REDEFINING BRITISH THEATRE HISTORY
GENERAL EDITOR: PETER HOLLAND

PLAYERS, PLAYWRIGHTS, PLAYHOUSES

Investigating Performance, 1660–1800

Edited by
MICHAEL CORDNER and PETER HOLLAND

Redefining British Theatre History

General Editor: **Professor Peter Holland**

Redefining British Theatre History is a five-volume series under the general editorship of Professor Peter Holland. The series brings together major practitioners in theatre history in order to establish ways in which previous assumptions need fundamental questioning and to initiate new directions for the field. The series aims to establish a new future for theatre history, not least by making theatre historians aware of their own history, current practice and future.

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Series Standing Order ISBN 978-0-333-98219-8 **(Hardback)** 978-0-333-98220-4 **(Paperback)**
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and

Peter Holland

Redefining British Theatre History Series
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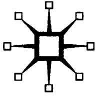

Introduction, selection and editorial matter © Michael Cordner and Peter Holland 2007
Individual chapters © contributors 2007
Softcover reprint of the hardcover 1st edition 2007 978-0-230-52524-5

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First published 2007 by
PALGRAVE MACMILLAN
Houndmills, Basingstoke, Hampshire RG21 6XS and
175 Fifth Avenue, New York, N.Y. 10010
Companies and representatives throughout the world

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ISBN 978-0-230-25057-4 ISBN 978-0-230-28719-8 (eBook)
DOI 10.1057/9780230287198

This book is printed on paper suitable for recycling and made from fully managed and sustained forest sources. Logging, pulping and manufacturing processes are expected to conform to the environmental regulations of the country of origin.

A catalogue record for this book is available from the British Library.

A catalog record for this book is available from the Library of Congress.

10 9 8 7 6 5 4 3 2
16 15 14 13 12 11 10 09 08

Transferred to Digital Printing 2012.

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Series Introduction: Redefining British Theatre History

Peter Holland

On the surface, it doesn't look like much of a problem: conjoining the two words 'theatre' and 'history' to define a particular practice of scholarship has a long and illustrious history. Nor does it appear to over-complicate matters to add the word 'British', for all that the word is so furiously questioned at different moments of history (and especially at the moment). Yet what kind of history theatre history is and what kind of theatre theatre history investigates, let alone what the Britishness is of its theatre history, is endlessly problematic. For all the availability of shelves full of the outcomes of its practices, theatre history is in need of a substantial reassessment. This series is an attempt to place some markers in that vital project.

It is hardly as if theatre history is a new area of scholarly enquiry and academic publication. Within a general, varyingly academic mode of publication, one could point, in the UK, to the longevity of *Theatre Notebook*, a journal founded in 1945 by the Society for Theatre Research; its subtitle *A Journal of the History and Technique of the British Theatre* neatly sets out its scope and the assumed scope of theatre history. A number of US journals have had similar concerns, including *Theatre Survey* (from the American Society for Theatre Research) and more narrowly defined examples like *Restoration and Eighteenth-Century Theatre Research* or *Nineteenth-Century Theatre Research*. Lying behind such work is the complex institutional history of the formation of university drama and theatre departments on both sides of the Atlantic and their vexed and often still unformulated connection both to theatre training (the university as feed to a profession) and to departments of English Literature.

For the early modern period theatre historians might chart the subject's early twentieth-century history as being encapsulated by the work of E. K. Chambers (especially *The Elizabethan Stage*, 4 vols [Oxford: Clarendon Press, 1923]) or G. E. Bentley in his continuation (*The Jacobean and Caroline Stage, 7* vols [Oxford: Clarendon Press, 1941–68]), phenomenal individual achievements of documenting theatrical events, theatre performers and theatrical contexts. Their work might be matched for a later period by, say, E. L. Avery et al., eds, *The London Stage 1660–1800*, 11 vols (Carbondale, Ill: Southern Illinois University Press, 1960–8) or Philip Highfill, Kalman Burnim and Edward Langhans, eds, *A Biographical Dictionary of Actors, Actresses, Musicians, Dancers, Managers and Other Stage Personnel in London, 1660–1800*, 16 vols (Carbondale, Ill: Southern Illinois University Press, 1973–93). Further back still comes the fundamental work of such people as Boaden (*Memoirs of Mrs Siddons*, 2 vols [London, 1827]) and Genest (*Some Account of the English Stage*

from the Restoration in 1660 to 1830, 10 vols [Bath, 1832]), who saw themselves neither as scholars nor as academics and yet whose work implicitly defined the accumulative function of data collection as a primary purpose of theatre history. Behind them comes the achievement of the greatest of eighteenth-century editors of Shakespeare, Edmond Malone.

Yet, seeing that there is a practice of theatre history is not the same as understanding or theorizing such a project. While many academics are engaged in the practice of something they would unhesitatingly term 'Theatre History' and while they would differentiate it carefully from a variety of other contiguous fields (e.g. performance theory or history of drama), there has been remarkably little investigation of the methodological bases on which the shelves of accumulated scholarship have been based or the theoretical bases on which Theatre History has been or might be constructed. Even within organizations as aware of the need for theoretical sophistication as IFTR/FIRT (Fédération Internationale pour la recherche théâtrale) the emphasis has been placed more squarely on performance theory than on the historiographical problems of theatre. In part that can undoubtedly be traced to the disciplines or institutional structures out of which the work has evolved: one would need to examine its early and still troubled connection to literary studies, to the analysis of drama and, most visibly, to the study of the history of Shakespeare in performance or, on another tack, to consider the ways in which theatre departments have structured their courses in the US and UK.

