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What, if anything, can we say about the Renaissance drama that does not survive? And how much of it is there?

One could start by considering a special case, that of the early modern commercial theatres. It has been estimated that, in the lifetime of the commercial playhouses, roughly 1567 to 1642, around 3,000 different plays must have been written and staged in them: and that of these, a minority survive, among them, obviously, the plays of Shakespeare, Jonson, Marlowe and the rest. The latest and most authoritative count puts the number of surviving plays at 543. But among the remainder are many identifiable “lost plays”, typically preserved in the form of a title in Philip Henslowe’s Diary, or Sir Henry Herbert’s Office-Book, or a similar source. The number of identifiable “lost plays” in this category has long been underestimated by scholars. In the twentieth century, for instance, E. K. Chambers noted only 74 lost play-titles in The Elizabethan Stage, to which G. E. Bentley added approximately another 268 in The Jacobean and Caroline Stage. Alfred Harbage listed around 500 lost commercial-theatre plays in this date range in his Annals of English Drama, a figure endorsed by other scholars including Andrew Gurr. But the latest count of plays in this category suggests that there are now no fewer than 744 identifiable “lost plays” from the commercial theatre in this period.

All three of these numbers – 3,000 overall, 543 extant, 744 identifiable as lost – elide numerous problems of definition. The first number is, in point of fact, difficult to even approximate, since the available evidence (such as Henslowe and Herbert) is partial and ambiguous, and since it must then be extrapolated across a period which saw rapid and enormous fluctuations in the theatrical environment. The overall estimate offered here, that of Andrew Gurr, is broadly in line with
earlier estimates by W. W. Greg and G. E. Bentley. The numbers given here for extant plays and identifiable lost plays are provided by Martin Wiggins, author of the new and seminal work *British Drama 1533–1642: A Catalogue*, and mark a significant advance over the best estimates of even twenty years ago, although they remain, of course, provisional. But although these numbers are approximate, they serve to suggest some surprising conclusions about lost plays as they pertain to the commercial theatre of the early modern period.

First, and most obviously, whereas previous counts had suggested that there were approximately equal numbers of extant plays and identifiable lost plays, it is now clear that in the commercial context, identifiable lost plays are significantly more numerous than extant ones. Second, the (approximately!) 543 extant early modern commercial-theatre plays cannot be considered an entirely representative sample of the total output of early modern commercial theatre, in that their very survival makes them untypical. In particular, the large majority of those which survive do so by virtue of having been able to make the transition into the early modern print marketplace. Those qualities that made a play viable in that marketplace will be over-represented in the survivors (compared to their nearly 2,500 unpublished siblings), and the 744 “lost plays” might arguably provide a better window on the typical features of early modern professional theatre. Third, those 744 “lost plays” are still among the better-documented plays from the period. They are more knowable than the much larger number of plays which have disappeared without any trace at all.

What is true of the drama of the commercial theatre is also true of all the other forms of drama in early modern England: the drama of schools, universities, and courts, of guilds and private households. Whereas with commercial theatre we at least have a sense of the likely overall population from which we have some surviving plays and some “lost plays”, in the case of amateur drama such an overall ceiling is much harder to estimate, but certainly survivors from it are plentiful. In addition to the 543 extant plays from the commercial theatre, there survive from the same period around 450 dramatic texts which could be badged as “elite”, and around 220 which could be called “literary”, that is, written seemingly as poetic documents without the intention of performance. Using those same categories, Martin Wiggins records 383 “lost plays” belonging to the “elite” category, and 34 lost “literary” plays, so that in all there are more than 1,100 identifiable “lost plays” from the early modern period. Lost plays offer a huge, and little-explored, network of reference points about early modern drama.
The purpose of *Lost Plays in Shakespeare’s England* is to call attention to this “invisible drama” and to offer a number of approaches to what is essentially new subject matter for early modern scholarship. Why should we care about lost plays? In the first instance, having a more accurate sense of the true scope of dramatic output in early modern London helps guard against making unjustified inferences regarding the significance or prominence of certain genres and subject matters treated by playwrights. What strikes us as dominant or frequent may in fact be an over-represented aberration. Marlowe is rightly remembered for his tragedies; what of the fact that a comedy called “The Maiden’s Holiday” was attributed to him and John Day in a Stationers’ Register entry of 1654 and in the list of manuscript plays supposedly owned by John Warburton? Conversely, a low number of extant plays on a theme may have had more numerous siblings during the period of their first production. Travel plays steeped in recent history, including *The Battle of Alcazar*, *Captain Thomas Stukeley*, and *The Travels of the Three English Brothers* appear unusual now, but were once complemented by “The New World’s Tragedy” (1595), “The Conquest of the West Indies” (1601), “A Tragedy of the Plantation of Virginia” (1623) and a play about Amboyna (1625).

