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The Eunuch’s Tale: Reviewing Reviewed

Since its inception in the 17th century, the peer review process has generated much discussion and dissention. This largely introspective, occasionally tongue-in-cheek, paper examines the nature of the reviewing process in academic marketing research. It highlights the sorts of problems commonly associated with peer review, evaluates the many and varied reforms that have been proposed and, by introducing the principles of genre theory, discusses the academic paper as a highly codified literary form.

Introduction

Reviewers, according to the celebrated Irish playwright and dipsomaniac, Brendan Behan, “are like eunuchs in a harem; they know how it’s done, they’ve seen it done everyday, but they are unable to do it themselves”. Indeed, if the anthologies of literary and artistic quotations are any indication, Behan’s opinion of reviewers and critics is very widely shared (e.g. Muir and Brett 1981; Metcalf 1987). It has been said, for example, that “any fool can criticise and many of them do” (Garbett); reviewers have been described as “individuals uninhibited by ignorance” (Macmillan), “people who know the way but can’t drive the car” (Tynan), and the equivalent of “a legless man who teaches running” (Pollock). For Mel Brooks, moreover, “critics can’t even make music by rubbing their back legs together” and, in John Osborne’s deathless put-down, “asking a working writer what he feels about reviewers is like asking a lamp-post what it feels about dogs”.

Although Behan’s aphorism on the role of the reviewer, and the manifold variations on the theme, encapsulate the long-standing intellectual assumption that “those who can, do; those who can’t, review”, this time-worn and somewhat hackneyed contention does not stand up to close scrutiny. Apart from the latter-day advent of “post-criticism”, which elevates the comments of the reviewer over the creativity of the author (Brown 1995a), the simple fact of the matter is that many, if not most, literary luminaries also serve in a critical capacity as occasional reviewers, and equally many critics seem keen to try their literary hand. As a glance through the Sunday supplements amply testifies, the Martin Amis’s, Will Self’s, Gore Vidal’s and John Updike’s of this world appear quite prepared to prostitute themselves on the (lucrative) alter of the review pages and critics ranging from Gilbert Adair and Sarah Dunant to David Lodge and John Carey are more than capable of tilting successfully at the best-sellers list. There’s more to eunuchs, so it seems, than meets the eye.

When the academic world is examined, much the same situation prevails, albeit on a less elevated plane. We all labour in the vineyards of marketing research, hoping to produce a grand cru but invariably ending up with vin ordinaire. Yet we

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also function as the academic equivalent of appellation contrôlée, standing in
judgement on the output of our marketing peers. Some cynics, admittedly, might
maintain that, when it comes to marketing scholarship, a form of intellectual
intervention policy obtains. No matter how awful our papers are, someone
somewhere will probably publish them and, while there is undoubtedly a case for
cerebral set-aside, most of us continue to add to the vast wine lake of academic
marketing endeavour — undistinguished, undrinkable and only suitable for
recycling as anti-freeze, balsamic vinegar or boot polish.

Notwithstanding the serious structural problems of the CAP (Common Academic
Policy), the fact remains that we, as individuals, spend a great deal of time reviewing
the efforts of our fellow academics, and they, in turn, expend considerable energy on
our behalf. In theory, these reviews should be neutral, disinterested and predicated
entirely upon the merits of the contribution in question. In practice, of course, this
does not seem to be the case. We all have a fund of after-dinner stories concerning
our misguided, malicious and/or misogynistic reviewers (and which, given half a
chance, we will recount before dinner, during dinner, over coffee and liqueurs, or
until such times as the final guests have made their excuses and left). In fact, I have
yet to meet an academic who is not prepared to launch into a defamatory diatribe
concerning their alleged antagonists, the cabal of jealous mediocrities conspiring to
do them down.

Like everyone else, I have suffered from the slings and arrows of outrageous
reviewers (I know who you are and I’m going to get you — just wait!). However,
as the member of several editorial boards, a reviewer for publications as diverse as
The Economic Journal and Professional Geographer, and guest editor of a number of
journal special issues, I seem to spend an ever-increasing amount of time in
reviewing “mode”. Indeed, in my capacity as Book Reviews Editor of Irish Marketing
Review, I have been foolish enough to champion a “revolution” in critical standards,
though the fruits of this insurrection are slow to materialize (Brown 1993, 1995b).

Be that as it may, I have been an active participant in the beauty parade of
marketing scholarship, both as contestant and judge, and welcome the opportunity
to contribute my largely introspective twopennyworth on the commodification of
marketing knowledge. The present commentary commences with some examples,
drawn from my own personal experience, of the review process at work; continues
with a discussion of the academic paper as a literary genre; culminates in an
explication of the nature and characteristics of the double-blind review; and
concludes with some reflections on the paper writing process.

Introspective Intromission

Kotler is Dead!

For some years now I have taught a postgraduate course on marketing theory and
thought. Despite my best efforts to make the module sexy and exciting, even to the
extent of incorporating non-theoretical concerns, I am forced to admit that the
content can be pretty dry and forbidding. Students find it abstract, ambiguous and
arcane, a conclusion which is predicated, at least in part, upon the stultifying aridity
of the currently available textbooks. Outstanding works of scholarship though they
undoubtedly are, Shelby Hunt’s (1991) Modern Marketing Theory and John O’Shaugh-
nessy's (1992) Explaining Buyer Behaviour can hardly be described as easy reading. Sheth et al.'s (1988) history of marketing thought is much more accessible, though it reeks of self-promotion and being economical with the vérité (strange how there turn out to be exactly 12 schools of marketing thought, three for each of the four categories of the authors' explanatory matrix, and that the highest "scoring" school of all turns out to be Sheth's own specialism of consumer research). A cynic might conclude that never in the history of marketing scholarship have so many been misled by the misrepresentations of so few, though I would never dream of suggesting such a thing.

Cognisant of the need for a reasonably priced primer on marketing theory, I had the brainwave of writing it in the form of a murder mystery. Entitled Kotler is Dead! or Who Killed Kotler?, and set in the marketing department of a provincial university, it provides all sorts of textual scope for introducing theoretical issues (lectures, seminars, conference papers, student essays, coffee-time discussions etc.) while maintaining reader interest through the twists, turns and ultimate resolution of the plot. Although such approaches have proved successful in other fields of study — Gaarder's (1995) Sophie's World, Pirsig's (1974) Zen and the Art of Motorcycle Maintenance and Mulkay's (1985) The Word and the World, spring immediately to mind — and although the idea is very much in keeping with Sherry's (1991) plea for "new literary forms", within marketing, the publishers I approached just didn't want to know. Marketing texts, it was made perfectly clear to me, must be clones of Kotler, interlarded with an array of bullet-points, learning objectives, pseudo-case studies and, naturally, written in words of one syllable or less.

You Cannot be Serious!

I was still smarting from the unceremonious rejection of Who Killed Kotler? when I was collared by a postgraduate student at a marketing conference. She was deep in the coils of her PhD thesis and proceeded to give me a blow-by-blow account of her research project. As someone who suffered from a severe case of supervisory neglect when I was wrestling with my own thesis — and who was only rescued by the generous intercession of a distinguished academic from another institution — I have always gone out of my way to be helpful to anyone who asks for advice. However, as the nature of this particular student's research unfolded, it dawned on me that she was in the process of writing up a completely theoretical PhD. She had undertaken no empirical research, none whatsoever. Now, there's nothing to stop someone from submitting a theoretical thesis, but, in the British system at least, some sort of empirical work is "expected". For a purely theoretical PhD to pass muster, it would have to be extraordinarily brilliant and, while this student may well have been endowed with the requisite intellectual gifts, it seemed to me that she was taking an enormous risk. My advice to her was to play it safe by incorporating some appropriate empirical analyses. She could write theoretical papers to her heart's content once she got the qualification, but when 4–5 years of her life were on the line, it was unwise — to put it mildly — to take the sorts of risks that a theoretical PhD invariably involved.