By comparison with the traditionally positivist accumulation of data that marks, say, *Theatre Notebook*, one could, however, see signs of the emergence of a new concern with the processes of historiography as it affects the specific study of a massive cultural institution like theatre in, to take just one significant example, the collection of essays edited by Thomas Postlewait and Bruce McConachie, *Interpreting the Theatrical Past: Essays in the Historiography of Performance* (Iowa City: University of Iowa Press, 1989). But while individual theatre historians are demonstrating an expanding awareness of the specific areas of historiography relevant to their work (e.g. economic history) and while theorizing of performance including its historical traces has grown immensely over the past 15 years, there is little enough to set out on a large scale the parameters of something that might hope by now to see itself as a discipline. The shelves of libraries and bookshops and the reading lists of courses do not show major resources for understanding what theatre history is, while an unending stream of books offering to help students understand the history of theatre pours from presses. In part this may be connected to the absence of departments of theatre history and the further substantial absence, within theatre departments, of courses concerned to do more than teach theatre history as an assumed and shared methodology based on an acceptance of what constitutes evidence and of how that evidence generates the potential for meaning.

Redefining British Theatre History sets out, extremely ambitiously, to make a major statement by bringing together, in the course of its series of five volumes, some fifty major practitioners in theatre history in order to establish ways in which

previous assumptions need fundamental questioning and in which a future for the field can be enunciated in modes as yet undervalued. It aims to be a significant review of where we are and what we think we are doing.

The project began from an unusual collaboration between research library and publisher. My gratitude goes first and foremost to Dr Roy Ritchie of the Huntington Library and Josie Dixon of Palgrave Macmillan for contacting me to see whether I would develop a proposal that would create a series of conferences and subsequent volumes based on a single theme. Their support, not least financial, has been crucial in bringing the project to a reality both in the pleasures of the conference and the creation of this book. If we succeed, *Redefining British Theatre History* should chart the beginnings of a new future for theatre history, not least by making theatre historians newly and self-consciously aware of their own history, their practice and their future.

Introduction: Expanding Horizons

Michael Cordner

Theatre historians who work on the long eighteenth century enjoy primary resources which colleagues who specialize in earlier periods can only envy. It is true that, as Robert D. Hume remarks in this volume, a scholar working on the 1780s–90s is faced with a plenitude of different kinds of documentary evidence unavailable to one whose research focuses on the theatre either side of the 1688 Revolution. But, from the perspective of pre-1642 theatre history, the 1660s onwards afford a richness of materials (including eye-witness reports) and information (about, for instance, performance dates and casting), for which the earlier period provides no equivalent. With this relative wealth, however, come challenges and snares. Possessing the diary, for example, of an ardent and opinionated theatregoer like Samuel Pepys for the first decade of the reopened playhouses is an extraordinary boon. But it can lure the unwary into treating the testimony of this most obsessively idiosyncratic of observers as if it offers us unmediated and infallible access to majority opinion among the variegated array of fellow spectators with whom he patronized the two, newly established monopoly theatres. Deriving maximum benefit from the invaluable record he has left us, while avoiding reifying the rest of the audience into his clones, demands delicate calculations.

Similarly, the resources made available by two magnificent achievements of theatre history in the last century, *The London Stage 1660–1800* and the *Biographical Dictionary*,[1] stand dauntingly on the shelves, often referred to, but rarely read with the care and attention they demand, assumed instead to offer positivist and transparent information about the day-to-day performance calendar and the careers of theatre workers across the period. We may have conspicuously more to deal with than our colleagues working on early modern theatre; but what have we done with what we have? And what should we be doing now?

Hume's contribution to this collection is much preoccupied with problems of this kind. He identifies opportunities galore for innovative contributions to scholarship, but is also critical of much contemporary scholarly practice in the field and laments what he sees as unadventurous or misguided use of those resources which previous tillers in the archives have already placed at our disposal. The theatre history for which he legislates is one where 'the ultimate objective is illumination of plays in their theatrical, social, and political contexts'; and, as he surveys the current fruits of research, he discovers relatively little which fulfils

his ideal vision of what this definition might/should in practice produce. His analysis is accordingly peppered with statements which begin with variations on the lament that 'no one has ever attempted' one of a dozen or more eminently accomplishable projects which he then proceeds to specify. The positive aspect of Hume's discontents is, therefore, his recurrent insistence on all that remains to be achieved, whether in the reinvestigation of terrain already provisionally but unsatisfactorily mapped or in the exploration of performance arenas – as, for instance, the theatre of the fairs – which have so far received very little systematic attention. Not every hare he starts is pursued in the subsequent essays in this volume, and most of our authors occupy methodological and theoretical positions radically disjunct from Hume's own;[2] but their essays are all characterized by a matching desire to expand the boundaries of theatre history's inquiries in the long eighteenth century.

My own contribution, which is the only one in the collection focused on Restoration theatre, seeks to do this by resituating one of Aphra Behn's Exclusion Crisis comedies, *The Roundheads*, in a longer history which includes Civil War and Protectorate polemic and vigorous and scurrilous traditions of sexual slander which reach further back still. In the process it seeks to override the barriers which have traditionally separated the pre-1642 and post-1660 theatres into firmly demarcated academic specialisms, with little constructive dialogue occurring between them. Behn constantly drew inspiration from other playwrights' work, including scripts from more than half a century earlier, but, in the process, she also imprinted what she adopted with her own concerns and distinctive invention. Charting how this general proposition works in the particular case of *The Roundheads* reveals a dramatist alert to the pressure of earlier crises upon the plays to which she was indebted for provocation and inspiration, and also canny and imaginative in remodelling the latter in response to the convulsive political crisis which was still unfolding as she put pen to paper, and which was itself deeply inflected by the mid-century upheavals from which her source-texts had themselves derived. One of the most productive ways forward for Restoration theatre history may be to look backwards with a renewed curiosity and vitality of purpose.

