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## **Chapter 5**

### **Ethnographic Approaches to the Study of Western Art Music: Questions of Context, Realism, Evidence, Description and Analysis**

Ian Pace

#### **Introduction**

Many words have been written by scholars in multiple disciplines about the value or limitations of various practices that have come to be regarded as ethnographic. To summarise these debates would require more space than is available in a chapter of this size. But many ethnographic practitioners in musical fields make significant claims for the superiority of their approach (or of associated methods conventionally employed by ethnomusicologists) over other approaches and sub-disciplines. In the process, they often treat most pejoratively other long-established musicological practices for research and teaching, and/or Western art music per se, as the traditional subject of inquiry of much musicology. Peter Dunbar-Hall calls Australian music syllabi ‘colonialist’, whose ‘analytical perspective’ imposes ‘Eurocentric ways of understanding’ that are at odds with those of various music’s creators (Dunbar-Hall 2005; cited in Drummond 2010, 119), while Philip Bohlman talks of Western art music being portrayed by musicologists as ‘totally international and purged of any ethnic or historical particularity’ (Bohlman 1991, 255) and elsewhere of ‘the process of disciplining to cover up the racism, colonialism, and sexism that underlie many of the singular canons of the West’ (Bohlman 1992, 198). Laudan Nooshin speaks of ‘occupied musicology’ (illustrated by a picture of the Israeli Wall) and a ‘fetishist focus on music as sound’ (Nooshin 2016),<sup>1</sup> while Christopher Small calls a symphony concert ‘a celebration of the “sacred history” of the western middle classes’; and the

work of Boulez and others at the *Institut de Recherche et Coordination Acoustique/Musique* (IRCAM) no more than ‘a naive, gee-whiz celebration of the most superficial aspects of modern technology’ unless considered in the ritualistic terms he favours (Small 1987, 19, 29). Yara El-Ghadban calls contemporary Western art music ‘highly canonized, ritualized, and institutionalized’ (El-Ghadban 2009: 141), Henry Kingsbury finds little more than the reproduction of the worst social hierarchies in conservatory musical education (Kingsbury 1988), while Bruno Nettl says that music schools, like orchestras, are ‘a replication of a factory or a plantation—with its dictatorial arm-waving director, the hierarchical structure of its sections, its rigid class structure’ and claims that these are supposedly ‘run more autocratically than other departments’ (Nettl 2002, 236). Georgina Born, also investigating IRCAM, writes of ‘a music that cannot communicate, because no one will listen’, ‘the aesthetic impotence of an “autonomous” modernism’, and ‘the sense of sterility attached to composition techniques such as serialism’ (Born 1995, 6, 39, 198).

These and other writers regularly assert the need for other types of musicologists to employ ethnomusicological approaches (that ethnomusicologists could learn from other musicological sub-disciplines is a far rarer claim). As someone who continues to believe that much of great value is to be found in many established forms of musicology, in canonical composers in the Western art tradition,<sup>2</sup> the symphony concert, the conservatory,<sup>3</sup> and modernist and serialist music, including some of that produced at IRCAM, I believe the work that generates such conclusions should be subject to the same degree of critical scrutiny as its writers routinely apply to others.

In this chapter, I will focus on a particular tradition of academic writing, applying ethnomusicological approaches to the study of Western art music, not to present a thoroughgoing critique of this field (for the beginnings of this, see Pace 2020a), but to focus in particular upon a subset of writing therein, in order to examine the implications of the application of an ‘ethnographic’ approach. I will consider some critical arguments that have developed within the wider fields of anthropology and ethnography, examine the limitations of dogmatic approaches that are dismissive of other non-ethnographic methods and data, elucidate key categories of ‘description’ as opposed to ‘analysis’, and then apply my findings in the process of outlining a brief critical overview of the field. This is followed in Chapter 6 by closer investigation of three case studies.

## **Ethnography**

‘Ethnography’ is a difficult term to define with any precision, and the term has been sometimes unhelpfully conflated with anthropology in general, or the use of participant observation (see Ingold 2008; Hockey and Forsey 2012). Entries in a few common dictionaries and encyclopedias define it literally as ‘writing about people’ (for example Bruce and Yearly 2006, 95; Hammersley 2007, 1479; also Ingold 2014, 384–5), while many also focus on participant observation of activities of particular groups of people (Warren 2000, 852; Bloor and Wood 2006, 176; and the aforementioned). In the *Oxford Dictionary of Sociology*, the only further qualification is ‘producing a written description thereof’ (Scott 2014, 223; a similar view is found in Johnson 2000, 111); others add ‘the description and evaluation of such activity’ (Abercrombie, Hill and Turner 2006, 136), or allude to Clifford Geertz’s concept of ‘thick description’ (Geertz 1973, 3–30). A few go further, for example demanding

‘analysis: the generation of concepts, patterns, or typologies from thick description, and their linkage to concepts, theories, and literatures already established in the discipline’ (Warren 2000, 852; for a looser demand for analysis, see Fetterman 2010, 2–3). John D. Brewer takes a freer view, arguing that ethnography is ‘not one particular method of data collection but a style of research’ and posits that ‘travellers tales’ written over the centuries count as a type of ethnographic research. However, Brewer views ethnography proper as only beginning in the twentieth century, coming out of British social anthropology, closely linked to colonialism but also to the Chicago school of sociology, and applying observational techniques to marginal social groups—though in the latter case it was more likely to be called simply participant observation or field research (Brewer 2000: 11–13; see Hockey and Forsey 2012 for a critique of this equation). Most writers seem to agree that an ethnography requires some sort of fieldwork (which can take various forms), as one source of data (Paul Atkinson and Martyn Hammersley argue that ethnographers generally employ multiple sources of data (2007, 3)); opinions differ on how much else is required.

The idea of fieldwork has been a fundamental aspect of ethnomusicology throughout much of its history (see Barz and Cooley 2008 for extensive discussion of this). In more recent times, however, a number of social scientists have taken a more critical attitude specifically towards aspects of this and some other established ethnographic practices and methods, though without necessarily rejecting the discipline.