Caution, and a healthy scepticism, is required though. In turning our attention to lost plays, it would not be prudent to insist on a hierarchy of values that cannot be substantiated: it may not be the case that the “best” survived and the “worst” perished. Every copy of the first quarto of *Titus Andronicus* (1594) seemed to have perished until the discovery in Sweden in 1904 of a unique copy. It cannot be inferred from this example, though, that scarcity or complete loss has a correlation to low quality or lack of popularity: *Titus* went on to be printed in two further quartos (1600, 1611) before being included in the First Folio (1623). Q1 appears to have been read to pieces rather than callously discarded. The Stationers’ Register contains many references to plays that were registered with an intention to print, but for which no extant copies survive. It would be risky, at best, to assume these were ultimately deemed unworthy of printing. Indeed, recent work in theatre history and cultural bibliography has drawn attention to the complexities of the relationship between stage and print “success” or “popularity” (however they might be defined), so that it is not entirely clear that those plays committed to print by early modern stationers would necessarily be the same plays that a twenty-first century reader might have chosen to preserve. In his chapter for the present volume, Michael J. Hirrel offers an instructive case in this regard, drawing attention to the
playwright Thomas Watson, whose widespread influence on the drama of the late Elizabethan period is evidenced from contemporary records, but none of whose plays in English survive.

Few would regard the loss of a play by William Shakespeare with ambivalence, let alone claim that its loss means that it cannot have been of much interest, yet we know of at least two lost Shakespeare plays. A play called “Love's Labour's Won” was referred to by Francis Meres in his *Palladis Tamia* in 1598 and more importantly, appears in a bookseller’s list of 1603, indicating that the play was probably in print. A play called “Cardenio” (or “Cardenno”, “Cardenna”) by Shakespeare and Fletcher was played at court in 1612–13 and registered for publication at Stationers’ Hall by Humphrey Moseley in 1653. Understandably, the majority of critical energy expended on the study of lost plays has been directed towards these Shakespearean titles. Relatively little progress has been made on “Love's Labour's Won” (perhaps it too will turn up in a Swedish cottage) beyond T. W. Baldwin's publication, in 1957, of the bookseller's list in facsimile and transcription. Various critics have offered their theories as to the “true” identity of this play, which is often assumed to be an extant Shakespearean comedy masquerading under an alternative or misremembered name. (The discovery of the second reference to this title, in the bookseller's list, ought to have ended such conjecture.) Frederic Gard Fleay nominated *Much Ado* as his preferred candidate; Felix E. Schelling thought *All's Well* a more likely fit; Chambers, writing (like Fleay and Schelling) before Baldwin published his news, suggested *The Taming of the Shrew*, a title which occurs a few lines earlier in the bookseller's list. From October 2014 to March 2015, the Royal Shakespeare Company will be performing “Love’s Labour's Won (Or Much Ado About Nothing)”.