It is a couple of years now since I dispensed my "words of wisdom" to that student and she has made no subsequent effort to get in touch. I have no idea whether she took my advice and it proved helpful (or otherwise), or whether she
decided to go her own way and proved me wrong (or right). Yet, in light of my own experience with *Kotler is Dead!* I can’t help thinking about the hypocrisy of the guidance I so “generously” disbursed. On the one hand, I’m happy to admonish publishers for failing to take a chance on my own ambition to break the marketing mould. On the other hand, I actively dissuade — in the nicest possible way, of course — an ambitious postgraduate from seeking to do likewise. More to the point, what should I say to my own postgraduate students when they come to me with the “brilliant” idea of writing their thesis up as a poem, a three-act play or, indeed, setting it to music? Do I tell them to get a grip, to be sensible, to avoid risks, even though I quite like the idea? Do I urge them to go for it, to take a chance, to rethink the very nature of marketing thinking, even though their external examiner might not be so amenable? Or, do I opt for the coward’s way out by explaining the advantages and disadvantages of both courses of action and suggesting that they make up their own minds?

*Slicing the Salami*

Another academic conundrum I faced recently concerned a conference paper. I was asked to review a manuscript for a forthcoming international conference and, although the paper omitted to mention the single most important contribution to the field (thereby, raising all sorts of doubts in my mind about the merits of the manuscript as a whole), I felt that the topic was highly relevant to the overall conference theme and, as such, was likely to stimulate a great deal of useful discussion. So, I recommended acceptance subject to the incorporation of the missing literature. Now, I have no idea whether the paper was accepted by the conference organizers, nor do I have the faintest idea who the authors were, but, lo and behold, some weeks before the conference was due to take place, I was asked to review the *exact same manuscript* by a Category One academic journal. Maybe I’m being irrational and unnecessarily dogmatic, but I was extremely annoyed at this behaviour. Granted, the paper may have been rejected by the conference and the authors were perfectly within their rights to submit it to a journal (though the question has to be asked, if it was not good enough for the conference, what hope could it possibly have in a Category One journal?). Conversely, the paper could have been accepted and, for all I know, currently occupies pride of place in the proceedings. Indeed, had I received the manuscript after the conference had taken place, I probably would have thought no more about it. But, for some reason, its arrival in advance of (probable) publication elsewhere upset me enormously and I recommended outright rejection.

In light of the pressure to publish that we all face, especially in these pre-RAE dog days, my decision to reject may seem exceptionally harsh. After all, the very nature of the refereeing process should ensure that the manuscript will be reshaped to some degree and, hence, the final published version is likely to be markedly different from that which appeared in the conference proceedings. Some readers, moreover, may be astonished at the author’s manifest double standards. It’s a bit rich when someone who has never been reluctant to set the scholarly salami slicer on “extra thin” starts taking umbrage at the unethical behaviour of his peers. Talk about the pot calling the kettle black! However, as I have pointed out elsewhere (and a whiff of the salami slicer notwithstanding):
"It's a well-known fact of academic life that the rules apply to everyone but ourselves. We demand objectivity, neutrality and disinterestedness from our peers — and express outrage when these norms of academic etiquette are broken — yet as individuals we are willing to trample others underfoot in our ceaseless pursuit of publications and self-advancement, we take every available opportunity to settle old scores, invariably under cover of the so-called double blind review process, and we shamelessly seek and expect personal favours in return for the most minor acts of collegial courtesy." (Brown 1995b, p.157).

Brown's a Bastard!

I suppose if I were interested enough, I could track down those particular conference proceedings in order to find out who the authors are. Indeed, as the other reviewers of the Category One journal may have been impressed by the manuscript, I could well be seeing it in print whether I want to or not. Regardless of my (dis)interest in the identity of these individuals, the possibility also arises that they might have identified me as their tormentor. I don't know about you, but whenever I receive referees' comments I invariably ruminate on the identities of my protagonists and, since my field is so small, consider myself fairly adept at working out who the vindictive, know-nothings are. I'm pretty sure that the same applies to my own refereeing endeavours; in fact, I'm more than pretty sure, I'm certain. For my sins, I happen to be cursed with a fairly distinctive writing style — or "voice" as we pretentious literary types like to call it — and the editorial grapevine has occasionally informed me that, despite the protection of the double-blind review process, my identity has been successfully decoded. In other words, that the disgruntled authors have communicated with the editor about their alleged maltreatment, as in the suitably restrained, scholarly and brutally honest remark, "I know who the referee was, it was that bastard Brown, wasn't it?" Only the most hard-hearted reviewers would fail to be concerned by such unmasking and, although it embarrasses me to confess the fact, I have often gone to elaborate lengths in an attempt to disguise my identity — writing in the most illiterate manner I can devise, commencing the review with the words "this paper", a turn of phrase that I am on record as deploiring above all others, asking my wife to write out the comments in the hope that her handwriting will throw them off the scent and many other manoeuvres besides.

The foregoing remarks may seem like the ravings of a candidate for the Care in the Community school of marketing scholarship, but let me give you a pertinent and highly current example. Sitting on my desk as I write this paper is a manuscript which I have been asked to review for a well-known marketing journal. The paper reports the results of a major research project, an industry-funded initiative that has attracted a great deal of publicity. Hence, I am in absolutely no doubt as to the identity of the authors, though their fondness for self-citation makes it fairly obvious in any event. The problem, however, is that the paper is bloody awful. It deals with an issue which is generating a great deal of discussion in other academic disciplines,
a concept that has been completely rethought in recent years. But, do the authors exhibit any inkling of this material? No, they do not. They appear to have no knowledge whatsoever of its existence; they continue to draw upon outdated marketing contributions to this tradition; they seem to think that brute empiricism will always provide the answer. I have drafted my comments, excoriating the authors for their lack of outside reading and that should be the end of the matter until such times as a substantially revised manuscript is resubmitted. But, and this is a very big but, we are dealing here with some very distinguished academics, some extremely powerful people who need this publication for the forthcoming RAE, and who are perfectly capable of identifying the cut of my jibes. So, do I stick to my guns and assume that, as distinguished scholars interested only in the advancement of marketing knowledge, the authors will accept my remarks without demur? Do I attempt to camouflage my comments, which will merely prompt them to conclude "Oh, that's Stephen Brown, the bastard, endeavouring to disguise his malicious handiwork"? Do I soften my review, even though I know the paper is a disgrace and that the publication of papers like this is part of the reason marketing continues to be held in such low intellectual esteem? Do I let it through with a few supportive comments, knowing that the authors will recognize my "voice" and be suitably grateful? Or, do I send it back to the editor with a note to the effect that I don't consider myself competent to review the manuscript and thereby disrupt his journal's finely-tuned, just-in-time administrative system?