Paula Backscheider's essay extends our vision in a different direction – to the substantial, richly suggestive, yet largely unexplored, engagement of the eighteenth-century novel with the theatre. The scale and range of the material identified reaffirms, in her view, the extent to which drama was, in this period, 'the dominant genre' and how thoroughly knowledge of it informed other modes of writing. The instances Backscheider cites notate and interrogate audience behaviour, generic histories, and performance styles across the century. She discerns a clear evolution in the ways in which such in-set scenes can be interpreted as seeking to shape and refine theatregoers' taste and connoisseurship, and illustrates an increasingly detailed and sophisticated alertness in them to the contrasts between the techniques and accomplishments of leading players. Spectators of the 1670s doubtless debated the relative merits, for instance, of Thomas Betterton and Charles Hart in rake roles; but it is only in the new century that such aesthetic controversies leave textual records behind them. Backscheider's analysis opens up an enormously fertile area of investigation for future theatre historians.

Judith Milhous introduces us to even more uncharted territory. She modestly remarks that the documentary evidence with which she is dealing 'requires extensive processing'. The devoted care and technical agility with which she solves the interpretative challenges posed by the surviving playhouse account books and related financial documents are both exemplary and revelatory, an exciting rethinking of primary documentation, much of which she has been responsible for identifying. Her discussion documents clearly how little solid information we have previously possessed about 'the internal dynamics of each company' and how often conventional preconceptions about crucial issues have been mistaken – for instance, the belief that writers producing 'formula' product for the stage in the eighteenth century could earn a living wage from that source alone. Her pioneering research brings fresh definition to key moments – for instance, the astonishing initial success and extended opening run of *The Beggar's Opera* – but it also contributes crucially to establishing the secure foundations from which a radically innovatory history of eighteenth-century theatre must in future be constructed.

Lisa Freeman and Matthew Kinservik retraverse more familiar ground, but in ways which open up distinctive new perspectives upon it. Both are concerned with the period's campaigns for theatrical reform and focus, respectively, on the two key events on which histories of that subject have tended to concentrate – the Collier Controversy at the close of the seventeenth century and the 1737 Licensing Act. Analyses of the former have usually prioritized its moral and aesthetic dimensions and after-effects. In contrast, Freeman seeks to develop an understanding of the political quarrels which were also, in her reading, being fought out through the paper warfare and treats the individual pamphlet contributions as 'cultural performances in themselves that need to be read and interpreted', with each of them promulgating its 'own readings of history' and its 'own articulations of the body public'. Ardent polemicists espousing a common anti-theatricalism can thus be revealed as yet irretrievably opposed in the way they understand the legacy and implications of the Civil War, while one deft strategy for defending the theatre's indispensable value to society is shown to be inspired by 'a desire to leave behind' the 'legacy of violence and upheaval' repeatedly recycled by such obsessive retrospection.

Kinservik argues an equally revisionist case. With an abundance of evidence, he demonstrates an extensive, but until now completely neglected, 'tradition of philo-regulatory schemes' in the decades preceding the 1737 Act – that is, expansively argued theorizing by pro-theatre writers which regrets the contemporary state of the drama, attributes much of the blame for its imputed decadence to the playhouses' dependence upon the vagaries of audience taste and the market, and recommends the firm intervention of the state to impose higher expectations, stringently monitor theatrical output, and offer rewards to those playwrights who can meet the demanding standards thus set. A fundamental premise in such polemic is the credo that 'a commercial stage is incapable of producing quality drama' – a belief which, Kinservik argues, also underpinned the later campaigning for a British National Theatre. He identifies modern assumptions that freedom of expression is a primary value as anachronistic when applied

to the eighteenth century and illustrates how easily government arguments in favour of the Licensing Act could draw sustenance from this widely disseminated 'philo-theatrical' tradition.

Michael Dobson's contribution shifts attention from the world of exclusively professional performance to that of amateur dramatics, a milieu to which recent – and indeed earlier – scholarship has paid disappointingly little attention. He underlines the contrast between the industriousness with which non-professional performance has been documented and analysed in the pre-Civil War period and the relative lack of interest shown in the much more voluminous evidence of such activity surviving from the eighteenth century. His discussion is throughout characterized by a keen pleasure in bringing back into public view an array of fascinating stories and events. He gives special prominence to 'the boom in non-professional drama in the 1770s and 1780s', maps the variety and complexity of the ways in which professionals and amateurs interacted, and sketches the arguments which contemporary critics of amateur drama deployed. His exploration climaxes with a detailed account of a double-bill, mounted in 1774 by an all-female cast in the Cathedral Close at Salisbury, and the waves it created. Like Backscheider's essay, Dobson's opens up fertile terrain which invites further inquiry.