A ‘postmodern turn’ in ethnography began in the 1980s with critiques of the conventions of what was labelled ‘ethnographic realism’. This latter was said to entail a limited presence for the ethnographer, downplaying of individuals in favour of groups, a focus on everyday experience, issues of representation, extrapolation of data

and so on. Critics of this approach favoured more creative approaches to ethnographic *writing*, drawing upon experimental literary models. This type of turn went hand in hand with a questioning of the purported objectivity and dispassion of the ethnographer and other assumptions, not least in light of post-colonial concerns (see Marcus and Cushman 1982; Marcus 1986; Clifford and Marcus 1986; Geertz 1988; Clifford 1988). In one of a series of influential writings, James Clifford evoked Mikhail Bakhtin's conception of 'heteroglossia', entailing a 'bewildering diversity of idioms', and the fact that not only writing itself, but also the use of 'texts' that are taken away from fieldwork, are far from disinterested acts (1988, 22–3, 39–42). Clifford explored different ways of dealing with this, most obviously the use of regular long quotations from informants, to provide an alternative to the singular vantage point of realist work (46–53).

In 1992, Martyn Hammersley, in a far-reaching critique that modified in a cautionary manner some of the prescriptions of 1980s writers on ethnography, argued that approaches based on data collection involve an imposition of the researcher's assumptions, are contrived and used to generate questionable generalisations, rely upon what people *say* they do (rather than observing what they *actually* do and grappling with the dialectic between the two), and as such constitute a reification of social phenomena (1992, 11–12). Nonetheless, he also argued that ethnography entailed discovery of the social world through observation and participation, consideration of social processes and the meanings behind them, in order to produce 'theoretical', 'analytical' or 'thick' descriptions, grounded in concrete events, but allowing for generalisation, the greatest emphasis being placed upon *theoretical* description, employing wider concepts and theories (12–13).<sup>4</sup>

Anticipating some critiques of what were then relatively recent postmodern turns in various disciplines, Hammersley was sceptical about fetishisation of novelty (either of presentation or subject matter), and took a cautionary attitude towards political advocacy (Hammersley 1992, 14–16, 22–3). He also questioned those ethnographic rationales whose advocates dismiss judgment from others, or the claim that ethnographic models constitute theories in any conventional sense; such advocates present them instead as ‘contributions to a public dialogue that should compete on equal terms with those from other sources’. By claiming immunity from critique, Hammersley worried that ethnographers’ work might not be viewed as *scholarship*, and pointed out that such a view might imply that ‘both factual and value claims must be judged solely in terms of their pragmatic utility or their market appeal’ (15). Nonetheless, he was also critical of ethnographic realism and the idea that there was ‘one true description that the ethnographer’s account seeks to approximate’ (24).

Hammersley also noted a range of critiques of highly selective ethnographic descriptions, when the latter reinforce already-existing theories or priorities, whether to bolster Marxist convictions, or to discover a multiplicity of ‘deviants’. Nonetheless, he did not conclude from this that the problem is one of imposed values, in contrast to some supposedly objective description of how things really are, but instead saw it as the deliberate omission of relevant things, and as such an insufficient guiding principle of discovering what is true (25–7). Above all, he emphasised the crucial factor of ‘understanding events *in context*’ (22–3).

In 1995, John van Maanen registered a range of then-current critiques of ethnography, on the basis of various of the arguments above, and also noted a large degree of inertia in ethnographic practices (Van Maanen 1995, 2–4, 7, 45–72). While essentially supporting others who had emphasised the role of language in shaping



representation, and keenly aware of the problematic nature of concepts such as ‘truth’ and ‘reality’, van Mannen also noted how rare it still was for ethnographers to overturn previous representations by restudying the same group of people (15–18), in contrast to various scientific and historical methods in which data is subject to repeated scrutiny.

In the same volume as van Mannen’s essay, Harry F. Wolcott sought to draw a clear distinction between the merely qualitative/descriptive and the specifically ethnographic, insisting that the latter term referred ‘both to the *processes* for accomplishing it—ordinarily involving original fieldwork and always requiring the reorganization and editing of material for presentation—and to the presentation itself, the *product* of that research, which ordinarily takes its form in prose’, whilst insisting it is underlaid by a rationale of ‘cultural interpretation’ (Wolcott 1995, 82–3, 93–5). In contrast to this, Wolcott employed the term ‘haphazard descriptiveness’, coined earlier by George E. Marcus and Michael M.J. Fischer to characterise the view presented by E.E. Evans-Pritchard of the work of Bronisław Malinowski and Margaret Mead, in contrast to his own analytical approach (Marcus and Fischer 1983, rev. 1999, 56; Evans-Pritchard 1951, reprinted 2004, 96-104). Wolcott used the term to indicate a rather distracting process of simply listing different pieces of factual information without further interpretation (Wolcott 1995: 85–6), a situation that will be encountered in some of my case studies in Chapter 6. Perhaps even more importantly, he insisted that culture was ‘an abstraction based on the ethnographer’s observations of actual behaviour’, which they attempt to pull together into a comprehensive picture, though with caution (86–7). Simple inclusion of a good deal of detail, incorporating vague references to culture, or even simply labelling one’s

work 'ethnographic', were amongst strategies he identified as superficial means to make a study 'more ethnographic' at an advanced stage in the process (90–97).

In a general introduction to social and cultural anthropology, Thomas Hylland Eriksen took an appropriately sceptical view towards idealised accounts of fieldwork and many of the more exalted claims made for it, noting its over-reliance upon rigid dichotomies, and arguing that anthropologists' careerist concerns may be more significant than any particular love of generating knowledge (Eriksen 2015, 32–51). He also noted how anthropologists have been inclined until recently simply to account for a particular society or culture without explaining any of the causes such as might be possible through more conventional historical study (39–40, 43–4). According to Eriksen, this latter issue, combined with the tendency towards 'realism', renders much ethnography extremely susceptible to reification (he does not use this term here, but his argument is of this nature), whereby a particular existing and historically contingent state of cultural affairs is presented as if it were innate and inevitable. Such a neo-Brechtian critique also constitutes a response to a legacy of studying non-Western musics and cultures as if they were unchanging and timeless (Nettl 1985, 12–17).

