo" as far back in theatrical history as possible have also impelled them to try to push "Soto" back even further. Quiller-Couch and Dover Wilson's second hypothesis is preferred to their first. A lost Soto play is more comfortable for many critics than the idea of a non-authorial insertion, however small, in Shakespeare's text. If *The Taming of the Shrew* must be dated before *The Taming of a Shrew* (and some critics date "A" *Shrew* to early 1592, arguing that it preserves a reference to the actor Simon Jewell, who died that year⁵²), then some form of *Women Pleasèd* must be dated even earlier than 1592. That all of the other available evidence places *Women Pleasèd* between 1619 and 1623 is not the point. Gabler summarizes the hypotheses about revisions in Fletcher's play, all founded solely upon the necessity to avoid a late date for *The Shrew* and its Induction, but sees no internal evidence for such revisions.⁵³

The hypothesis that Fletcher might have revised a play three decades old is used to exclude the possibility that he might have touched up another three-decades-old play, *The Shrew*. The idea that plays by playwrights other than Shakespeare were revised over many years is acceptable, but the idea that Shakespeare's plays were revised to this extent creates uneasiness. Fletcher's plays are moving targets; a Shakespeare play is an ever-fixed mark. Also excluded is the possibility that plays underwent gradual revision and mutation between their first productions and their later printings. Every revision-centered argument about the purported inconsistencies between Soto's role and the Lord's description of that role posits an earlier version of the play, which had been superseded by 1620 or so. The possibility that the Lord's summary, printed in 1623, might be an accurate statement about *Women Pleasèd* circa 1623, and that Fletcher's play might have undergone changes between that comment and its first printing in 1647, is excluded entirely. Hypothetical revision is allowed in order to push the date of composition back, not forward. And a variant text

⁵² Mary Edmond, "Pembroke's Men," *Review of English Studies* n.s. 25 (1974), 129–136; Morris, ed., 51–52; Thompson, ed., 3; and Greenblatt, gen. ed., 140.

⁵³ Bowers, gen. ed., 5:444.

which does not exist, the *Ur-Women Pleased*, is conjured out of thin air in order to deny revision as an explanation for existing variants.

To admit speculation about late revisions, to treat dramatic texts as potentially open until the moment of publication, would flirt with one of the last great taboos of Shakespeare scholarship: the implicitly forbidden hypothesis that Shakespeare's works might have been substantially improved by his collaborators. The Folio text of *The Taming of the Shrew* is published a decade into Fletcher's career as the King's Men's company playwright—in fact, near the end of that career. The Folio text explicitly points at another of Fletcher's works. But the notion that Fletcher might have had any significant hand in the 1623 text is tacitly rejected; it is professionally unthinkable. Surely, I would not positively identify Fletcher's as the revising hand; I do not propose to set up any new orthodoxies upon the foundation of negative proof. But the texts of the *Shrew* are not served by ascribing them simple histories or single parents.

Revision of Shakespeare's work by other hands can be admitted as a hypothesis, as in the case of *Macbeth*, if that revision is imagined as a source of interference or inferiority. *Macbeth's* brevity, its occasionally garbled text, the presence of discrete songs or scenes that can be treated as foreign accretions are all acceptable signs of another hand. Indeed, this is more comfortable than the idea that Shakespeare himself produced a truncated and sometimes confused play. But textual critics treat *The Taming of the Shrew* far more gingerly, despite elements that might in other circumstances be blamed upon a ham-handed adapter, what the Norton edition calls "marks of confusion or incomplete revision,"⁵⁴ especially in the Hortensio subplot. But the Norton edition is careful to ascribe even the faulty revisions to Shakespeare and to quarantine them to his initial drafts. If mistakes were made, the conventional reasoning goes, they were made in the original composition process and left uncorrected for the next thirty years.

The standard account of *Macbeth* insists that any mistakes or inconsistencies arise from post-Shakespeare tampering; the standard account of *The Shrew* insists that any mistakes are Shakespeare's own, and that they were carefully preserved from manipulations of any kind until the publication of the First Folio. I find it difficult to imagine Shakespeare's fellow actors revering his manuscript so slavishly that they would refrain for three decades from correcting even his obvious plot-related errors; only Shakespeare's critics are capable of such uncritical fidelity. Equally mysterious is the idea that Shakespeare's personal manuscript was so treasured that it was hoarded for thirty years before being sent off to serve as disposable printer's copy, which is the essence of the unmodified foul-papers hypothesis. (The foul-papers story presumes that fair

⁵⁴ Greenblatt, gen. ed., 140.

copies, such as the promptbooks, were too valuable to be sent to the printer, so the dispensable foul papers were supplied instead; why the King's Men would still have the foul papers in 1623 but no longer consider them worth keeping has not been made clear.) It is also peculiar to imagine Shakespeare leaving his own mistakes uncorrected for approximately twenty years while *The Shrew* stayed in repertory in his own playing company (and even while he presumably acted in it himself).