In some ways, the logic underpinning scholarship on “Love's Labour's Won” is surprising: the general response has been to subsume the unknown title under the identity of a familiar play rather than to celebrate the possibility of a new Shakespeare play. In an article for *Early Theatre* in 2006, John H. Astington drew attention to this phenomenon in passing, referring to scholars with a desire for combining the unknown with the known as “clumpers” and those intent on preserving the discreteness of the titles as “splitters”. For scholars interested primarily in statistics, the identification of unfamiliar titles with known plays is largely irrelevant: it does not affect the sum total of all drama produced, it merely adjusts the ratio of plays chalked up in the “named” column to plays in the “unnamed” column. But for others, the bundling together of titles which may possibly refer to discrete plays poses an
immediate problem, risking the complete effacement of what is sometimes the only documentary evidence testifying to the one-time existence of a play. Is George Peele’s Edward I the same as the play referred to as “Longshanks” by theatrical impresario and manager of the Rose and Fortune playhouses, Philip Henslowe? It might be, in which case we could glimpse some of Peele’s play’s stage history; but it might not be, in which case we might draw substantially different conclusions about playgoer demand for the Longshanks story. Andrew Gurr’s chapter for this volume surveys the use of alternative or sub-titles for plays, and ponders the implications of such dual identities for the study of lost plays including “The Disguises” and “Mahomet”. Astington’s chapter develops his earlier thoughts about “lumpers” (which he notes is the more accurate terminology derived from Darwin) and “splitters”, teasing out some of the implications of these two schools of thought on the handling of historical material. Astington’s essay is also in dialogue with that of Roslyn L. Knutson, who analyses a similar habit of thought practised by earlier lost play scholars: the invocation of a fabled precursor or “Ur-play”. As Knutson notes, the rhetoric of “Ur-arguments” carries with it a characteristic freight of ideas and assumptions about early modern drama, and her chapter seeks to “understand more fully the reasoning that drives Ur-arguments” so as to “discern more clearly the boundaries between judicious conjecture and fanciful narratives”.

The lost “Cardenio” has been the subject of much controversy of late, as the sensation surrounding Lewis Theobald’s claim to have a new Shakespeare play in 1727 was somewhat replicated in 2010 by the publishers of the Arden Shakespeare series who chose to include Double Falsehood in their Shakespeare imprint rather than their Early Modern Drama series.11 If Theobald’s play were indeed based on the lost Shakespeare-Fletcher collaboration – albeit at a stage or two of removal, if a Restoration manuscript (or manuscripts) of the Renaissance play lie behind Theobald’s adaptation – it appears “Cardenio” may have been based on a subplot from Don Quixote.12 Not only do we appear to have a palimpsest of the Jacobean play, but we also appear to have an original song still extant: “Woods, Rocks, & Mountaynes . . .” by the composer Robert Johnson, who was associated with the King’s men.13 Beyond what it tells us about Shakespeare’s late style, collaborative activity, and possible interest in Cervantes, then, the “Cardenio” case is salient because it illustrates another key point about working with lost plays: new plays, or parts of plays, continue to turn up from time to time. The chapters by William Proctor Williams and Martin Wiggins which bookend this edited collection testify to the potential rewards
of diligent and dogged pursuit of leads, but also to the possibility of serendipitous discoveries – for example, Wiggins’ discovery of the “challenge” from *Prince Henry’s Barriers* just months after the Cambridge editor of Jonson lamented that “[t]he exact text of the ‘two speeches’ in which the challenge was issued is not preserved”.14

But to fixate on “Cardenio” and “Love’s Labour’s Won” is to privilege Shakespeare over his contemporaries and to retain an author-centric bias which is now looking distinctly dated. The present volume moves beyond the ostensible singularity of Shakespeare to recognize the value of attending to lost plays from Shakespeare’s England more generally. In doing so, it contributes to the growing documentation of the period’s dramatic output and builds on the critical desire for completeness, but equally it promotes the discussion of lost plays as a valuable area of scholarship in terms of their insights into the practices of playwrights and playing companies. The ephemeral nature of lost plays has prevented them from becoming the primary focus of literary scholarship as long as that scholarship is driven by formalist tendencies (since textual analysis cannot be performed without a text). In some cases, the lack of even a title has hampered discussion of these plays, which cannot easily be indexed or referenced.15 Theatre history is less troubled by such textual absences, having learned to think of early modern drama not simply as a literary text that happens to be performed, but as a complex and multi-faceted cultural phenomenon in its own right. Of founding importance in this movement has been the *Records of Early English Drama* project, rigorously and systematically transcribing local records of drama, of any sort, within a period stretching from the Middle Ages to 1642. In many cases, these records had long been known, but they had previously received only partial attention from researchers whose real interest lay in the possibility of finding references to a touring Shakespeare.16 *REED’s* interest in these records has coincided with, and helped to lead, a revival in the whole discipline of early modern theatre history, reconceptualized as a form of historicized performance studies. This new area of study is exemplified by a whole series of burgeoning related fields: company histories and repertory studies; the analysis of collaboration and revision in early modern dramatic authorship; work on the whole relationship between script and performance, stage and page; and investigation of the “historical phenomenology” of Renaissance playgoing.17