His concerns are also with theatre outside London, what was once disdained as 'provincial'. With the chapters by Susan Cannon Harris and Helen Burke the spotlight moves to the Irish theatre and Irish playwrights, a challenge to the exclusivity of conventionally articulated British focuses on theatre in the period. Harris tracks in exhaustive and revelatory detail the intricate mutations which overtake the script of Thomas Sheridan's *The Brave Irishman* across sequential performances in Dublin and, subsequently, London. Already itself an adaptation of a French original, which had undergone earlier reinventions and re-domestications before Sheridan decided to annex it, *The Brave Irishman* proves, in Harris's analysis, subtly responsive to changing political and theatrical circumstances. Her analysis makes finely honed deductions about the fit which can be observed between its differing versions and the moment and location of their first performance, and in the process closely maps the shifting, multiple implications of its marriage plot and of its strategic manipulation of competing, contemporary stereotypes of the Irish male.

Helen Burke's essay also moves between Ireland and England, but she adds a second polarity by querying the way in which 'a town/country opposition' has 'served as a key structuring and delimiting concept' in the construction of 'eighteenth-century Irish and British theatrical archives' and, therefore, in the scholarship derived from them. Theatrical historiography has thus concentrated on urban locations, while non-urban performance practices have become the preserve of other kinds of experts. Burke's examination both of the behaviour of Dublin audiences in the eighteenth century, as heavy migration from the countryside took place, and of the provocations and influences which generated *She Stoops To Conquer*, calls such a separation emphatically into question. Goldsmith's masterpiece emerges in a decisively new light once it is returned, as it were, to

the Irish countryside, to which, in this account, it owes its birth, and to which, in another sense, it was also fated to return, via John O'Keeffe's itinerant impersonations of Tony Lumpkin.

In her contribution Mita Choudhury draws on the theoretical writings of Judith Butler to challenge some of the conventional assumptions frequently encountered in narratives of the development of eighteenth-century acting, which claim to identify substantive transformations in contemporary representations of such figures as Shylock, Othello, and Oroonoko. Her argument ranges across the whole period, but also looks forwards to Biyi Bandele's 1999 dramatization of the Behn novella for the Royal Shakespeare Company. She seeks to anatomize the status (and consequences) of the 'theoretical principle' of 'universality ... as a prescriptive rule in the theatre' and detects a consequent inveterate 'tendency toward the normative' in performance practice, which makes her regard with intense scepticism the claims which have been made about a novel realism or a paradigm shift in the theatrical representation of 'the other' via, for instance, the artistry of David Garrick.

Peter Holland's essay also focuses on this greatest of eighteenth-century actors, but asks very different questions of, and about, him. Observing with regret how preoccupied theatre history is with 'the visual rather than the aural', he seeks to begin to redress the balance by exploring how much, if any, evidence can be retrieved about how Garrick sounded. The answer turns out to be: a great deal. By deploying material from the Garrick bibliography to which previous investigators, including the actor's biographers, have granted scant, if any, attention, Holland lays before us a mass of detailed testimony – not all of it flattering – about his selection and deployment of inflection, phrasing, pause, timbre, and tempo in particular passages. From this he gradually educes a sense of the artistic priorities which inform the choices these witnesses record, but also leaves us with the conviction that we too 'start to hear' Garrick 'better, not yet clearly but no longer quite so inaudible'.

Where Holland's project rests on mobilizing and using to best advantage previously unidentified material, Shearer West concludes the volume by asking how best we might interpret and deploy a kind of evidence ubiquitous in the period (especially its later decades), the decoding of which, however, poses many problems – the 'healthy quantity of portraits of actors and representations of the stage' which the eighteenth century has bequeathed to us. She resists the naive impulse to read such 'images as documentary and revelatory', explores the 'enhancement of the visual sphere' from the mid-century onwards, and seeks to develop 'more nuanced ways' of conceiving the multiple forms of spectatorship this enhancement fostered, and on which it depended. She maps the differences between watching actors in performance and viewing static images of them, but is also alert to the ways in which the two experiences can overlap and interrelate. She then exemplifies, via a series of case-studies, the intricacy of signification discernible in, for instance, Reynolds's famous portrait of *Mrs Siddons as the Tragic Muse*. Here, as throughout the collection, there is evidence aplenty of rich, new possibilities opening up for the study of British and Irish theatre in the long

eighteenth century, ways of thinking that are designed to provoke and question, challenge and reorient, so that those archival riches and their modern printed representations can start to show us the world of performance anew.

Notes and references

1. See E. L. Avery et al., eds, *The London Stage 1660–1800,* 11 vols (Carbondale: Southern Illinois University Press, 1960–68), and Philip H. Highfill et al., eds, *A Biographical Dictionary of Actors, Actresses, Musicians, Dancers, Managers and Other Stage Personnel in London, 1660–1800,* 16 vols (Carbondale: Southern Illinois University Press, 1973–93).
2. See, for instance, Robert D. Hume, *Reconstructing Contexts: the Aims and Principles of Archaeo-Historicism* (Oxford: Oxford University Press, 1999).

Part 1

Drama, Theatre, and History

1

Theatre History, 1660–1800: Aims, Materials, Methodology

Robert D. Hume

'Theatre history' is a discipline much practised but severely under-theorized. Astonishingly little has been written about what the theatre historian is to try to do, how it is to be done or why it is worth doing. Collecting evidence about the theatrical past has been done and can be done, but to what end? With what aims and according to what principles? We now work in a postpositivist world and we cannot simply assume that cheery antiquarianism is a thing good in itself. In this essay I want to address both some general questions about the discipline and some very specific ones about the problems and possibilities of working in the long eighteenth century. A great deal of scholarship has been published in this area during the last sixty years. Surveying what has been accomplished since 1945 from the vantage point of 2005, I am struck by how much of it is good, but also by how patchy and limited a lot of it is. Investigating what has been done in such realms as texts, performance records, performers, physical production circumstances, economics, socio-political contexts and audience responses, I find myself forced to admit that theatre history is a badly balkanized field. Scholars have mostly been unadventurous and unimaginative – one could say timid. Singularly poor use has been made of *The London Stage* and the *Biographical Dictionary*. Fundamental differences in the practice of theatre history between the late seventeenth century and the later eighteenth century have been little understood and have received almost no comment from either practising theatre historians or theoreticians of historiography. I shall argue that we need to get out of our ruts and make more imaginative use of the evidence available to us. Theatre history is wide open for transformational changes, both within this period and more broadly. Indeed, I shall make the claim that the objects of theatre history need to include kinds of interpretation rarely practised within this discipline.