**Genre Theory**

Some cynics among the *JMM* readership may dismiss the foregoing anecdotes as instantiations of the author's guilty conscience, as feeble, albeit no doubt cathartic, attempts to "write" the wrongs that he has perpetrated upon unwitting and entirely innocent marketing scholars. Others might consider them a scandalous betrayal of academic etiquette or, worse, an unconvincing smokescreen, a disingenuous attempt to exploit this prestigious academic platform for nefarious, self-aggrandizing ends (*Kotler is Dead* indeed!). While there is probably some truth in such conclusions — I often suffer pangs of doubt about my refereeing decisions, as I suspect many if not most reviewers do, and I know that when it comes to being put upon by reviewers no one, *but no one*, suffers more than yours truly — it is arguable that the above introspections also illustrate some important aspects of the publications process, most notably the academic paper as a literary genre.

Genre, according to Palmer (1992, p.112), "is the French term commonly used to indicate that texts can be sorted into groups which have common characteristics". As exemplified by the standard shelf-markings in most bookshops — crime, romance, war, fantasy, travel etc. — we are all conscious of and familiar with literary genres even though the majority of us give them very little thought (Figures 1 and 2). After all, the bulk of our mass-mediated experiences come wrapped in some form of generic packaging — television game shows, soap operas and situation comedies; cowboy, spy and horror films; country and western, heavy metal and acid house music; and, look-alike advertisements for beer, shampoo and washing powder. Although the study of genre is often dated to Aristotle's *Poetics*, with its tripartite distinction between tragedy, comedy and epic literary forms, it is only within the past 40 years, and the elision of the traditional division between elite and popular
Figure 1. Write your own country & western hit. Source: Berger (1992, p.64). Original by Larry Tritten.
Figure 2. Make your own James Bond movie. Source: Berger (1992, pp.122–123) Original by Larry Tritten.
culture, that genre has emerged as a separate field of academic study (Berger 1992). Like most academic specialisms, genre studies are beset by all manner of competing conceptual frameworks. Distinctions, for example, are often made between "theoretical" and "historical" genres (Todorov 1970), "enunciative situation" and "subject matter" (Schaeffer 1989) and the manifold loci of literary meaning — the unconscious, capitalism, reader response and the like (see for example Frye 1971; Cavelti 1976; Palmer 1992).

For our present purposes, however, it is sufficient to note that genres work in two closely related ways, described by Berger (1992) as being akin to figure and ground. First, they provide a "horizon of expectations", a framework or schema within which individual works are placed and interpreted. Thus, if we recognize a text as belonging to the category "situation comedy", "film noir", "sword and sorcery novel" or whatever, we come to it with certain pre-formed expectations derived from our previous experiences with the category in question. Second, genre functions as a norm, a set of unwritten and ill-defined rules which apply to individual works and which, if flouted, result in confusion and rejection. Radway's (1987) renowned study of romantic fiction readers, for example, found that the novels deemed disappointing or unrewarding were invariably those that failed to adhere to the norms of the form. Similarly, a blues song which follows "well I woke up this morning" with "in my 36 bedroom mansion" or "and I'd won the National Lottery" is likely to be less appealing to aficionados of the genre than, "with a hellhound on my trail" or "and my baby done left me".

Set against the need to provide readers with a sense of the familiar, genre theory emphasizes that it is necessary to introduce sufficient variety and innovation to maintain the audience's interest. In this respect, Cavelti (1971) distinguishes between the need for "convention" and "invention". The former ensures that an individual text falls into a familiar and recognizable category, while the latter provides the surprise, whether it be in terms of form or content (or both), which elevates a particularly successful text above its copious competitors, the also-rans of the genre. These inventions, in turn, serve to shape the readership's extant horizon of expectations — the rules of the game — which is explored and tested by subsequent textual contributions, and so the genre evolves. Although, as Palmer (1992) points out, works of high culture are usually associated with the "invention" end of the literary spectrum, whereas works of low culture are often dismissed as "convention" bound, the fact of the matter is that a dialectic of order and novelty, similarity and difference, identity and opposition, has long been discernible in all manner of artistic fields, both elite and popular (Lotman 1975).

In light of the foregoing, it takes a very small leap of the imagination to appreciate that the academic paper is also a very distinctive literary genre and can be studied accordingly. Granted, such a suggestion may be anathema to those who regard the published record of the physical sciences (and epigone social sciences) as an unadorned, unambiguous reflection of scientific "truths", untainted by the emotions, idiosyncrasies and rhetorical flourishes that tend to prevail in the field of literary criticism. While these sentiments remain surprisingly widespread — more than 35 years after C.P. Snow's (1959) landmark lecture it would appear that his two cultures of literature and science are very much alive and well — they simply do not withstand close scrutiny. According to Locke's (1992) recent analysis of scientific prose, what he appropriately describes as "the art of artless writing", physical
scientists make full and frequent use of ostensibly "literary" devices from irony and cacophony to metaphor and parody. Indeed, a number of longitudinal literary analyses of scientific journals have been undertaken, including several informed by the principles of genre theory (Bazerman 1988; Halliday and Martin 1993; Selzer 1993). As Locke (1992, p.168) makes perfectly clear:

"The papers of the scientist are the most formulaic of writings: their rigidly prescribed patterns put the generic conventions of the western or the detective story to shame. A glance through a scientific journal reveals paper after paper with identical patterns of organisation, often precisely the same headings."

If the published output of the physical sciences has proved amenable to genre-based interpretations, and the published record of "hard" social sciences, such as economics, has proved equally susceptible (e.g. McCloskey 1985, 1990, 1994), it seems reasonable to conclude that the endeavours of marketing academics are ripe for analyses predicated on genre theory. One only has to peruse a few back issues of any of our discipline's principal journals to appreciate that the papers adhere to a clearly discernible set of conventions concerning content, form, sequencing, language and protocol. Most of us, for example, can recite the "typical" structure of an academic paper in our sleep (indeed, many of us have been put to sleep by authors' rigid adherence to the "rules"). We are all well aware that writing in a neutral, disinterested, pseudo-scientific fashion is de rigueur, that it is necessary to bolster our claims with an appreciation of the existing body of literature and the positioning of our contributions therein; and, not least, that it is imperative to conclude with some suitably vague allusion to "managerial implications", even though no manager in their right mind is ever likely to read our pearls of wisdom or, heaven forbid, proceed to act upon our recommendations.

However, as the above Kotler is Dead! and You Cannot be Serious! episodes clearly illustrate, failure to comply with the rules of the game invites immediate and unceremonious rejection. This rejection, admittedly, is usually conveyed in reviewer-speak — the secret code that everyone understands — where "interesting" means "unoriginal", "provocative" means "unpublishable", "controversial" means "lock them up" and, as I can personally testify, "beautifully written" translates into "completely incomprehensible". Be that as it may, the fact remains that flying in the face of the norms of the genre is a sure-fire guarantee of academic tears before bedtime.

Having conceded that genre theory is applicable to our understanding of marketing understanding, it is tempting to examine its implications in minute detail. To itemize, in other words, the variations on the formulaic theme in diverse sub-genres like textbooks, working papers, literature reviews, monographs, consultancy reports, PhD theses, practitioner-oriented periodicals and, perhaps the most convention-bound artifact of all, the referee's report (see Appendix 1). Likewise, the rigidity of the rules is likely to vary somewhat in different sub-fields of marketing scholarship. It seems to me, for example, that consumer research is much more amenable to unorthodox approaches than my own specialization of retailing, and whereas advertising and promotion is forging ahead intellectually, international marketing remains immured in the disciplinary dark ages. Rather than immerse
ourselves in the minutiae of genre theory as it pertains to marketing, it is perhaps more appropriate for our present purposes simply to note two particularly salient points. The first of these is that the conventions of the academic paper genre perform a very important function. It is easy to be dismissive of "look-alike" papers, but a frame of reference is vitally necessary. The existence of guidelines, however tacit and ill-defined, makes the writing task much less demanding than it already is, especially for inexperienced researchers. Few of us, after all, spring fully armed from the scholarly soil and it is only reasonable to expect that our intellectual apprenticeships will result in hackneyed and derivative manuscripts. Whether such juvenilia should be published is another matter entirely, as is seasoned academics' continuing reliance on the familiar and formulaic, but there is no doubt that a sense of "what is expected" plays an important disciplinary function.