The unhappier ways in which the history of anthropology was enmeshed with colonialism have taken their toll upon ethnographers (as acknowledged in Evans-Pritchard 1951, repr/ 2004, 67; see also James 1975; Asad 1994, 59–62, and the latter's claim that 'typification' entails 'the imposition by the anthropologist of an idealized cultural system on remembered and recorded data' (65)). In theory at least, the approach advocated by Clifford, involving quotations from informants, is a means to avoid ethnography's entailing an act of domination (Madison 2005, 7–8, citing Noblit, Flores and Murillo 2004). To this end, Charles Kurzman, echoing Clifford,

also emphasised the vitality of allowing the subjects of inquiry to ‘speak up for themselves’, presenting as the ideal ‘to give the subjects a voice in the academic world’ while recognising that this may have the opposite effect because those subjects ‘may not feel qualified to participate in academic debates’ (Kurzman 1991, 267).

A different type of critique came from anthropologist Tim Ingold, who, while making clear his continuing belief in the value of a particular conception of ethnography, expressed his frustration with overuse of the term ‘ethnographic’, which he said ‘appears to be a modish substitute for qualitative’ (echoing Wolcott), generating a surplus of secondary literature, often as a substitute for actually engaging in the activity (Ingold 2014, 383–5, 391). Ingold agreed with others that the term ‘ethnography’ should not in itself be seen as defining of anthropology, and sounded a wry note about the spinning of fieldnotes as ‘data’, and the projection backwards of conceptions derived at a later stage, including of one’s own ethnographic experiences (384, 386). As such, he suggested dropping the term ‘ethnography’ altogether, as many cases are simply ‘participant observation’, though he was also sceptical as to whether the participant can really observe at the same time as participating (386–8). Ingold suggested such activity constitutes above all an *education* for the anthropologist that involves an especially attentive mode of being, and entails a ‘way of working’ rather than a ‘method’ (388–91).

But perhaps the most incisive critique comes from outside of the anthropological profession, in a book by law professor Steven Lubet. Lubet was drawn to investigate further after the success of Alice Goffman’s 2014 study of low-income African-American communities, *On the Run*, leading him to entertain some distrust towards the methods and ethics employed. Surveying a wide range of urban ethnographies, Lubet asked a range of standard scholarly questions: (i) to what extent

have the ethnographers relied on rumours or hearsay?, (ii) how much have they fact-checked their sources?, (iii) have they ignored inconvenient evidence?, (iv) have they accepted the word of unreliable witnesses?, and (v) have their arguments exceeded what could be factually substantiated? (Lubet 2018: xiv; my numbering).<sup>5</sup> In particular, he drew upon Mitchell Duneier's conception of an 'ethnographic trial', in which the ethnographer must defend their work against charges of malpractice and show that they have provided readers with 'a reasonably reliable rendering of the social world' (Duneier 2011, 2). Duneier notes that:

When ethnographers don't have to worry about hearing from the witnesses they have never met or talked to, they more easily sidestep alternative perspectives or deceive themselves into thinking that these alternative perspectives either don't exist or don't have implications for their developing line of thinking. (Duneier 2011, 3)<sup>6</sup>

Drawing upon Duneier's observation of a general lack of expectation of fact-checking among ethnographers, Lubet argued wryly that in this respect 'An ethnographer's work might be incomplete or inaccurate, but it isn't malpractice if everyone else does it too' (2018, 5). Subsequently he found a wide variety of practice in the ethnographies he scrutinises, with some of the more questionable examples overly reliant upon hearsay, ethnographers offering opinions on specialised areas that exceed their professional expertise, lack of scrupulousness in checking available documentation, use of questionable recollections from informants and taking unreliable witnesses at face value, editing so as to cherry-pick evidence that supports a hypothesis and ignoring that to the contrary, unwillingness to question rumour or folklore, and various abuses of anonymisation, for example through creating composite individuals from multiple live subjects, without making this apparent to the

reader, or altering details from anonymous informants such as may change the arguments (15–108). This is not to say that there were not exemplary ethnographies with respect to his criteria, in particular Matthew Desmond's *Evicted: Poverty and Profit in the American City* (2016), which Lubet held up as a model. Amongst his conclusions were that ethnographers should lessen their reliance on unsourced generalities, avoid composites, document how and when there have been 'minor' changes to data, clarify the distinction between direct observation and that from other sources, and what is hearsay, rumour or folklore, be more sceptical of informants, include all contrary facts and inconvenient witnesses, give as clear detail as possible for sources and informants, including dates and natures of communications for anonymised subjects, allow third parties to check field notes, and have ethnographers routinely fact-check each other's work (Lubet 2018, 136–7). He also cited Duneier's account of Stephen Steinberg's concept of 'the ethnographic fallacy',<sup>7</sup> an 'epistemology that relies exclusively on observation', which 'sharply delineates the behavior at close range but obscures the less visible structures and processes that engender and sustain the behavior' (Duneier 2016, 343), resembling some of the arguments of Eriksen.

While some ethnomusicologists have moved away from the 'realist' model and explored experimental forms, and employed long quotations to emphasise a 'dialogical' approach, many of the writings by such scholars on Western art music are deeply vulnerable to the other types of critique I have detailed. These will guide the following analysis, and I will return to the specific critiques in the conclusion to Chapter 6.

## **Territorial Issues**

In commenting on ‘traditional musicology’, Stephen Cottrell asserts that its practitioners

cast themselves in the role of informed experts, providing readings or interpretations of musical practice *which remain subjective*, notwithstanding that their interpretations or readings of given artworks may attempt to establish links with other social or cultural practices in a manner analogous to ethnomusicological method (Cottrell 2004, 4).

His disparaging use of the term ‘experts’ anticipates populist rhetoric that became commonplace in the United Kingdom over a decade later, most memorably in the words of Conservative politician Michael Gove, when speaking of the lack of economists who backed Britain’s exit from the European Union (Mance 2016; for a rigorous, nuanced treatment of the subject of expertise and its detractors, see Nichols 2017).