Accordingly, the very definition of a “play” has come under pressure in recent scholarship; the playtext being seen, in Tiffany Stern’s terms, as a document of performance rather than as “complete” literary work.18 If the post-publication circulation of prologues, epilogues and songs
individually of the main dramatic script frustrates any simple attempt at delimiting the boundaries of what constitutes a “play”, the existence of backstage plots, authorial plots, arguments, playbills and actors’ parts at the time of a play’s stage run makes it harder still to continue claiming that “the play’s the thing”. In his chapter for this collection, Matthew Steggle pushes this logic further, asking whether the play’s title might itself be considered a document of performance. Such a move away from an exclusive focus on the playscript has implications for lost plays, which we argue should be regarded positively as witnesses to otherwise unrecorded theatrical events rather than as mere failures to preserve a literary text.

“Repertory studies” provides an alternative to author-centric models, considering the company itself as possessing a quasi-authorial identity that unites its output and arranges its dramatic offerings strategically to attract and retain playgoer patronage. Because it emphasizes the importance of individual plays as the basic commodity of the playing companies, this branch of theatre history can produce useful insights from such fragmentary evidence as titles, eyewitness accounts, payment details and plots. From this perspective, “Love’s Labour’s Won” is of interest not because Shakespeare wrote it, but because its name suggests it was a comedy (the most prominent genre at the Rose in the 1590s, and thus possibly offered by Shakespeare’s company for prudent, market-driven reasons) and because it may have been a sequel or spin-off play, both of which were standard features of the commercial companies’ strategies in the 1590s. Shakespeare’s first and second tetralogies are evidence that he and his company saw the value in offering plays that were related to each other by subject matter or chronology; over at the Rose playhouse, Henslowe was carefully considering how and when to stage the two Tamburlaine plays, the two “Tamar Cham” plays, the two “Hercules” plays, and numerous other multi-part dramas. A unified study of a particular company’s repertory at a particular date requires integrated discussion of plays by different named authors; plays by anonymous authors; and also the lost plays that can be attributed to that company. Roslyn Knutson in particular has championed the value of attending to the role of lost plays both within a company’s repertory and as part of cross-company competition. In giving due consideration to the lost plays in the repertories of the Admiral’s and Chamberlain’s men in 1599–1600, for example, Knutson has helped put to rest the notion that the tenants of the Rose lacked the means to compete with their newly arrived neighbours across Maid Lane at the Globe. Shakespeare’s Henry V and 2 Henry IV were complemented by a
lost “Oldcastle” play in the Chamberlain’s offerings, but were met with competition from an “Owen Tudor” play and “2 Henry Richmond” at the Rose, all of which likely covered related English historical material. Looking to lost plays thus helps establish previously overlooked patterns and echoes between the offerings of the theatre companies operating in Shakespeare’s London, with the result being a more nuanced understanding of the ways companies selected plays for performance.

The extensive records of playhouse manager Philip Henslowe are an invaluable aid to studies of company commerce. Henslowe’s diary of receipts and expenses pertaining to plays in the repertory of (primarily) the Admiral’s and Strange’s men in the period 1592–1604 is a unique snapshot of the role of plays, now lost, that were once part of these companies’ repertorial offerings. The combination of new and continuing plays, the decisions about the order of performances, the patterns of staging serial or multi-part plays, and the trends in subject matter being portrayed are all important factors which are deducible from Henslowe’s documents. The absence of playtexts certainly prevents close reading, but knowledge of titles alone can yield substantial information. The chapters in this book by Misha Teramura and Paul Whitfield White both draw heavily on Henslowe’s records to establish the likely subject matter of lost plays and to understand the role of these plays in the repertory of their respective companies. Teramura recovers the probable narratives (from material available to playwrights as sources) of a group of Admiral’s Men plays from the second half of the 1590s, all dealing with episodes from Trojan-British history. White conducts a similar investigation of five Arthurian-themed plays acted by the Admiral’s Men between 1595 and 1599. When we think of English history plays in this period, we tend to think of the medieval era and Shakespeare’s tetralogies: but Teramura and White show that other periods of English national history, broadly defined, were also the subject of groups of possibly interlinked plays in a company’s repertory. Christi Spain-Savage takes her cue from a single entry in Henslowe and observes a notable shift in early modern attitudes to the innkeeper / cunning woman, Gillian of Brentford (or Brainford), which she attributes to the dramatization of this role by the Admiral’s men in a lost play of 1599. This in turn has implications for the dating of *The Merry Wives of Windsor*, the texts of which seem to register this change in Gillian’s signification.