The second significant consideration concerns the mutability of literary genres. The rules of the game can be and are broken. Invention is as important as convention. As a trawl through the back issues of JM, JMR, JCR, JAMS or whatever clearly testifies — and several detailed longitudinal studies of scientific journals serve to corroborate (e.g. Bazerman 1988) — the current contents of Category One marketing publications are markedly different from those of 30 years ago. Almost without exception, the papers are much longer, more rigorous methodologically, more sophisticated philosophically and more citation strewn than before. Indeed, it is no exaggeration to state that many marketing "classics", as they are referred to in the anthologies, would simply be unpublishable today. Although the quality of latter-day academic rhetoric may be no better — possibly worse — than that of a generation ago, the standards of marketing scholarship have increased so much in the interim that the Journal of Marketing no longer appears to have room for papers, like Kotler and Levy's (1969) prizewinner, of six pages and four references.

In these circumstances, the question has to be asked; how does change take place? If genres are rule-bound and would-be heretics or non-conformists are routinely excommunicated (literally, in that they are effectively denied the opportunity to communicate their views), how can we account for the academic transubstantiation that can and does transpire? Clearly, some of these changes are attributable to developments in the broader socio-economic and intellectual environments (Figure 3). Consider, for example, how the prevailing attitude towards "science" and its progressive-cum-utopian appurtenances, has shifted dramatically in the post-war period (Midgley 1992). Others are undoubtedly the result of sheer boredom, a feeling that the existing formula is becoming stale and unrewarding (Skinner 1985). Nevertheless, it is important to appreciate that however widespread this sense of ennui and stasis, no matter how degenerate — in Lakatosian terms — a research programme becomes, regardless of currents and turbulence in the prevailing intellectual climate, significant change cannot be divorced from the politics of publishing, the peer review system in particular (see Appendix 1).

**Peer Pressure**

Few issues in academic life generate as much discussion, debate and, let's be honest, bad blood as the peer review process. It constantly impinges on our day-to-day activities and looms large over our career aspirations, whether it be in the form of job references, grant applications, promotion prospects or, most frequently of all, in the
judgement that is passed on our research output. We are all familiar with the palpitations induced by a glimpse of the distinctive “decision” envelope; the temporary loss of motor functions we experience on attempting to unseath the letter that seals our fate; the bitter tears of disappointment as our superhuman efforts are swept aside in a few acerbic and ill-informed sentences; and, on occasion, the overwhelming sense of relief, swiftly followed by punch-the-air euphoria, when we have been anointed by the wise and perspicacious reviewers of our chosen communication channel, the scholarly standing of which has suddenly been elevated to a level beyond the journal editor’s wildest dreams.

When the emotions of the moment have subsided, however, most of us have paused to reflect on the arbitrary charade that is the peer review process. After all, we know that reviewers are capricious, inconsistent and motivated by personal animosity, professional jealousy and a narrow-minded conviction that their preferred research approach is incontestable. What’s more, we have a pretty good idea who they are and, by Christ, we’ll make them pay for it! We also know that journal editors are a repellent bunch of self-serving, self-satisfied, self-abusers, who despite all our attempts at insinuation, are hopelessly biased towards submissions from their own institution and the old-boy, mutual back-scratching, motley crew of fellow editors. They simply don’t have room for papers from uninfuential nonentities, the likes of you and me, and they conspire to send our manuscripts to the reviewers most inclined to kill them off or — mixed metaphor ahy — kick them safely into touch whilst plagiarizing our insights and beating us to the punch. Most importantly perhaps, our convictions concerning the peer review farrago are confirmed by the fact that our very best work is inexplicably but invariably rejected, whereas our second rate knock-offs, the iffy papers we consider unworthy of publication but thought we’d try our luck because you never can tell, proceed to sail through the refereeing procedure with nary an adverse remark!
Given the prominent place peer review occupies in the academic imagination, it is not surprising that the issue has attracted a great deal of research attention. Since its inception in the 17th century, this regulatory mechanism has been subject to periodic scrutiny, though thanks largely to the pioneering analyses of R.K. Merton a flourishing sub-discipline devoted to the study of peer review now exists (Chubin and Hackett 1990; Reanie 1990; Daniel 1993). Conducted in scientific fields as diverse as economics, criminology, astrophysics and biochemistry, the results of these investigations are remarkably consistent. They reveal that referees are unreliable, in so far as the degree of agreement between different reviewers of the same manuscript is very low indeed (Marsh and Ball 1989). By highlighting, moreover, that manuscripts rejected by the leading journals often turn out to be the ones with the greatest long-term influence, such exercises demonstrate that reviewers’ judgements are invalid and unsound (Bornstein 1991). More disconcertingly still, they have served to expose the manifold biases that inhere in and taint this supposedly objective procedure. These include the author’s rank (professors do better than postdocs), gender (patricracy prevails), nationality (non-nationals and, especially, third-world authors need not apply), institutional affiliation (striking “in-house” concentrations have been observed), the nature of their findings (those that support the preconceived opinions of the reviewers are more favourably treated) and, above all, his or her professional standing (well-known academics are much more likely to be published than the mediocrities). The last of these biases is known as the Matthew Effect, after the famous passage in the Gospel According to St Matthew:

“For whosoever hath, to him shall be given, and he shall have more abundance; but whosoever hath not, from him shall be taken away, even that he hath.” (see Merton 1968, 1988).

Now, in light of such devastating findings it seems reasonable to conclude that the whole reviewing process is hopelessly compromised and should be dispensed with entirely. The system, as Daniel (quoting Kornhuber) points out, is “unreliable, invalid and harmful to the best type of research — that which is innovative” (Daniel 1993, p.3). Indeed, some commentators have called for its complete abandonment (Mahoney 1985) and several reputable academic periodicals have eschewed blind review or opted for a battery of potential bias reducing procedures — formalizing the reviewing instrument, involving authors in reviewer selection, eliminating reviewer anonymity, increasing the number of reviewers, establishing a right of appeal and so on. However, before such drastic steps are contemplated, and introduced wholesale into marketing, it is necessary to appreciate three important points. First, that the reliability “problem” is trivial since most editors endeavour to send a manuscript to two different “types” of reviewer (one specialist and one generalist, for example) and therefore a lack of consistency in their comments and conclusions is only to be expected (Fiske and Fogg 1990). Second, the extent of the validity “problem” — the fact that highly significant contributions are liable to rejection — has been challenged by recent research based on citation rates and journal impact factors, which shows that, generally speaking, the most cited papers are published in journals with the greatest impact (Daniel 1993). Third, the manifold biases are at least partially attributable to the fact that in most academic disciplines
the single-blind review system tends to predominate (i.e. the referee is aware of the author’s identity, but the identity of the reviewer is withheld from the author).