I. Defining 'theatre history'

I must commence by attempting a bit of clarification and disentanglement. Scholars from many disciplines and with wildly varying interests make use of drama and theatre in their work. 'Theatre history', 'theatre studies', 'drama criticism', 'biography', 'literary history', 'cultural history', 'cultural studies' and

'performance studies' (among others) overlap in messy ways and sometimes seem radically contradictory in their aims, methods, rules and conclusions. My concern in the present essay is almost entirely with the study of scripted performances in public theatres (mostly in London) during the long eighteenth century. In a present-day world in which 'theatre historians' may deal with pantomime, music halls, vaudeville, minstrelsy, celebrity culture and the concept of 'performance' as applied to almost anything, my angle of vision is deliberately quite restrictive. I believe that there is plenty of room for all sorts of scholarship and criticism on an enormous diversity of subjects. Freely granting that other enterprises have their own aims and rules, I am trying to suggest that a lot of exciting work remains to be done in the historical investigation of English theatre and drama 1660–1800 – and to establish some basic ground rules for 'good practice' in this realm. This said, I want to address a very particular problem: how should we conceive the aims and subject of this sort of history?

We must address two fundamental questions: First, what is 'theatre history' a history *of*? And second, what exactly do we mean by 'history'? The problem of subject is by now a chestnut. Almost all practitioners concentrate either on *plays* or on *theatres, actors and production circumstances*. These focuses are understandable in practical terms, but unsatisfactory. Drama and theatre are not the same thing, but they cannot be readily separated. Plays can be studied in isolation by drama historians, though the results are sometimes more than a little peculiar, given that the success and impact of the plays depends heavily on performance. 'Drama history' normally consists of chronological consideration of *new* plays, which is methodologically ludicrous.[1] After the early 1660s the vast majority of performances in London were of *old* plays (many of them very old plays), which often had far more influence than new ones (most of which quickly failed and disappeared). Plays were usually written with particular performers in mind, a circumstance that strongly affected their structure, design, and content. A play analysed as a play must be considered in light of its performance circumstances. One can treat play scripts as literature in purely verbal terms, but this takes us out of the realm of 'history' and into that of 'literary criticism'. The latter has its uses and virtues, but it is essentially a different discipline.

Drama historians often want to remove their subject from the grubby particularities of performance circumstances. Contrariwise, theatre historians have often tended to avoid the subject of plays, odd though that may seem. (An eminent historian of the theatre in this period once said to me, 'How can you bear to read those awful plays?') Important scholarship can admittedly be done without much reference to plays. Leslie Hotson's *The Commonwealth and Restoration Stage* (1928), for example, hardly mentions them at all, though it is an admirable and important book. Theatre historians are of course right to say that studying the plays without attention to the buildings in which they were performed and the physical circumstances of performance is misguided. Yet to study theatre architecture, stages, machinery, scenery, lighting, and costumes purely as ends in themselves seems perverse. I grant that most plays in our period have relatively limited 'literary' value and even the best suffer in comparison with Shakespeare.

I would take the position, however, that if the plays are of no interest, then neither are the theatrical circumstances in which they were produced. We are dealing with commercial theatre scripts, which have their limitations but also their own performance pleasures and considerable value as cultural studies artifacts.

By way of a first principle, I will offer the thumping cliché that 'theatre' and 'drama' need to be studied together. Often said, seldom done. Of course one may legitimately work on lighting, just as one may legitimately study gender implications in play texts, but the conceptual split between text and performance is ultimately not admissible. For a literary critic, perhaps. For a theatre historian, no. Consequently we need to commit to an *integrative* rather than a *separatist* model of scholarship. Of what this means in practice, more in due course.

My second basic question about 'theatre history' concerns the nature of the enterprise. In my view, 'theatre history' can legitimately cover such matters as plays, production circumstances, and the socio-political contexts in which the plays were written and performed. These are appropriate subjects for 'theatre history' – things it can be *about* – always supposing that relevant evidence can be found. Well and good, but just what do we mean by 'history'? Do we mean any form of antiquarian investigation in which the scholar digs up such evidence as may be found about the past (i.e. 'historical scholarship')? Or does 'history' imply or require a sequential narrative? Theatre historians often eschew narrative; drama historians normally insist upon it – though new plays taken in chronological order have no necessary connection to one another. We can legitimately write the life of an actor (Garrick) or the story of a company (Drury Lane under the triumvirate management, 1710–32), but if our subject is drama, then what constitutes the basis of our narrative? 'Literary history' is often written as though plays, poems and novels are self-propagating animals obeying some sort of Darwinian rules, but this is a metaphor, not a fact.[2] Serious historiographers have generally concluded that history is best conceived as 'problem solving', not as 'story-telling'.[3] I agree. Narrative is by no means *verboten*, but narrative is not the point of our enterprise.