Cottrell does acknowledge that ethnomusicologists also cast themselves in the role of experts, but he does not wish to give their views precedence over ‘others for whom the music may be equally important or meaningful (and often more so)’ (Cottrell 2004, 4), which begs the question of what is the value of education and associated expertise at all? His associated claim that traditional musicologists seldom allow ‘room in their texts for other voices’ is untenable in view of the reality of much scholarly work undertaken in that area, and will be discussed further in Chapter 6. As for ethnomusicologists writing on Western art music, it is rare that they allow for any dissenting *expert* voices (as opposed to evoking a range of canonical figures: Alan Merriam, Marcel Mauss, Geertz, Pierre Bourdieu and so on). As I will show in his own case and that of Amanda Bayley and Michael Clarke, ethnomusicologists also often ignore other relevant scholarship when produced by scholars from other sub-

disciplines. Many traditional musicologists *are* experts in a very meaningful sense of the term: for example, many Schubert scholars demonstrate intense knowledge of a breadth of repertory, musical and textual conventions and provenance, historical conditions of performance, a wide range of poetry, aesthetics, and existing analytical and other literature. Anything comparable to this sort of range of learning (as distinct from a certain amount of what is essentially focus group data) is almost nowhere present in the particular branch of ethnomusicology I will discuss below.

Jonathan Stock is a little clearer in his definition of ‘traditional musicology’, saying that he refers ‘mostly to what we might today more formally identify as historical musicology’ (Stock 1997, 41), thus excluding analytical musicology, study of performance practice (dating at least back to the 1920s), music aesthetics, acoustics, and much else, all with long and rich traditions. There is not the space to deal here with the various false dichotomies entailed in his Manichean opposition between his object and idealised ethnomusicology (for more on this, see Pace 2020a); suffice to say that Stock’s arguments, like those of Cottrell, rely quite fundamentally upon the construction of an ‘other’ that is portrayed as ‘traditional’ musicology.

Cottrell writes elsewhere that ethnographic approaches to Western art music ‘antagonize a small number of musicologists, who continue to argue for the primacy of the musical text and decry sociological or anthropological approaches that fail to put the musical sounds and structures at the centre of the methodology’ (Cottrell 2017, 322), echoing Nooshin’s description of a ‘fetishistic’ focus on sound, noted above. My own scepticism is towards those approaches that do not simply fail to place sounds (not just ‘texts’) and structures at the centre of the methodology, but ignore them altogether, producing what I have elsewhere called simply ‘musicology without ears’ (Pace 2016a). Without a focus on sound, what gives musicology any

separate disciplinary identity? Some, for sure, might view the loss of the entire discipline as no bad thing, leading to a situation (already existing in some institutions) in which all types of musicologists would instead be housed in sociology, anthropology or cultural studies departments (on this, see also Nettl 2005, 189–91), and academic positions and teaching would not necessarily require any prerequisite skills specific to music (see Pace and Tregear 2020). But this move would likely lead to the near-dissolution of musicology altogether, with musicologists—including ethnomusicologists—consigned to an extremely marginal position if their work is indeed desired at all, compared to those with more specialised training in the other subjects.

But what else might be wrong with musicology without ears? Beyond the unhappy spectacle of a profession eschewing aural and associated intellectual skills, it can lead to serious misrepresentations of music when that music is understood as little more than a stylistic ‘brand’. Björn Heile has noted how Georgina Born, in her 1995 ethnography of IRCAM, asserts that ‘for decades’ the courses at the *Darmstädter Ferienkurse* were characterised by serialism, subsuming a wide range of heterogeneous developments under one heading as part of what Heile calls part of a ‘fundamentally distorted picture of modernism’. Born’s lack of knowledge of the many developments of open form composition, aleatory technique, post-Cage experimentalism, live electronics, heavily politicised musical aesthetics, debates about *Weltmusik*, free text scores for improvisation and more at the courses at Darmstadt, are said to result from her ‘dependence on secondary literature instead of independent readings of composers’ works’ (Heile 2004, 167–8; see also Pace 2011 for falsification of the claim that Darmstadt was dominated by serialism even in the 1950s). Richard Herrmann, in a more detailed critique, examines the simplistic



dualisms upon which Born's study (which he calls 'a polemic; it is not scholarship' (Herrmann 1997: 6.1)) is founded, and how consistently falsifiable they are in the case of the repertoire in question, as well as how ill-defined and understood are terms such as 'serialism', 'neoclassicism', 'tonal', 'modal' and so on, concluding that 'Born has difficulties not only with aesthetic concepts but also more directly with technical concepts and even relatively recent historical facts about music' (4.13). He attributes this to 'her comparative lack of technical training or historical study of music' and the fact that 'she did not analyze any of the music under discussion', relying instead upon 'blatant, unsubstantiated, and unmediated value judgments against modernism, at least as practiced by IRCAM' (4.13; 5.1). Heile recognises that it may be harsh to judge a 'non-musicologist' in such a fashion (though at the time of writing, Born holds a position as 'Professor of Music and Anthropology' at the University of Oxford, associated with the Faculty of Music as well as the Institute of Social and Cultural Anthropology, which surely suggests some type of musicological expertise). Arguably, a purely anthropological study, for which music is only important as a window onto a wider culture, could be undertaken successfully without any engagement with the actual music produced (though I remain sceptical and have not seen any positive example of such a thing), such as would require listening; but this is insufficient for Born, who does wish to pronounce on the actual music, but without having to engage with it in any detail.<sup>8</sup>

Similar problems can be located throughout related literature, for example in Yara El-Ghadban's pronouncements, cruder than those of Born, on contemporary Western art music, derived primarily from the views of other anthropologists and ethnomusicologists, without external verification, and so disregarding Hammersley's dialectic and Lubet's questions (i), (ii) and (iv) (El-Ghadban 2009); or Hettie

Malcolmson's attempts to construct a trinity of 'mainstream'/'new complexity'/'experimental' approaches to composition, on the basis of casual remarks by composers, with no exploration or evidence of understanding of the provenance and aesthetic meanings of such concepts (Malcolmson 2013, 128–9). In all such cases, Hammersley's requirement of contextual knowledge is also significantly lacking, and it is difficult to see how this could be remedied without genuine musical engagement. Those who doggedly exclude aural and analytical evidence from their enquiries will always be limited in the scope of their endeavours.

### **Description and Analysis: Journalistic and Scholarly Writing**

Earlier I mentioned differing requirements of 'description' and 'analysis' for ethnographic writing. This dichotomy is used as a yardstick by some academics to distinguish 'scholarly' from 'journalistic' writing, though it has not often been subject to more sustained exploration (though see some of the essays in Pace and Wiley 2020), and is ignored in some textbooks on academic writing (e.g. Murray and Moore 2006; Bailey 2011). Certainly one characteristic would be the implied readership, whereby scholarly writing is produced for other scholars with the requisite expertise, while journalistic writing aims for a wider constituency.<sup>9</sup> In a rare article attempting to identify wider characteristics, Andrew Duffy argues for some continuities, whereby 'Scholarly writing can be viewed as an advanced form of journalism', through more rigorous and transparent methods, careful use of data, and enhanced editing (Duffy 2015, 7–10). Whether there is such a clear hierarchy of editing quality between publications produced by academic outlets and others is certainly open to question, while in a musical/musicological context, methodological rigour can have a great many meanings.