With this in mind, it is tempting to go to the opposite extreme and assume that, as a consequence of its almost total dependence on the double-blind review system (where both referee and author are unknown to each other), the widespread use of three reviewers instead of the usual two and the virtual ubiquity of formal guidelines in the shape of Reviewer Report Forms (as opposed to “unstructured” commentary which tends to prevail elsewhere), marketing scholarship is uncontaminated by the biases that taint other academic specialisms. Sadly, the evidence concerning these procedures is not particularly comforting. Studies have shown that the double-blind review system is not simply a transparent veil — almost 80% of reviewers, for example, claim to be able to recognize the authors’ identity — but the pseudo-anonymity also prompts reviewers to be more critical than they would otherwise be (Ceci and Peters 1984). Indeed, as the system gives rise to lower acceptance rates generally, unorthodox or innovative submissions tend to suffer to a disproportionate extent under the double-blind approach (Blank 1991). Similarly, research on the use of three reviewers reveals that this ostensibly fairer approach (more referees equals less bias) adds substantially to the administrative burden of the editor and, since almost all manuscripts receive at least one negative review, editors are inclined to accede to the majority verdict rather than devoting due care and attention to the intellectual merits of the opposing arguments (Giles et al. 1989). The upshot, once again, is that original research tends to suffer. Even the use of formal guidelines for manuscript evaluation, which go some way towards increasing inter-reviewer reliability, is by no means beyond reproach. Apart from the fact that these guidelines often appear to be lifted verbatim, or with comparatively minor adjustments, from other periodicals and hence can hardly be said to reflect the editor’s unique vision of the journal in question, the details of the guidelines themselves can be called into question. Thus, it is not uncommon with academic marketing journals to find a statement in reviewer report forms concerning the managerial relevance of the manuscript. Many marketing scholars, however, do not subscribe to this practitioner-oriented ethos, arguing that association with a particular interest group in society subverts our desire to be accepted as a “legitimate” academic discipline (e.g. Holbrook 1984, 1985; Belk 1986; Hirschman 1987). Regardless of whether you agree or disagree with this somewhat elitist position, it is clear that the explicit managerial bias in reviewer guidelines discriminates against and serves effectively to silence the voices of these dissenting individuals.

Although the irredeemably tainted peer review system has been responsible for much weeping, wailing and gnashing of teeth within the academic marketing community (e.g. Holbrook 1986, 1994; AMA 1988; Churchill 1988), it may be worthwhile attempting to draw some tentative lessons from the extant body of research. The first of these is that, imperfect though it is, there are no real alternatives to the present system of peer review, or rather no alternatives without serious imperfections of their own (Daniel 1993). While it is true that a major cross-disciplinary survey revealed that 75% of respondents were unhappy with the peer review process, leading the author to report that “sizeable majorities of those in seven broad disciplines think the peer review system for deciding that what gets published in scholarly journals is biased in favour of “established” researchers,
scholars from prestigious institutions and those who use "currently fashionable" approaches to their subjects" (Jacobson 1986, p.1), surveys have also shown that very few academics want to abandon the system altogether (Brackbill and Korten 1970). Indeed, when lambasting the peer review process, it is all too easy to forget that nearly every manuscript is improved as a result of reviewer comments.

Second, just as submissions can be and are enriched by exposure to constructive criticism, so too the refereeing procedure itself is capable of improvement. The leading commentator on the peer review process in marketing has recommended that the veil of secrecy be lifted (Holbrook 1994). In the belief that "few reviewers would write the kinds of reviews they do if their names appeared at the bottom" (p.570), he espouses a "signed review" system, which is more likely to foster a conciliatory atmosphere where reviewers and authors work together in peace and harmony. Unfortunately, experiments with this approach in other disciplines, such as medicine, have not proved particularly successful, since very few referees choose to sign their reviews, especially when recommending rejection or major revision (Knox 1981; McNutt et al. 1991). What is more, when signing is made mandatory, reviewers either refuse to handle the manuscript at all or simply throw the decision back into the lap of the hapless editor. Anonymity may well be unattainable (authors think they recognize their tormentors, though they can never be certain), but it:

"is better for all concerned: for the referee, who does not have to mix emotional factors into his judgements; for the editor who gets a more honest opinion to guide his decisions; for the reader, who gets more reliable and better expressed papers that have been subject to a higher standard of criticism; and, strangely enough, for the author who, when his mistakes are pointed out, can vent his chagrin harmlessly in the direction of an impersonal critic without falling into the mortal sin of acquiring a supposed enemy." (Ziman 1976, p.264).

A somewhat better, though still imperfect, way of attaining Holbrook's admirable if characteristically romantic vision, would be to switch over to the single-blind review system as practised in most other academic disciplines. Our fetish for double-blind review seems to be a hangover from marketing's early desire to prove itself worthy of scholarly status, as part of the price that had to be paid to permit us to pass through the illustrious portals of the academy and take our place in the basement (naturally) of the ivy-clad ivory tower. If, so the logic seemed to run, single-blind reviewing is good, double-blind reviewing is bound to be even better — more rigorous, more objective, more scientific and so on. This is simply not the case. As we have already seen, the double-blind review system results in more aggressive referees' reports, gives rise to lower acceptance rates overall and acts as an impediment to creative or unconventional perspectives. Indeed, when this point is appreciated, then Hunt's (1994) complaints about marketing's inability to generate original knowledge or the AMA Task Force's (1988) concerns about the lack of innovative papers in the premier periodicals, suddenly take on a whole new complexion.

Another way of encouraging novelty and invention is to ensure that editors rely on no more than two reviewers instead of the increasingly ubiquitous three. This
latterday tendency is again part of marketing’s adolescent “more, more, more” school of intellectual aspiration and scientific status-seeking, though it also helps ease editors’ onerous decision-taking duties. It is arguable, however, that in this particular case “less is more”; that the editorial burden should not be relieved; that they should not be permitted to pass the buck — and thereby feign neutrality — by reliance on majority decision. Journal editors should seek to lead, to go out of their way to encourage heresy, and if this means that they have to decide between divided referees and impose their vision upon their publication, then so be it. At least we’ll know where they (and we) stand. After all, it is editors who complain loud and longest about the “me-too” manuscripts they receive. Yet, part of the reason they are flooded with look-alike papers is the inherent conservatism of the system over which they preside and could take it upon themselves to reform. In this respect, appropriate adjustments to manuscript reviewer guidelines might be a useful place to start. Instead of lazily recycling existing guidelines, with all their pseudo-practitioner pretensions, explicit statements concerning the journal/editor’s determination to encourage and actively promote innovative manuscripts should become the norm. Granted, this transformation is unlikely to take place overnight — reviewers are inherently conservative, some say small-minded — and, in fairness, many editors are open to radical experimentation (as this special issue of JMM testifies), but it is time to divest ourselves of the notion that more is always better when it comes to the review process. It is a manifestation of marketing’s long-standing lack of scholarly self-esteem, the belief that we have to be more scientific than science in order to be taken seriously in academic circles. Ironically, it is our very lack of intellectual innovation, the brute empiricism which the present peer review system helps perpetuate, that continues to condemn us to a fairly lowly position in the disciplinary firmament.