Having rejected the text/production dichotomy and a 'story' basis for theatre history, I want at this juncture to leave definitional generalities and turn to three more specific methodological issues. These are the implications of postpositivist principles, evidentiary constraints, and the haunting problem of 'enforced omission'.

(1) *Postpositivism*. The Bad Old Positivist Horse is long dead; it has been flogged to bloody mush; and no great virtue now attaches to one's pious declaration of loathing for the beast. Some left-over positivists (mostly of a closet variety) could still be found forty years ago and no doubt there are even now some innocents who simply pay no attention to the philosophical underpinnings of intellectual constructs. Basically, however, we all now know that objectivity is essentially delusory and that 'facts' do not have significance in themselves (etc. etc.). Fine. We are all postpositivists now. What are the practical and operational implications of this state of affairs for the theatre historian?

Collecting facts is now understood to be insufficient in itself. Contrariwise, however, we need to remember that collecting facts (or perhaps we should say 'data') is not a bad thing. One might with some justice argue that if someone does not gather factual information then we have no solid foundation on which to base any kind of historical investigation. If we do not possess data verifiable to some degree, then what we write is basically going to be fantasy fiction. This may be brilliant, amusing, and happy-making, but is it theatre history? Rejecting positivism is no ground for glorifying sloppiness, inaccuracy, and uncritical extrapolation of conclusions not subject to any form of proof. The inadequacies of positivism do not justify our using any ex-post-facto anecdote that happens to suit our prejudices.[4] By any logic I can find, postpositivism is a ground for demanding greater rigour in assembling and testing evidence, not for abandonment of all concern with its verification.[5]

Claims for a new, postpositivist theatre history were conspicuously mounted twenty years ago in an important essay by Bruce McConachie.[6] His biting analysis of Oscar G. Brockett's abominable *History of the Theatre* textbook (originally published in 1968 and still widely in use today in later editions) was admirable and overdue; his marxist demands for 'socio-criticism' made sense then and still do now. Viewed in retrospect, however, the essay does not get very far into the business of establishing a new set of principles for theatre history. The central thrust of the piece is actually a claim for the importance of scholarly attention to the social milieus in which plays were written, performed and received.

Tidy pigeonhole construction is not my ambition here, but McConachie's passionate and quite legitimate insistence on the importance of socio-political criticism invites some reconsideration of how we can most fruitfully conceive our enterprise(s). To define 'theatre history' as the collection of data and the reconstruction of performance conditions is excessively limiting and trivializing. At the other extreme, utilizing plays and performance as evidence for broader kinds of cultural studies analysis (which is where McConachie seems to be heading) takes us into markedly different kinds of projects. One of my fundamental points in this essay is to insist that *Interpretation of texts within their historical performance circumstances is an appropriate occupation for a theatre historian.* Where texts are being analysed for present-day literary meaning or dramaturgical potentialities, we are perhaps more in the realm of critical analysis than 'history'. There are no precise boundaries here and trying to impose them is misguided. We may usefully recognize, however, that different enterprises have different aims and rules.

I would suggest that one crucial function of the theatre historian is *To demonstrate how production and performance circumstances affected the writing and public impact of plays.* Depending on the availability of evidence, this may involve study of architecture, acting style, scenery, lighting, costume, the sociology of playwriting, company ownership and management, ticket prices, audience demographics, censorship, repertory, current events, and critical precepts of the time.[7] The theatre historian will always hope to be able to document the spectrum of public response to plays, performance and performers. He or she has the right – indeed, the obligation – to analyse plays and their performance in ways that help

us comprehend why they were written, performed and received as they were. Textual analysis is an entirely legitimate part of theatre history and so is the kind of ideological and political analysis that treats the intellectual *content* and implications of the play as well as its genre and performance history. I would argue, however, that when we start to analyse a script towards present-day performance we are starting to undertake a different function. And when we use plays as evidence for cultural studies analysis (however historical in site), then we are definitely putting on a different hat and engaging in another kind of enterprise.

An essential difference among the three – theatre history, dramaturgical analysis and cultural studies – is that they have radically different claims to truth and different forms of verification. Theatre history, if it is anything but a self-indulgent game carried on for the self-aggrandizement of the historian, attempts to be *true*, to arrive at conclusions that will stand up under serious scrutiny and meet severe tests of evidentiary interpretation and challenge. Dramaturgical analysis makes no such claims: if an interpretation is effectively producible then it possesses a kind of legitimacy, even if the playwright never thought of it and the script has never yet been performed that way. Ernest Jones's Oedipal interpretation of *Hamlet* is a famous example. Cultural studies is another matter: such 'proofs' as can be offered of its conclusions are rarely of a sort even potentially susceptible of rigorous demonstration, so that two interpreters looking at the same body of evidence may arrive at radically incommensurable results with neither of them being 'wrong'. As an estimable illustration of such work I will offer James Grantham Turner's *Libertines and Radicals in Early Modern London: Sexuality, Politics and Literary Culture, 1630–1685* (2002). All three are valuable disciplines and the same person may at different times engage in all of them.[8] My point is merely that 'theatre history' (if seriously practised) makes a different kind of 'truth claim' and must be judged on that basis.