The reluctance to hypothesize a late collaborator or adapter for the *Shrew*, even as a scapegoat, can be attributed to the existence of the quarto text. If the superior Folio text was shaped by hands beside Shakespeare's, where does the source of its literary superiority reside? Suppose what Marcus describes as the "burnished, eloquent language" of the "wittier" and "more refined" 1623 text reflects a late-Jacobean emphasis on refinement and wit, and Shakespeare was not the sole burnisher?⁵⁵ Between a quarto text that many Shakespeare scholars wish to disown, and a Folio text "tainted" by non-Shakespeare elements, would there be room for Shakespeare at all? Of the two *Shrews*, one must be assigned solely to him, in order to sustain his literary authority. Otherwise, where could Shakespeare be found among his many sources and partners, between Fletcher's sequel writing and a late collaborator and *A Shrew* and Gascoigne's *Supposes*?

The question is natural but ultimately improper to ask. It is not the task of scholars to defend or advance William Shakespeare's literary reputation. Nor is advocacy for a beloved figure likely to gain us any keener appreciation for his work. The scholar's task is not to create the author we prefer, but to read the works as they have been left to us. Shakespeare can stand up for himself.

VI. AGAINST CONCLUSIONS

The conventional and expected goal of textual criticism is to provide an overall account of the text, organizing the pesky details into a well-rounded and easily summarized whole. The imagined goal of working with specifics of the text is the ability to speak about text in generalities. By this standard, my criticisms of previous textual critics should culminate in my own revised but complete and encompassing narrative about the text of *The Taming of the Shrew*, or my labor will be turned to no account. In this model, a narrative is critiqued so that it may be replaced.

My own instinct is to place *A Shrew* and *The Shrew* in a narrative of revision driven by the needs of playing companies—to suggest that the play the Pembroke's Men owned was transformed into a more sophisticated work for the Lord Chamberlain's Men and to note the similarities between Shakespeare's

⁵⁵ Leah S. Marcus, *Unediting the Renaissance: Shakespeare, Marlowe, Milton* (London: Routledge, 1996), 123–24, 128.

transmutation of the old Queen's Men's *King Leir* into *King Lear* and the metamorphoses undergone by *A Shrew* and *The Shrew*. But I will confess that the violence done to logic and evidence in the case of the *Shrew* plays, always in the service of promoting a coherent general narrative, makes me wary of conclusions. I would rather tear down a general narrative and replace it with nothing at all, at least for a while. There have been more changes to the texts of *The Shrew* than any one story explains. If I part ways with Sams in the matter of the *Shrews* it is here, because I do not accept any single date for this play. This text is not the product of any discrete historical moment. It was not written at one time. It is not an integral text.

While editors have remained agnostic about the Sincklo/Soto crux but convinced about the larger picture, I would prefer to move in the opposite direction. Evidentiary details are hard to use until they are organized into some theory, but what use is a theory that obscures and distorts the evidence? Textual theories, especially in Shakespeare studies, have driven the treatment of bibliographic facts far more thoroughly, and far longer, than anyone is happy to acknowledge. And the great advantage of such theories is also their great poverty: they serve to make the condition of the text simpler and more easily comprehensible. If I advocate for anything, it must be the pleasures of textual skepticism. Early modern texts, and especially early modern dramatic texts, are messy, complicated, and puzzling. For scholarship to mitigate that mess and complication is to reject the very nature of the texts with which we work, and to obscure them. I do not purport to understand every aspect of the *Shrew* plays' textual condition, and I harbor deep suspicion of any scholar who does profess such all-inclusive comprehension. The texts in question are far too open, far too promiscuous in their histories, for any simple stemma, and premature conclusions have been the great bane of Shakespearean textual criticism.

What the case of Sincklo and Soto illustrates most clearly is the temptation and folly of the *terminus ad quem*; critics have repeatedly attempted to foreclose textual possibilities, to reduce the number of potential agents, to simplify the account of the text, and to shore up the author's authority by placing the earliest limit possible on the text's development, even when that means fixing an artificial boundary three decades before publication. But the reference to Soto suggests, quite casually, that such boundaries are merely academic artifice, premature conclusions in every sense. *The Taming of the Shrew* was evidently open to revision after its author was dead, and until a few years before it was printed. I would be a fool to presume that the latest demonstrable revision is necessarily the last, or that early modern plays had any *terminus ad quem* but publication. Early modern plays were never finished; they were merely sent to the printers.