Lawrence Manley’s contribution to this volume exemplifies how a repertory studies approach can be transformed by appropriate attention to lost plays. Very few texts survive which unambiguously belong to the repertory of Lord Strange’s Men, and those that do are complicated
by the circumstances of their preservation. By close attention to the
titles recorded by Henslowe, however, Manley demonstrates several
recurring themes and motifs across the plays they acted in 1592–3.
Repertory studies underpins, too, David McInnis’s re-examination of
“the most neglected of backstage plots”, the fragmentary plot known as
“2 Fortune’s Tennis”, associated with the Admiral’s Men and the period
1597–1602. Analysis of the characters named in that plot helps relate
this lost play to the Admiral’s investment at the turn of the century in
a kind of positive nostalgia.

Henslowe, then, is seminal to this book, but records from Stationers’
Hall are useful with respect to titles and dates too. The name “George
Scanderbeg”, found in a Stationers’ Register entry in July 1601, may not
mean much to the modern ear, but the narrative of this fifteenth-century
Albanian national hero’s resistance efforts against the Turks is recover-
able, and the frequent association of Giorgio “Scanderbeg” Castriota’s
name with that of the Scythian warlord Tamburlaine speaks volumes
about why a London playing company (the Earl of Oxford’s men) might
acquire a play on this subject.22 The title of another entry (August 14,
1600), the “Tartarian Crippell Emperour of Constantinople” (if it were
indeed a play) might fall into a similar category of Marlovian spin-offs.
None of this information is unambiguous, however, and as the essays
in this volume attest, historical records are frequently the subject of
competing interpretations.

So far, we have argued that study of lost plays is timely because of
changes in how we think about early modern drama, in particular
because of a shift towards repertory studies in various different manifesta-
tions. But it is also timely because a new generation of scholarly resources
enables fresh progress on the interpretation of records of lost plays.

These resources come in various forms. The primary documents
themselves are more accessible than ever before, thanks to work such
as the ongoing Henslowe-Alleyn Digitization Project under the leader-
ship of Grace Ioppolo. This project offers not just new scholarship
on the Henslowe papers as a whole, but also full digital images, view-
able through a web browser.23 A second major resource for lost plays,
Sir Henry Herbert’s Office-Book, has been re-edited by N. W. Bawcutt
from the surviving transcriptions, with Bawcutt adding many hitherto
unknown records to this important but fragmentary document.24 Yet a
third major resource in this area, the Stationers’ Register, still awaits a
new edition to replace the invaluable but cumbrous nineteenth-century
transcription of Edmund Arber. But the fact that Arber’s edition, too,
is now available in digital format makes it much easier to work with.25
As for secondary material, the encyclopedic work of Chambers, Bentley, Greg, Harbage and the like remain part of the reason that new progress can now be made. Martin Wiggins’ ongoing *British Drama* project, which supplements these seminal texts with new research, aims to produce a comprehensive catalogue of extant and lost plays from the period, and is already changing the overall picture of early drama and providing a new platform on which to build. Here, too, fits the *Lost Plays Database* (http://www.lostplays.org), a website established by Roslyn Knutson and David McInnis as a clearing-house for information specifically about early modern plays whose playscripts do not survive. The *Lost Plays Database* already lists over seven hundred lost plays from the period 1570–1642, with entries completed or in progress for some 250 or so titles. Knutson and McInnis observe that “knowledge of lost plays expands the fields both of repertory and cultural studies”, and have deployed a wiki-based format to facilitate collaborative scholarship in a bid to record and collate as much information about lost drama as can be found. Many of the chapters in this volume arise, directly or indirectly, from contributions to the *Lost Plays Database*, and one example of this is Christopher Matusiak’s essay on lost stage friars. Matusiak collates the known evidence about lost friar plays including “fryer fox & gyllen of branforde” (1599) and “friar Rushe & the prowde womon” (1602), making connections between them in the course of an argument whose implications reach out to much more obviously canonical plays including *Doctor Faustus* and *Measure for Measure*. The format of the *Lost Plays Database* facilitates such work of comparison and categorization.