(2) *The limits of evidence*. What can be usefully investigated by the theatre historian necessarily depends on the evidence that can be found – something that changes enormously in the period at issue. If we look, for example, at Postlewait's excellent essay on historiographic practice, we will find that much of what he says is essentially irrelevant to anything that can be done in the eighteenth century.[9] His test case is the first London production of Ibsen's *A Doll's House* (1889). The kinds of evidence available at that date and for that particular event are simply not to be had for *any* theatrical performance before the nineteenth century. We have to accept the limits of our knowledge. We will probably never know much about the costumes worn in Shakespeare's day. We can only guess at what most of the scenery in Betterton's theatres looked like. The reception of *The Conquest of Granada* is essentially unreconstructable except by conjecture and will very likely always remain that way. If we insist upon speculating or fabricating 'evidence' about playwrights out of their plays or indulging in 'must have' claims, then the result is to destroy any real value that theatre history (or history of any kind) can have.[10]

Evidentiary issues are particularly tricky in the 1660–1800 period because the nature of what is available changes so drastically between those dates. Prior to

1705 we often know no more than five or ten per cent of what was performed night by night in a given season in London. After that, our performance records are close to 100 per cent complete, at least for theatres operated under a patent or royal licence. Our knowledge of late seventeenth-century theatre buildings is radically conjectural; of late eighteenth-century theatres, relatively detailed and precise. We do possess printed texts of a large majority of the plays professionally performed in London throughout the period, though extant promptbooks give us painfully little help in reconstructing production concepts and performance practices.[11] Not until late in the eighteenth century, however, do newspapers start to do detailed and systematic reviewing and even when they do we cannot build with much confidence on what they tell us. Because of evidentiary differences, working in the 1690s is a startlingly different enterprise from working in the 1790s (or even the 1730s) – a fact well known to practitioners who have attempted both, but little commented upon in print.

Whether the pertinent evidence is scanty or overwhelming, it needs to be known and used. Enormous amounts of new documentary material have been discovered in the last half century, long after pioneers like Hotson and Nicoll seemed to have exhausted the possibilities – or so a lot of people thought when I was a graduate student. I have no doubt (based on a lifetime of archival scholarship) that major discoveries are yet to come. Exciting as this is, we should not fail to make use of what is already known. One of the recurring themes of this essay is the frequency with which scholars under-use or ignore (or perhaps simply do not know about) major bibliographical and archival sources. I have counted seventeen separate occasions on which I have pointed to instances of such failure. I am sorry to seem monotonous, but this really is important.

(3) *Enforced omission.* Except where evidence is largely lacking or where the subject is extremely limited, almost all 'histories' are radically selective because they have to be. As an example, let me instance a chapter I wrote on 'Drama and Theatre in the Mid and Later Eighteenth Century.'[12] I was asked to cover the period 1730–1790 in thirty typescript pages. Some nine hundred professionally staged, attributed new plays from these years survive. Of necessity, I had basically to state and then ignore the fact that about 85 per cent of the performances at Covent Garden and Drury Lane were of *old* plays. I had no space to deal with adaptations, though there were a lot of important ones. The beginning and ending dates had virtually no logic in either drama history or theatre history: they were simply given to me by the publisher. I could not discuss parallels in the novel or influences from the novel. Gothicism and the picturesque had essentially to be ignored, as did the social and political issues that became so important in the 1790s. Dance was a conspicuous, important and expensive part of theatrical offerings, but I had no space to discuss it. I managed to name some eight-five plays *en passant* (and about twenty older ones for comparison), but the selection was a mish-mash of titles then popular and others now critically esteemed. Value judgements about dramatic genres had to be imposed: I could do no more than mention the enormous importance of pantomime and musicals, which conveys a badly distorted sense of theatregoers' experience at the time. The numerous and

important non-English sources and influences had to be ignored.[13] Discussion of the impact of staging was simply a lost cause. Make no mistake about it: 'History' is almost always a radically selective representation whose construction is heavily influenced by publication constraints and the objectives and prejudices of the history writer. This is essentially unavoidable, but an honest historian can at least bluntly disclose choices, prejudices and suppressions. Much can be said in favour of 'microhistory', but of course it cannot supply the big-picture overviews that even scholars seem to crave.

So where does this leave us? As I conceive our enterprise, the theatre historian needs to commit to the principle that the ultimate objective is illumination of plays in their theatrical, social and political contexts. He or she also needs to submit to the constraints of 'good practice'. Analysis of playscripts is very much a legitimate (indeed a vital) part of the overall undertaking. 'Narrative' history is appropriate in treating individuals and institutions – but not in drama history. Where the objectives and claims go beyond the realms of particularizable evidence, then one is practising something other than theatre history. Failure to respect the limits of available evidence produces shoddy scholarship at best. The often radical selectivity inevitable in all but the smallest-scale 'history' needs to be acknowledged bluntly by the historian and understood by the reader. Such are my assumptions about 'good practice' in principle. With these stipulations in mind, we are ready to consider the particularities of theatre history in the long eighteenth century.

II. Texts

Between 1660 and 1800 more than 2,400 'new' plays were professionally staged in London. This total includes both mainpieces and afterpieces and it includes quite a lot of translations and adaptations.[14] Consequently no exact definition of 'new' can be given. A very high proportion of the plays were published within weeks or months of their performance (in striking contrast to pre-1642 drama). Most were made available half a century ago on Readex Microcards and almost all are now even more accessible in electronic form via EEBO and ECCO.[15] Many post-1737 plays for the patent theatres also exist in the 'Larpent manuscripts' submitted to the censor (now in the Huntington Library). In the realm of texts, copious evidence survives.[16]