Two brief examples will serve here as an indication of clear distinctions of style, approach and intended readership.<sup>10</sup> The first is from Graham Johnson's book on Gabriel Fauré's songs, here looking at Fauré's setting of Paul Verlaine's poem 'Une sainte en son auréole', Op. 61, No. 1:

Fauré's usual time-travelling music often suggests the seventeenth-century madrigal, or the eighteenth-century *fêtes galantes* of Versailles, but here his part-writing and counterpoint imitate the rigorous devotion of a medieval motet—somewhat 'archaïsant', as Suckling puts it—without sacrificing a jot of his own style or sound-world. [...] The music has a firmness that discourages sentimentality; Fauré understands that the function of this chatelaine is to improve her husband, as well as to motivate and inspire him—a kind of purity by example. The entire song breathes an air of seraphic calm despite a tempo that moves forward with implacable assurance—as if to say 'this is ordained'. There is some word-painting: a D $\flat$  concealed in the accompaniment (with the following F $\flat$  a minor third above) mournfully suggests the 'golden note' of the horn in the distant forest. This unexpected touch from a composer who usually refrains from such illustrative details reminds us of works by Debussy as yet uncomposed—the Verlaine setting *Le Son du cor*, and Golaud's horn-call in the prelude to *Pelléas et Mélisande*.

The curvaceous characteristics of theme A are to be heard traced in the introduction to the song, and in many a bar of the piano writing, but the actual theme appears for the first time only at bar 15 (just before 'de grâce et d'amour; Example 5.1); Jankélévitch refers to this six-crotchet motif as being 'as pure and lily-like as the neck of a woman.' This 'Carlovingien' theme

reappears three times in the song's last two lines; we will hear it again in Nos 3, 4, 5, 6 and 9 of the cycle. (Johnson 2009, 229–30)

This can be compared with the second example, from Beate Perry's book on Robert Schumann's *Dichterliebe*, specifically here about the song 'Wenn ich in deine Augen seh'', which is accompanied by a musical example of bars 12–17.

Obviously, the singular prominence and impact with which Schumann projects the diminished-seventh chord owes much to the repetition of the E-minor chord in the preceding bar (b. 12) from which it emerges. While this rhythmic and intervallic structure of b. 12 has been established as the song's nucleus from the very start (bb. 1–2), and although this motive has been transformed constantly up to this point through intricate interaction between voice and piano (bb. 1–11), its unison presentation at the words 'doch wenn du' (b. 12), just before 'sprichst', is certainly new. Earlier, the piano either followed the voice (b. 2) or preceded it (b. 4) in the statement of the motive. Thus, the repeated E-minor chords before 'sprichst' (b. 12) with their new-found congruence between voice and piano convey a sense of 'expectation'.

The diminished-seventh chord before 'Ich liebe dich' is the structurally crucial aspect of 'Wenn ich in deine Augen seh'', leading as it does to the striking suspension on 'liebe' (b. 14). Let us note here the movement of the voice: a third-step up followed by a whole-step down. This lyrical detail will reappear conspicuously three times at a later stage in *Dichterliebe*.

In bb. 13–14, that is from 'sprichst' through 'dich', the song conveys a sense of stasis: for a moment, nothing moves as the diminished-seventh chord spreads its veil over 'sprichst' in order to come to a halt on 'liebe dich', again constrained in terms of time and tempo by a *ritardando*. Motion resumes again

all the more perceptibly at ‘so muß ich weinen bitterlich’ (bb. 14–16), which the piano continues to ‘sing’ three times, decreasing in tempo (*ritardando* in bb. 17 and 19) and dynamics (*p* in b. 16 and *pp* in b. 18). The ambiguity created by the diminished-seventh chord is resolved locally on ‘dich’; however, this resolution is only tentative, as the A-minor sonority (on the second beat of b. 14) functions as ii<sup>6</sup> of G and is hence tonally distant from the home key. The real resolution is the ensuing V<sup>7</sup>-I progression (bb. 15–16) on ‘bitterlich’. (Perry 2002, 185–6)

Johnson describes the song, relates the musical idiom to historical precedent, considers briefly the aesthetic context, and then provides a few stimulating if quite general thoughts on the music with a small amount of technical or analytical detail, as well as relating the song to some music of Debussy. Perry’s text does not employ any formalised theories beyond reasonably straightforward harmonic analysis,<sup>11</sup> but examines the microscopic details of the song to reveal aspects less than obvious from merely aural familiarity. Elsewhere in the book she combines this approach with an intense critical consideration of early Romanticism and its theoretical models. In short, Johnson presents context, description and a certain amount of ‘appreciation’ for a general reader; Perry writes more detailed analysis and aesthetic theory for specialists. The difference is not so much of value as of type: between the ‘general’ and the ‘specific’, between species of writing that can be identified as description and analysis respectively.

But consider also the following passage, by Paul F. Berliner, an ethnographic study of the music of the Zimbabwean Shona People.

A beginning student of the *mbira dzavadzimu* usually first learns the *kushaura* or lead part of an *mbira* piece. After learning the *kushaura* parts to several

compositions he then learns the *kutsinhira* parts. As a matter of personal taste, some musicians specialize in either the first or the second part to mbira pieces. Others methodically learn both the *kushaura* and *kutsinhira* parts of mbira compositions. Gradually mbira students expand their repertoires not only with new pieces but with the traditional variations on the classic pieces they have mastered.