A third issue arising from the above discussion concerns the all-important part played by the authors themselves. Just as paper reviewing procedures and associated editorial arrangements need to be re-examined, so too improved guidelines are necessary for the contributors to academic journals. These guidelines, however, do not concern the style, structure and argument of the submitted papers, important though such issues are, they pertain rather to the intractable problem of multiple submissions, of slicing the salami, of flooding the periodicals with manuscripts offering, to put it charitably, minor variations on a particular research theme. In theory at least, this sort of behaviour is totally unacceptable, it invites instant rejection and, more importantly perhaps, is likely to inflict serious damage on an researcher’s carefully nurtured academic reputation. In practice, however, the rules are not clear cut and, such is the pressure to publish, that these unwritten rules are being continually rewritten. How similar, for example, does a manuscript have to be before it is sufficiently similar to warrant a witch-hunt? — 25%?, 50%?, 75%?, 99%?; is there any “difference” between a 5,000 word and a 10,000 word version of the same paper?; does publishing much the same manuscript in the journals of another discipline (say, economics or psychology, as well as marketing) count as slicing the salami, or not?; what, if anything, do we do when unaltered versions of refereed conference papers eventually appear in the journals (or working papers, for that matter)? How, moreover, can we possibly legislate for the different perceptions that are brought to bear by the authors, editors and readers concerned? For example, I have written several papers on the wheel of retailing theory and, to my mind, they
all attempt to add something new, take a different slant or play with the conventions of the literature review genre. Such an approach is perfectly in keeping with the "conceptual theorist" style of research endeavour (see Hirschman 1985), yet, for all I know, certain readers may see the papers and conclude: "Bloody Hell, there's Brown banging on about the wheel of retailing again. How on earth does he get away with it?". Indeed, and this is the ultimate imponderable, as a consequence of the rise of postmodernist philosophical perspectives, which do not subscribe to conventional notions of "originality" or "plagiarism" (Brown 1995c), consider a situation where the exact same paper is published in two separate journals. Is it the "same" paper, or not? On the surface, of course it is, but the context is likely to be different, the format different, the timing different, the readership different and, above all, our response to its publication is certain to be different. The first time we read it we may think, "what an original piece of work", whereas the second time it's more likely to be, "what a nerve, that author's a bit of a charlatan". So, to repeat the question, is it the same paper or not?

The purpose of these remarks is not to defend the indefensible, nor to offer a devious post-hoc rationale for my own manifold pirouettes on the thin ice of academic protocol. I am the first to admit that I have been parsimonious with the portions of my salami, if you'll pardon the expression and, in the time-honoured "can't see green cheese" academic tradition, I have expressed hypocritical outrage at the paper-eking antics of my peers. The purpose rather is to raise the more fundamental point that we, as authors, reviewers and editors, need to re-examine the whole publications circus. The fate of our discipline depends on it.

Kotler Coda

Having read thus far in the hope of picking up a few tips on writing better manuscripts, or beating the peer review process, most readers of this paper are likely to be feeling decidedly short in the change department. Not only is the paper badly written, but it contains nothing of practical worth. Like so many academic publications, it is utterly devoid of utility, of anything that passes for a real-world, implementable insight. For a so-called expert reviewer, Brown offers us very little by way of advice, nothing to help us improve our own manuscripts and thereby alleviate the agony of the peer review process, which — bizarrely — he has just gone out of his way to defend: What's more, the devious sod probably knows, or thinks he knows, the secret of successful paper submission, but because of his juvenile "art of war" mentality, he's not prepared to tell us (see Appendix 1). Those disgruntled authors were right, Brown is a Bastard!

Look folks, if I knew the secret of successful publication, I'd tell you, believe me. I can't give advice on writing better academic papers because I'm still learning myself and I'm a pretty slow pupil, remedial in fact. It's not a question of superstition — I don't imagine that if I tell you how to get published I'll never get published again — though I do have a healthy respect for hubris, the idea that pride comes before a fall. I'm not so proud of my publications record, such as it is, that I consider myself qualified to pontificate about best paper-writing practice. And, anyway, there are any number of published guidelines on how to unleash your literary talents, how to become a creative-cum-original thinker and so on (albeit you might be better off reading books on "how to win friends and influence people").
Holbrook (1986), for example, advises authors to be clear, succinct, poetic and humble. Daft (1985) recommends that we tell a story, pay due attention to the overall structure of the argument and, above all, listen to the reviewers, who “are on your side [and] enjoy helping transform a good paper into an excellent one” (stop laughing at the back — he can’t help his name!). Indeed, none other than that monument to the pursuit of scholarship before profit, MCB Press, has issued a set of guidelines and position papers on publishing management research (Literati Club 1995).

Like everyone else, I enjoy reading such compositional commandments. By affording us the vicarious thrill of comparing our own untutored habits against those of the “expert”, they are the academic equivalent of quasi-questionnaires and pseudo-surveys in glossy magazines and offer almost as much insight into the realities of publishing. It is, admittedly, interesting to note how many of these guidelines emphasize the importance of aesthetics in having papers accepted; in other words, the style of the manuscript rather than its content (textual analyses of reviewer comments reveal that accepted scientific papers are often referred to in aesthetic terms — “attractive”, “charming”, “elegant”, “appealing” etc.). Nevertheless, it seems to me that most of these best-practice blueprints are hopelessly naive and utopian. They describe a fantasy world of peace, love and understanding, where the lion lies down with the lamb, where authors and reviewers happily work together, side by side, for the greater good of the Academy and the disinterested pursuit of knowledge.

Get a grip!

The brute reality of the academic publishing game, as it is presently constituted, boils down to this simple question: what is it that you, as an individual, want to achieve? Do you want to get published, full-stop, or do you want to have an impact on your chosen field? If you simply want to get published, write to the formula. Learn the rules of the academic paper genre, stick to them religiously and forget about originality. The up-side of this strategy is that your CV will expand exponentially, your work will appear in the journals, thereby conferring the stolid, “highly respected” academic reputation you aspire to. The down-side is that no one, apart from your own students and postgrads (who don’t know any better), will take a blind bit of notice of what you have to say. The assumption that academic paper production and distribution automatically translates into consumption is one of the most widespread and mistaken myths in intellectual life. Your occupation of scholarly shelf-space, may well be noted, but your me-too product is likely to remain resolutely unmoved — just like its minute readership.

If, on the other hand, you want to have an impact on your field, if you want to stand out from the crowd, if you want to turn the marketing world on its head, you have to write in a radical or unconventional manner. Studies of seminal scientific papers, the real paradigm shifting milestones by the likes of Crick and Watson, Einstein, Darwin, Newton or, in a marketing context, Levitt, reveal that they invariably adopt a rhetorical approach that sets them apart, that differs from the prevailing norms (Locke 1992). Indeed, one of the reasons for the initial failure of Mendel’s innovative approach to genetics was on account of his decidedly conventional account of the discovery. Of course, the disadvantage of iconoclastic revisionism is, as genre theory demonstrates, that the possibility of rejection and disparagement is significantly increased. If you break the currently accepted rules of
the publishing game, if you overshoot the academic runway, so to speak, you run the very real risk of not getting published at all. In this respect, it is noteworthy that many of the most radical and innovative thinkers in contemporary marketing scholarship — the editors of this special issue of JMM for example — are individuals who have already "made it", who have got tenure or hold personal chairs, who have managed to climb the greasy pole of academic life by adhering to the rules, and whose established reputation ensures that they are granted access to the journals and permitted to express the sorts of heretical opinions which would be unacceptable from a postdoctoral student or junior lecturer. The lesson, in short, is that if you want to make a difference play the game, or, as Joseph Heller might have put it, if you want to get ahead, get ahead.