Of these hundreds of plays, only about twenty-five have received more than cursory critical analysis. The sole attempt at an overall critical survey is the first three volumes of Allardyce Nicoll's *History*, which contains valuable lists of known plays but which does little more than offer simplistic categorizations while delivering some offhand value judgements.[17] Two attempts have been made at comprehensive surveys of late seventeenth-century plays,[18] but large portions of eighteenth-century drama remain essentially unstudied since Nicoll issued his crude map in the 1920s and early 1930s. Book after book has been devoted to the plays of Etherege, Wycherley and Congreve – though most of them exhibit almost complete ignorance of other plays of their time. Quite a lot has been

published on Dryden's 'heroic' plays (but singularly little about most of the rest of his output). Many studies have been made of *The Beggar's Opera*, though the only full-dress investigation of ballad opera came out in 1937 and ignores the music.[19] One will find a smattering of attention to the work of Farquhar, Fielding and Sheridan. Aphra Behn has enjoyed a boom in recent years. Overall, both the quantity and the quality of the modern criticism of this drama must be considered disappointing. The reason for this is simple. For the most part, these plays are highly effective theatrical vehicles, but they tend to possess little literary depth. They do not provide the complexities and ambiguities dear to teachers of English literature, to whom they offer little challenge. In explicative terms, these plays are usually unproblematical.

We have a huge number of playscripts for this period. What ought we to be doing with them? A good beginning is to try to understand the nature of the plays. Only towards the end of the seventeenth century did 'originality' become much of a desideratum and by the end of the eighteenth the theatres were more interested in the broad appeal that would fill increasingly huge theatres than in literary quality.[20] Writing for television or films today is a fair comparison. No play was advertised in London with its author's name attached until 1699 and even in the middle of the eighteenth century playbills for both old and new plays often omitted any mention of the author – even Shakespeare.[21] We need, however, to realize both that plays were only marginally regarded as 'literature' and that they were constructed for performance and must be understood in light of that context. By way of rough parallel I would observe that one can legitimately study song lyrics by themselves, but one misses a lot if one does not have (or chooses to ignore) the music.

In an essay in the first volume of this series, W. B. Worthen asks 'Is it possible to understand performance through the scripted form of dramatic texts?'[22] (My own answer is 'Only with great difficulty and in severely attenuated form.') Worthen goes on to point out that 'we are only beginning to understand the consequences of print as a delivery system for works of art in general and for dramatic writing in particular'. This is true: print delivers a *reading* experience and theatre offers a very different one. At various points throughout this essay I shall be endeavouring to address the problem of how one can attempt to translate text into performance analysis. Right now, let me say simply that the interpreter has to possess at least a good working knowledge of the physical theatre circumstances, the production norms of the time, the original performers (if known) and the generic conventions in force when the play was written.

Remarkably few attempts to analyse these plays in *theatrical* terms have ever been published. One good reason for this is that almost all editions have been produced by textual bibliographers who generally seem to be both ignorant of and uninterested in theatrical matters. One might, at a bare minimum, expect both student texts and standard editions (like those published by Oxford University Press) to provide (a) an explanation of changeable scenery theatres and demonstration of the scenic requirements of the play and (b) some analysis of the original cast for the light it sheds on the production concept at the time of première. I defy

the reader to point to examples of such assistance to interpreters in any edition of 'Restoration and eighteenth-century plays' currently in print.[23] A theatre person devoting him or herself to analysis of scripts would not require such elementary assistance, but few theatre historians engage in such work and critics coming from literary backgrounds very definitely need all the help they can get. The continuing uselessness of modern scholarly editions for theatrical interpretation is difficult to understand. Even harder to see is why reviewers of editions of plays do not take the editors to task for failing to address the issue of theatrical context.[24] Much remains to be done with a large number of playscripts.

I should point out that almost all surviving texts were written for and performed in theatres operating under patents or royal licence. Plays performed there routinely got printed; other kinds of theatrical performances were less scripted, less elite and far more ephemeral. By no means, however, were they an insignificant part of the theatrical experience of the public in the 1660–1800 era. London had a fair amount of fringe theatre. Some of the things mounted, say, at the Little Haymarket in the 1720s and 1730s got printed (for example Fielding), but many did not. The drolls performed at Bartholomew and Southwark Fairs were hugely popular and made a fortune for an actor like Pinkethman, but we have only the sketchiest knowledge of what constituted most of them.[25] Like the chapbooks that sold in the tens and even hundreds of thousands (when plays usually sold only a few hundred copies), the theatre of the fairs was popular, not elite culture. *The London Stage* makes almost no effort to record puppet theatre (quite important in London *circa* 1710) or to report comic miscellanies and quasi-improvisations of the sort Tony Aston toured with for decades.[26] Lord Mayors' shows are very minimally recorded, though they have considerable socio-political significance.[27] We must also remember that most of the surviving evidence concerns theatre in *London*. We know of the existence of strolling companies and amateur groups in country houses and we sometimes know the titles of plays they performed, but theatrical conditions were certainly not those of Drury Lane and Covent Garden and one suspects that *Hamlet* as performed by Doggett's strollers had been 'mucked about with' a bit (and perhaps a lot).[28] The experience of theatre beyond the patent theatres in London remains profoundly murky and given lack of evidence seems likely to remain so. This is not, however, a reason for ignoring the subject or for failing to pursue such archival material as might be found.

III. Performance records

The publication of *The London Stage 1660–1800* in the 1960s provided a quite fantastic tool for the theatre historian.[29] Genest's ten-volume calendar (published as early as 1832) contained a good deal of the same information, but it reported the primary sources less fully and in a less helpful format. In *The London Stage* calendar one can see at a glance what each theatre was performing on any day. Casts and additional entertainments are reported; the standard of accuracy is very high indeed. What is there not to like? Several things, actually, though most of them are of relatively minor import. What is deplorable is the poor use made by scholars of one of the great reference works of our time. Of that, more shortly.