Once a student has developed confidence in his playing, he begins to experiment with the subtleties and nuances of the compositions. By slight changes in the rhythmic relationship between the two interlocking hand parts he can effect a change in the total feeling of the piece and its overall phrasing. By shifting accents to different keys or alternating accents from one hand to the other, new parts spring out from the polyphonic fabric of the piece. As the student progresses, he develops enough control to add new high-tone embellishments to the basic mbira parts, thus increasing the complexity of the mbira music. He can listen to the music in different ways, almost as if he were a member of the audience and his hands were playing independently of his body. At this stage the mbira player achieves the level of playing described in Chapter 5, in which the musician exchanges ideas with the mbira. This relationship between the mbira player and his instrument presents one of the greatest challenges for the learner. (Berliner 1978, republished 1993, 144–5)

To my mind this passage, which is typical of a significant part of the book,<sup>12</sup> entails description more than analysis, and as such is closer to the writing of Johnson than that of Perry. The likely remoteness of the subject matter to many Western readers might be said to render the work more for the specialist, but that could equally be said of much journalism or travel writing exploring remote or exotic locations.

‘Description’ is rarely likely to be a positive epithet if applied to academic writing on Western art music, but the concept is viewed quite differently in cultural anthropology and some branches of ethnomusicology, especially since Clifford Geertz’s theorisation of ‘thick description’ (Geertz 1973, 3–30). Geertz’s concept was taken from Gilbert Ryle, specifically what Ryle called ‘practising a burlesque of a friend faking a wink to deceive an innocent into thinking a conspiracy is in motion’. It may overlap with what I identify as analysis, encompassing synthetic interpretation: it involves knowledge of cultural codes and other contextual information; Geertz only eschews the term ‘analysis’ because of associations with ‘the cipher clerk’ (Geertz 1973, 6–9). Geertz was certainly not antipathetic to the application of theoretical ideas, which he believed were in general adopted from those found useful in earlier relevant studies, but refined, modified and re-applied (27). Certainly Perry’s writing would come closer to Geertz’s thick description than that of Johnson, or for that matter Berliner and some others.

Wolcott, however, believed incorporation of thick description to be ‘a thin basis for ethnographic claims-making’ (Wolcott 1995, 91), but he appears to have conceived the concept more narrowly than Geertz. Hammersley went further in arguing that ‘ethnographic descriptions are theoretical only in the sense that they *apply* theories to the understanding of particular phenomena’, and that the question of why one type of description is provided over another remains, often to do with questions of relevance rather than solely of truth (Hammersley 1992, 24–5). But the distinction between description and analysis made by Eriksen is most meaningful in terms of critical scholarship: for him, in an ethnographic context, description is generally closer to the conceptualisation of the natives studied, and will rely heavily upon direct quotations, whereas the alternative is to engage in comparative social or

cultural analysis and render the area studied in terms of concepts that are not common in the society itself (Eriksen 2015, 46–7).

Where there is a tendency towards a less ‘thick’ form of description rather than analysis—or description simply followed by taxonomy—then the work concerned can be difficult to distinguish from other non-academic phenomena. The very act of creating an ethnography or other type of work drawing upon words from human subjects can, for sure, never be neutral or dispassionate, because of the inevitable processes of selection, editing, translation (see Asad 1986), organisation, presentation and so on that accompany the production of a written text. Furthermore, the choices involved are rarely at all transparent, meaning that the reader has to take on trust that the ethnographer has acted fairly and equitably, a form of trust I am reluctant to bestow where there is clear evidence of a casual or heavily biased approach towards, or simple lack of appropriate knowledge of, other types of sources. Nonetheless, when such ethnographic writing embodies a critical intelligence, subtlety and creativity, then the description may approach a type of analysis. I will return to this dichotomy at the end of Chapter 6.

### **The Ethnomusicology of Western Art Music**

Some of the ethnomusicology of Western art music can be conceived as a particular variety of what has been conceptualised as ‘indigenous’ or ‘at-home’ anthropology (Fahim 1982; Jackson 1987), when carried out by active musicians. Other than in a few cases such as that of Cottrell I will discuss in the next chapter, this situation is the exception rather than the rule, as many of the ethnographers do not otherwise function as part of the groups they study, so can be considered etic rather than emic. One of the earliest such studies was an article published by the sociologist Robert Faulkner in



1973, a fairly balanced account of perceptions relating to hierarchies between orchestral players and conductors, based upon seven months of observation and interviews. Then in 1982, Catherine M. Cameron's completed her PhD dissertation, later published in revised form (1996), on 'experimentalism' in American music. Whilst making some important observations about the nationalistic ideologies that underlie the aesthetic positions of many of the composers, this study provides only a loose and vague definition of the 'experimental' (which had been extensively theorised prior to Cameron's thesis, in Nyman 1974; Smalley 1975; Schnebel 1976; Ballantine 1977; and Beinitez 1978; the earlier provenance of the term is analysed in detail in Metzger 1985; Blumröder 1995; Brooks 2012). It relies almost exclusively upon the pronouncements—and thus self-fashioning—of American radicals on their difference from their European counterparts, rather than engaging with the latter's work (thus collapsing Hammersley's dialectic). The result is a distorted picture of reality, which sometimes replicates some of the nationalistic and xenophobic assumptions it was intended to critique.

In 1987, Christopher Small explored the traditional priorities of a cultural anthropologist: the rituals associated with a symphony concert. Considering the spatial layout of buildings, players and audiences, the logistics of obtaining tickets, behavioural conventions, stage conventions, concert format, the character of music commonly played and so on, Small disregarded questions of historical contingency and variability in these factors, instead portraying the ritual as if atemporal, in the manner critiqued by Eriksen, while also making extravagant claims about the motivations of listeners, based in part upon a stereotypical view of various composers (Small 1987).<sup>13</sup>

In the sense identified by Madison (2005), Small's work shows a clear domination of his subjects, a trait mirrored in subsequent publications as the field expanded, especially with the appearance first of Henry Kingsbury's *Music, Talent, & Performance* (1988) and seven years later Bruno Nettl's *Heartland Excursions* (1995). Both of these titles were studies of music conservatories, though Kingsbury made the extravagant claim that his work was presented 'less as an ethnography of a conservatory than as an ethnography of music' (Kingsbury 1988, 14). It is a heavy-handed study, animated by Kingsbury's reductionist view that music is 'a metaphor of the society in which it takes place' (8). The book features many generalisations about music, pedagogy, analysis, canons, prestige, audiences and so on, which cannot be tested without a separate investigation into Kingsbury's unnamed institution,<sup>14</sup> though many of these can easily be falsified in a wider context. For example, his all-purpose claim that the authoritative pedagogue requires the authoritative musical score, or Urtext (Kingsbury 1988: 15) is certainly true of some teachers and performers, but decried by many others, as is made clear in interviews with leading teachers Jorge Bolet (in Kozinn 1982) and Earl Wild (in Dubal 1985, 342–3). Kingsbury did not hesitate to attribute motivations to teachers, students and administrators, and again it is impossible to gauge whether he is providing a fair account of the totality of what he experienced, or simply selecting information that bolsters a particular ideological agenda. He also attributes views to a range of musicological 'others' (for example about the supposed undesirability of considering social issues in music, without providing any actual examples of such individuals (Kingsbury 1988, 16)). His observation of how naming teachers can lend prestige to students (44–6) is not unfair (though by no means necessarily the only reason), but is rather ironic in light of

Kingsbury's own biographical description of himself as 'a one-time disciple of the late Alan Merriam' (Kingsbury no date).