Now, what was I saying before I digressed into a discussion of peer review? Oh yes, Kotler is Dead! The story starts in the office of a junior lecturer in a provincial university, who has just seen the results of his first student evaluation. His course on marketing theory received the lowest grades ever recorded and he is invited to discuss the matter with his head of department, Phil Kotler. Although often taken for the renowned marketing thinker — and mistakenly appointed by the university on that basis — Phileas Kotler is a deeply worried man ...

References


Lotman, Y. (1975), The Structure of the Artistic Text, Ann Arbor, Michigan Slavic Contributions.


McCloshney, D.N. (1990), If You’re So Smart: The Narrative of Economic Expertise, Chicago, University of Chicago Press.


Selzer, J. (1993), Understanding Scientific Prose, Madison, University of Wisconsin Press.


**Appendix 1**

**The Art of War (Or, How to Become a Manuscript Reviewer in Ten Easy Lessons)**

*Lesson One: War is hell.* The first and by far the most important thing to realize about reviewing for journals, is that it is a battle to the death. There’s a lot of stuff and nonsense talked about collegiality, mutual support, self-sacrifice, the community of scholars, “it’s a far, far better thing I do” and the like. Indeed, it has actually been said, though I find it much too improbable to be true, that some naive researchers have been taken in by — they actually believe — this tissue of misinformation and propaganda, which has been put about by generations of unscrupulous academics. They probably also believe in Father Christmas, the tooth fairy and that if you wish upon a star your dreams come true. Tempting though it is to exploit such touching credulity — though, as you know, I’m not that sort of person — it is time to grow up and get real. Academic life in general and the peer review process in particular is a battlefield, a theatre of war, where the objective is to win, to humiliate your opponents, to destroy everything that stands in your path and to put everyone, even your closest colleagues (especially your closest colleagues!) to the sword. Show no mercy, grant no quarter, brook no opposition, take no prisoners. Wage total war. Do or die. Exterminate, exterminate, exterminate. And remember at all times the three golden rules of reviewing: (1) if in doubt about the academic worth of a manuscript — reject; (2) if in no doubt about the academic worth of a manuscript — reject; and (3) if absolutely certain about the academic worth of a manuscript — reject! Get it?, got it?, good!

*Lesson Two: Railway timetables.* Just as certain gullible individuals still subscribe to the egalitarian and congenial ideals of the Academy — we call them “cannon fodder” in the peer reviewing trenches — so too some rookie researchers imagine that success in the art of academic warfare depends on brute strength, the big battalios, heavy intellectual weapons or copious cerebral resources. Not a bit of it. Victorious scholarly campaigning boils down to logistics, to railway timetables, to getting access to the journals, to being appointed as a reviewer and to exploiting this strategic position with extreme prejudice. Journal editors, as a rule, are an ‘orrible shower; a sorry bunch of losers, who in normal circumstances wouldn’t be permitted to clean let alone enter, the officers’ mess. They are however, contractually committed to producing a certain number of issues per year and, as a consequence, they need papers, they need reviewers and, above all, they are desperate for *prompt* reviewers. Response time, in short, is the key to victory. Most academics groan when another batch of long-winded manuscripts hits the desk and the offending material is ignored until such times as the editor’s pathetic begging letters start to arrive. An immediate response, by contrast, endears you to the greasy deadbeats, who are externally grateful for not having to debase themselves for once, and they invariably
come back for more, and more ... and more. Eventually, you are elevated to star reviewer status, they become totally dependent upon you and, not least, the slimy reptiles boast about you to the other denizens of the editorial snake pit. Then the next nonentity gets in touch, and the next, and then, before you know it, you are ready for war. The scent of victory — the smell of napalm in the morning — is already in your nostrils. The battle is over before it has begun.

Lesson Three: Forging alliances. Clearly, the long-term aim of the peer reviewing campaign is to win outright, to be the last academic left standing on the battlefield. However, in order to attain this ultimate objective, it is necessary to appreciate that it can’t be achieved immediately and, more to the point, that you can’t do it on your own. Editors may be the most inept generals this side of the Great War, but forging and maintaining alliances with them is imperative if you wish to deny your enemies access to the journals. Now, whereas you and I know that most editors deserve to be court-martialed for dereliction of duty, they like to think of themselves as sagacious and prescient leaders in their field, who are open to new ideas and radical experimentation. Hence, if you succumb to your perfectly natural inclination to torpedo every paper in sight, they pigeon-hole you as a “hard reviewer” and the flow of manuscripts slowly but inexorably dries up. The scenario planning and ballistics people inform me that the best way of dealing with this situation is a calibrated rejection ratio, where you endeavour to be quite generous and supportive — to begin with. As the editors become increasingly reliant on your expeditious reviews, however, you progressively increase the rejection ratio until you have pushed them as far as they will go. In this regard, watch out for the subtle warning signs, such as a little note which laughably asks you to go easy with a paper by an academic newcomer. When this happens, make a tactical withdrawal, collaborate with them for a while and then continue the build-up, the preparations for war.

Lesson Four: Basic training. Some people are born sadistic and others have sadism thrust upon them. Regardless of whether you are one of the lucky few “naturals” or, like most of us, find your reviewing skills forged in the crucible of battle, preparation and training remain of paramount importance. I’m sure you don’t need to be reminded of the manuscript reviewing basics, but the key to survival is very, very simple — never, repeat never, read the paper. To do so not only wastes valuable time, which can be better spent “reviewing” all the other manuscripts in your bulging pending tray, but it also runs the risk that you might actually like what you see and, in your misplaced enthusiasm, recommend acceptance without alteration. True, such is the vapidity of most marketing research that this is only the most remote of outside possibilities, but even if the manuscript is readable and, perish the thought, publishable, why deny yourself the pleasure of tormenting your antagonists with a few gratuitous insults and cavalier amendments? Take every opportunity to sap their strength, weaken their resolve, undermine their morale or, at the very least, frustrate and irritate them with every delaying tactic you can muster. That said, it is necessary to give the editor and, on certain occasions, the author, the impression that you have read the paper, though this is easily achieved with a quick scan to pick up a few misplaced commas, minor grammatical errors or, manna from heaven, a missing reference or two. If the worst comes to the worst, read the abstract and possibly the concluding paragraph, but try not to make a habit of it.
Lesson Five: Discipline and punish. Having mastered the difficult art of ignoring the content whilst retaining the support of the editor, another important issue often raises its ugly head — an extremely dangerous amalgam of arrogance, boastfulness and overconfidence. As you advance rapidly through the academic ranks and your enemies are as chaff beneath your chariot wheels, it is easy to become slothful, self-important and erroneously convinced of your invulnerability. Remember at all times that you are only as good as your next review, that you are dealing with devious individuals, many of whom are highly skilled in the art of war and are more than capable of slipping a surreptitious paper through your “impregnable” defences, if you’re not careful. Beware, therefore, of all papers with references to your own work (that means they have worked out in advance who the referee is likely to be, or, after reading your first set of remarks, who in fact you are). Don’t fall for their asinine compliments, most notably acknowledging the “helpful comments” of anonymous referees (this means that they can’t quite decide who it is). And, above all, laugh in the face of any transparently obvious attempts at ingratiaton (return unsolicited manuscripts unopened and ignore all pleas for assistance, especially from first-time authors, PhD students or analogous parasites). If you find your resolve weakening and your iron discipline starting to rust, do as I do and revert to a regime of solitary confinement, cold baths, bread and water, bed of nails, electrodes on the testicles and a hand-cranked battery. Remember, however, that it is possible to have too much of a good thing: so, no more than a few days furlough before returning to the fray.