But perhaps the most striking new tool for analysis of lost plays is provided by the range of electronic corpora now available, including *Early English Books Online; Literature Online; Eighteenth-Century Collections Online*; and *Google Books*. These four projects have very different parameters, and different strengths and weaknesses as research tools. But all four enable rapid word searches of very large corpora, permitting – in effect – very detailed analysis of tiny fragments of plays. Much of the research this volume contains would scarcely have been possible without recourse to such databases. Names, in particular, are susceptible to “brute force” electronic searches across large corpora. Jonathan Hope and Michael Witmore have coined the useful phrase “prosthetic reading” to emphasize the ways in which a computer search of a corpus differs from a conventional exploration of a book:

Strangers to saliency, computers treat all pieces of information equally: they are just as aware of the 609 usages of the word ‘the’ in *Macbeth*
as the 35 uses of ‘hand’. To humans trained in the western literary tradition of reading for ‘understanding,’ this may seem a pretty useless ability – even perhaps a blindness . . . 27

In the case of lost plays, there is an obvious utility to this mechanical approach to searching. An EEBO-TCP search, for instance, runs indefatigably and impartially through all the 30,000 texts in its database, even the tedious or obviously irrelevant, searched with equal diligence from beginning to end. In the process it may find the two or three occurrences which enable an identification of the name under discussion.28 For instance, three of the strangest play-names recorded by Henslowe are “Albere Galles”, “Sturgflaterey”, and “Felmelanco”. In the last three years, electronic searches have contributed to new and convincing solutions for the first two of these mysterious titles.29 The third – “Felmelanco” – remains as yet unsolved. Similarly, there remain many other problems which look as if in time they ought to yield to sufficiently detailed electronic searches. Who was Friar (S)pendleton, the eponymous hero of a play mentioned by Henslowe? What was the Bristol tragedy? What happened in “The Tanner of Denmark”? Surely, among the many unanswered questions in play titles there lurk some that are still soluble.

The essays in this collection ask us to reconsider our most basic assumptions about the nature of dramatic activity in Shakespeare’s London; they complicate our understanding of what constitutes a “play”, and invite us to explore a vital and vibrant context which has for the most part remained bogged down in tenuous conjecture or has simply been ignored. One note sounded in several of these essays, in different and sometimes competing terminology, is the idea that lostness is a continuum, not an absolute state, and that valuable things can be said about plays which do not survive in a main playtext. Another repeating motif is the axiom that more remains to be discovered – in relatively inaccessible archives; in easily accessible ones; in printed sources; and in electronic databases. Pursuing these leads will of course entail a host of important considerations about the responsible handling of historical evidence, the nature and function of criticism, and the process by which even well-meaning conjecture can ossify into citable “fact” in the absence of a certain frankness and skepticism. And it will almost certainly involve a greater degree of collaboration between scholars who recognize that the sum total of such ostensibly ephemeral inquiries exceeds the constituent parts. It is our hope that by raising the profile of lost plays as an area with significant research potential, this book will
build on the work of the Lost Plays Database and Wiggins’ Catalogue and encourage scholars to devote attention to a largely overlooked context for the plays that do survive from Shakespeare’s England.

Notes


4. See the entry for “The Maiden’s Holiday” in the Lost Plays Database, ed. Roslyn L. Knutson and David McInnis (Melbourne: University of Melbourne, 2009+), www.lostplays.org. It may be worth recalling that the clowning scenes from Tamburlaine were deliberately omitted from the published version by the printer Richard Jones (see “To the Gentlemen Readers . . . ” in Marlowe, Tamburlaine the Great [London, 1590], sig.A2).


7. See the entries for both these plays in the Lost Plays Database.


12. Theobald claimed that one of the manuscripts in front of him as he wrote Double Falsehood was in the hand of John Downes, a prompter for Davenant’s company (Duke’s) in the Restoration (see Hammond, ed. 3). A notable objector to the Don Quixote possibility for the lost play’s subject matter is
Tiffany Stern; see her “‘The Forgery of some modern Author’?: Theobald’s Shakespeare and Cardenio’s *Double Falsehood*,” *Shakespeare Quarterly* 62:4 (2011): 555–93.


15. For examples of untitled lost plays, see the *Lost Plays Database* entries for “A Huntsman in Green Apparel”, “A King with His Two Sons”, “Amboyna”, and others in the “Untitled plays” category.


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