A large section of the book is devoted to the concept of 'talent',<sup>15</sup> which is viewed disparagingly, though without giving evidence from psychology or elsewhere that might bolster the implied converse position with respect to the performance of Western art music—that there are no differences in aptitude, and so all could achieve the highest level (67–80). Some of the stock concepts Kingsbury encountered, such as 'expression' and playing 'with feeling' (64–9), are vague and amorphous terms if simply taken at face value, but it should not be too difficult to see how they may be ciphers for more specific perceptions relating to phrase shaping, rhythmic flexibility, subtlety of voicing, judicious use of vibrato, pedalling and so on. It is hard to imagine the value of any conservatory in which teachers and examiners did not pay attention to such rudimentary matters from students.

Nettl, who wrote in his memoir of having been inspired by Kingsbury's study (thus engaging in his own reciprocal process of esteem and exchange) (Nettl 2002, 230) took a similar approach, and notably disregarded a good deal of historical and contemporary evidence—as for example in his simplistic picture of the history of the orchestra<sup>16</sup>—in order to pass judgement 'from above' on his area of study, replete with loaded language and an almost wholly derogatory view of Western art music compared with an idealised one of musics from elsewhere in the world.

Just a year before the appearance of Kingsbury's book, Judith Kogan published her *Nothing But the Best: The Struggle for Perfection at the Juilliard School* (1987). Kogan was herself a graduate of the school, and provides a scandal-ridden tabloid account of the place, describing the ferocious competition, the ritual of auditions, the huge pressures upon students, the godlike nature of the teachers, the

central role of competitions, the bitchy gossip in the cafeteria and the students' love lives, all padded out with salacious anecdotes and conversations reported with relish. There is little described in the book that I do not recognise from my own time studying at Juilliard in the early 1990s (as I recognise some of the less desirable phenomena described by Kingsbury and Nettl, though not in such an overwhelming fashion), but it is also clear that this is an extremely partial and sensationalist account, filtering out much else that was pedagogically inspiring, co-operative and supportive, musically penetrating or for that matter simply routine but necessary. Kogan was writing a commercial book for a large audience rather than an academic study,<sup>17</sup> but the differences between this populist text and Kingsbury and Nettl's supposedly scholarly studies have more to do with literary style than content. Kingsbury is more sober in his language, and both he and Nettl are obviously more concerned to make reference to appropriate theorists, but are not fundamentally different to Kogan in their partisan reading of some of the rituals. Kingsbury and Nettl similarly share with Kogan a uniformly dismissive attitude towards such institutions and most of those who work there, few of whom have the *academic* platform available to Kingsbury and Nettl, and so are in no more of a position to respond as equals than Small's subjects.

A different but equally influential key text from this period is anthropologist Ruth Finnegan's *The Hidden Musicians* (1989), very much in the 'realist' tradition. This is an obsessively detailed description of every aspect of music-making in the town of Milton Keynes, from orchestras, through amateur choirs and brass bands, to folk groups and over 100 small bands, packed with long passages of prose and lists of figures, right down to detailed information about who does the washing-up, puts out cups and saucers, fills the electric urn for tea and coffee and so on, in some amateur musical groups (Finnegan 1989, 241). But the findings are quite modest, such as that

there are many people involved in one or other form of music, that many find this a significant part of their lives, family and class background is a factor, or that it is apparently noticeable that men and women's voices are divided in four-part choirs. This study is clearly written but lacks any incisive research questions, and avoids critical dialogue with the subjects; the bulk of the book thus comprises description rather than analysis.

The culmination of this phase of ethnographic work on Western art music and its institutions comes in Georgina Born's *Rationalizing Culture* (1995) on IRCAM. I mentioned above some scholars' accounts of its methodological, ideological and other problems (see also Deliège 2003, 962; Watson 2011, 109–130; Pace 2016a, 2020a; Pàmies 2020). It will suffice to say here that many of the issues found above in Kingsbury and Nettl are also present in an exacerbated form in this anti-modernist polemic.

If one were to consider the criteria of Kurzman, the claim that Kingsbury, Nettl or Born are giving their subjects a voice in the academic world is absurd; the authors' work pointedly omits any perspectives that do not concur with their own and as such constitute acts of domination.<sup>18</sup> Cameron and Finnegan are, however, certainly different. Finnegan reports but does not question the views of her subjects, while Cameron is sympathetic but not uncritical. In this sense Cameron's work stands out methodologically, which makes the limitations of its insights more disappointing.

In subsequent writings, the emphasis changed, and the model from Finnegan seems to have become the primary influence. A key text for a 'second period' of ethnographic work relating to Western art music and its institutions is Kay Kaufman Shelemay's essay 'Toward an Ethnomusicology of the Early Music Movement' (2001). After a period of several months spent amongst musicians involved in the