Lesson Six: Strategy and tactics. After establishing an alliance with that mob of unspeakables in pursuit of the unpublishable and having gone through the agonies and ecstasies of basic training, the next and by any reckoning one of the most crucial lessons, concerns battlefield strategy and tactics. The all-important rule of manuscript reviewing — the absolute and guaranteed secret of success — is knowing your enemy. The question, in short, of whether you are facing new recruits or battle-hardened veterans. It almost goes without saying that new recruits with freshly PhDs are comparatively easily dispatched. As they have invariably lived fairly sheltered lives, often subscribe to idealistic nonsense concerning the community of scholars and have absolutely no notion that war is hell, a review consisting of several savage sentences unfailingly saps their very will to live. They don’t like it up’em and a swift rattle of the bayonets, coupled with a rebel yell or two, is usually sufficient to have them begging for mercy and running from the field in disarray. Frontal assaults of this kind, however, do not work with battle-hardened veterans. On the contrary, if you go over the top by expressing extreme hostility to their endeavours, they invariably cry foul to the editor and, disastrously, may succeed in switching his or her allegiance. Lily-livered renegades to a man, editorial cowardice in the face of the enemy is only to be expected, but if they decide to change sides the long-term implications can be disastrous. Particular care must also be taken when fighting on unfamiliar terrain, since the slightest slip-up in your referee’s report — such as misattributing a key reference — allows the author to challenge your authority by complaining to the editor about unknowledgable reviewers. The upshot is that you have not only lost the battle, you may well have lost the campaign in that particular theatre of operations.

Lesson Seven: Feints and manoeuvres. When forced into a head-to-head confrontation
with individuals skilled in the art of academic warfare or find yourself campaigning in uncharted intellectual territory, the trick is to feint and manoeuvre. One option is to suggest that while the manuscript under review contains much of merit, it fails to refer to a highly pertinent body of literature. Cite a couple of obscurantist French philosophers — Jacques Lacan is always good value — and if your opponent is foolhardy enough to track the material down, or even better, seeks to understand or incorporate it, you can rest assured that you’re unlikely to see a revised version of the manuscript before 1999 at the earliest. Another possibility is to play the psychological warfare card by disrupting the author’s culturally engrained expectations. Thus, for example, as most American academics are used to receiving long, detailed and broadly supportive referees’ comments, a short, sharp shock to the system can prove extremely effective. British academics, by contrast, are perfectly familiar with a few surly paragraphs and the sight of scholarly blood. Therefore, give them five pages of hell, a bombardment so heavy that they will be too shell-shocked ever to contemplate anything other than raising the white flag of surrender. Yet another possibility is to fight dirty by scrutinising and criticising every tiny detail of the research and, after the authors have written a 30 page letter explaining the rationale behind their 20 page manuscript, concoct a completely different set of concerns. String them along like this for a couple of “rounds” and, when they have successfully responded to everything you asked for, and more, simply conclude that, on reflection, the paper’s subject matter is inappropriate for the journal. So traumatic is the impact of this particular tactic that grown men have been known to burst into tears and apply for an immediate medical discharge. Meanwhile, you can revel in the realisation that yet more of your mortal enemies have unceremoniously bitten the dust.

Lesson Eight: Early warning systems. Now, some might argue that while any number of feints and manoeuvres are available, the double-blind review process serves as an effective smoke-screen between our adversaries and ourselves. If we don’t know who they are, how can we possibly decide on the most appropriate tactics to deploy? The answer to this is perfectly obvious — careful reconnaissance. Indeed, if you are capable of recognizing the tell-tale signs, everything you need to know about an author is contained in the manuscript concerned. An effective rule of thumb is that the longer the list of references, the less experienced the researcher (tyro academics have to demonstrate their credentials, after all). A list with a lot of self-citation — i.e. the least well-known name has the greatest single number of references — is invariably an assistant professor (no one else mentions their papers, so they have to do it themselves). Be on your guard, however, whenever you encounter a short list of references with no obvious signs of self-promotion. You’re dealing with a lean, mean, fighting machine and, unless you’re feeling lucky or want to make someone’s day, simply let them pass. If, on the other hand, you discover that a disciplinary heavyweight is serving as reviewer for one of your own manuscripts, do everything in your power to flush them out of the undergrowth. Goading them into a comment and rejoinder exchange practically guarantees victory since, in time-honoured David versus Goliath tradition, the onlookers are automatically on your side. Not only do the giants of the discipline have more to lose, but the very fact that they deign to descend to your level goes some way towards elevating you to theirs’.
Lesson Nine: Defensive emplacements. Constant vigilance, it must be emphasized, is not confined to offensive situations or opportunist guerilla raids on the great and good. Certain defensive positions necessitate guard duty of the highest calibre. Prominent among these is the conference circuit, where it often proves necessary to "circle the wagons". You will find that as your reviewing star rises, journal editors will attempt to capture your attention and, as often as not, impose upon you with their conscientious objector propaganda. The golden rule of these admittedly loathsome personal encounters is always to agree with what they say. Under no circumstances contradict any opinion they express about papers presented at the conference. To do so, suggests that you fail to share the editor's impeccable intellectual taste, precipitates the conclusion that he or she was wrong about you all along and word soon gets out that you are not "one of us". Remember: careless talk costs lives. Another extremely dangerous situation is the ambush, the almost inevitable invitation to act as guest editor, or even editor, of an academic journal. Make no mistake, this bait is extremely tempting, not least because it raises the prospect of offloading some of your own unpublishable pieces (naturally, you'll say, my own manuscript went through the full rigours of the refereeing procedure — of course it did!), but the upshot of entering such a valley of academic death is that, mixed metaphor notwithstanding, the tables are irrevocably turned. As you now require contributors, reviewers and the like, you are hopelessly dependent upon other academics, the very combatants you set out to destroy. Incidentally, if you do get entrapped in this manner, do not seek the support of colleagues at your own institution. Never, as they say, end up fighting on two fronts.

Lesson Ten: Covert operations. If you have absorbed all of the foregoing lessons, you have nothing or no one to fear. As a finely honed exponent of the art of academic warfare, you are perfectly capable of developing all manner of personalized manoeuvres that should stand you in very good stead when the pounding is heavy and the hand-to-hand combat is at its most ferocious. My own signature stratagem is a sort of intellectual enfilade (raking fire from end to end) which involves returning the manuscript with a short comment to the effect that, although it is a worthy albeit imperfect contribution, the paper may not cohere with the overall objectives of the journal. This effectively kills four birds with the one stone in that it (a) sends a subtle "reject" message which even the dullest of editors can decode; (b) means that I didn't actually spurn the manuscript myself, thereby adversely affecting my carefully calibrated rejection ratio; (c) implies that by my refusal to pass judgement I am a worthy scholarly citizen interested solely in the disinterested pursuit of marketing knowledge; and (d) it flatters the editor — thereby adding to my reviewing duties — by suggesting that his filthy rag actually has a set of overall objectives in the first place. In conclusion, however, let me just mention that I have several other deadly arrows in my peer reviewing quiver, not least the possibility that this entire appendix is a tissue of misinformation and which, if acted upon, is liable to alienate you from journal editors everywhere, thereby denying you access to positions of influence and leaving more manuscripts for me. But, hey, what sort of marketing academic do you take me for? I wouldn't do that to you! You can trust me...honest.