Boston early music movement, Shelemay arrived at a series of findings, such as that ‘the early music movement [...] is powerfully informed by the creative impulses of its practitioners and the aesthetics of the present’, that musicians attested to ‘the centrality of creative activity in their conceptualization and performance of musical repertory’, that many demonstrated knowledge of their instruments’ technical aspects, but also their ‘history and social significance’, or that musicians make creative decisions about repertory. These conclusions are obvious to anyone with even a cursory knowledge of the movement and are self-evident from other scholarly literature about it (most obviously Kenyon 1988; Haskell 1988; Taruskin 1995; Sherman 1997; and Lawson and Stowell 1999). Shelemay reveals only that the Boston Early Music movement is like others elsewhere. The more complex questions are not explored in detail; the huge issue of ‘what is “Early Music”’ is passed over in a few sentences (8), while the potentially fruitful discussion of the economics of early music mostly reports musicians’ experiences of difficulties in making a living in this field, rather than giving data on earnings, subsidies and recording costs. The question of whether early music is ‘Western music’, which is discussed without any definition of the latter term, mentions a few cases of those involved with early music who have collaborated with others in the ‘world music’ field, but not the English *a cappella* movement from the 1970s onwards, which saw a marked shift away from the counter-cultural aspects of earlier medieval musical performers and groups, and had a wider international impact (see Page 1992 and 1993). The aforementioned literature and other contributions from scholars on earlier music and historical performance have engaged with these issues and others, such as the meaning of re-creating past practices in a contemporary setting, the possibility of doing so on the basis of fragmentary surviving evidence, or the value or otherwise of using instruments from later periods

than some of the repertory. The musicians interviewed by Shelemay are not necessarily verbally articulate about their work, so the burden is on the ethnographer to bring out more interesting responses through their questions. A more critical questioner might ask, for example, when musicians speak of the centrality of ‘creative activity’, what this term actually means, and how it is manifested in practice.

A wider aspect of this body of work should also be mentioned here.

Considering how regularly one reads attacks by ethnomusicologists on an allegedly inflexible canonisation process within Western art music, it is somewhat ironic to see the same process at stake with their own texts on this area. From the point at which a certain number of such texts had accumulated, they began to be presented as a type of catechism. The texts of Small, Kingsbury, Finnegan, Nettl, then of Born, Shelemay, Cottrell and others are invariably given a hallowed mention, as frequently are other texts in the ethnomusicological canon such as those of Merriam, Nettl, John Blacking, or Marcia Herndon and Norma McLeod (for examples of such canonisation, see Stock 1997, 40–1; Shelemay 2001, 5; Cottrell 2004, 4–6; Cook 2008, 48–9; Bithell 2008, 79–80; El-Ghabran 2008, 153–4; Nooshin 2011, 286–7; Dobson and Pitts 2011, 354; Moisala 2011, 444–5; Usner 2011, 415; Malcolmson 2013, 116; Nooshin 2016; Cottrell 2017). This is one manifestation of hagiographical tendencies within such work; I will deal with the question of hagiography in more detail in Chapter 6.

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<sup>1</sup> My response to this position paper, including a range of ethnographically sourced comments by non-ethnomusicologists on ethnomusicology (as Nooshin had included her own range of comments from other ethnomusicologists in her statement) can be found in Pace 2016b.

<sup>2</sup> For my wider views on musical canons, see Pace 2016c.

<sup>3</sup> I write here as one who has been publicly involved as a campaigner against abusive practices in musical education, with no illusions as to the worst things that can occur under the auspices of the conservatory. See in particular Pace 2013.

<sup>4</sup> Hammersley does acknowledge that all description involves some concepts and theories, but distinguishes ethnographic theoretical description as involving '(i) *insightful* descriptions; (ii) descriptions of *social microcosms*; (iii) *applications* of theories; (iv) *developments of theory* through the study of crucial cases' (1992, 13).

<sup>5</sup> Lubet also asks a question relating to complicity with criminal activity on the part of ethnographers, a major issue relating to Goffman, but one that is not generally relevant for musical ethnography.

<sup>6</sup> Duneier takes one of the most classic examples, Geertz's essay on the Balinese cock-fight (Geertz 1973, 412–53) and finds some potential problems here, to do with Geertz's over-reliance upon the view of the economic elite in the village in question, without observing others whose accounts might potentially contradict Geertz's claim that class resentment, in a deeply unequal community, is irrelevant to understanding

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the scene of the cock-fight. Without having access to accounts that might actually falsify Geertz's broader claims, Duneier nonetheless feels their existence cannot be ruled out (Duneier 2011, 4–8).

<sup>7</sup> Duneier cites here from an unpublished paper by Steinberg.

<sup>8</sup> For a related critique of the lack of engagement with Beethoven's music (and also of a reliance on secondary literature) in DeNora 1995, see Rosen 1996.

<sup>9</sup> Historian Richard J. Evans does not necessarily wish to draw the distinction much more widely than this, arguing that many questions of method, truth and objectivity remain the same whatever the target audience (Evans 2001, 258–63).

<sup>10</sup> These two examples, and that of Berliner below, are examined in more detail in Pace 2020b.

<sup>11</sup> Perry does elsewhere reproduce Heinrich Schenker's graph for the song in a footnote (Perry 2002, 184 n. 164).

<sup>12</sup> The rest of the book consists primarily of reportage of the views of the musicians.

<sup>13</sup> Bach entails 'secure, death-obsessed Protestantism', Chaikovsky 'barely controlled hysteria', Elgar 'the imperialist extravagance of Edwardian England' and so on (Small 1987, 19).

<sup>14</sup> Nettl himself suggests it may have been the New England Conservatory. See Nettl 2002, 236.

<sup>15</sup> Kingsbury writes that 'The analysis of musical "talent" in this chapter will proceed along lines similar to those taken in anthropological analyses of extraordinary powers that accrue to special persons, powers such as religious charisma, spirit possession, or witchcraft' (1988, 62), but spirit possession and witchcraft can be ruled out as nothing other than wholly imaginary, quite unlike religious or any other type of charisma.

<sup>16</sup> For examples of this see Pace 2020a.

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<sup>17</sup> A far more nuanced study of the Juilliard School, historical in nature (written by a former teacher of music history at the school), and certainly open-eyed to many of the issues detailed by Kogan, whilst also incorporating considerable detail on the internal politics of the administration, is Olmstead 1999.

<sup>18</sup> As Ben Watson points out, ‘IRCAM is swarming with intellectuals, any of whom could engage Born in talk about music theory, but in its ghastly attempt to reach an “objective” understanding of social relations, her anthropology leaves no room for the point-of-view of the analyst herself, who disappears, God-like, into an invisible, all-seeing eye. Adorno pointed out that it was an affront to “psychoanalyse” your friends; similarly, Born cannot avoid creaking condescension in such a supposedly “scientific” overview of the conflicts at IRCAM’ (Watson 2011, 116).