FROM FORENSICS TO CONSTRUCTIVE DESCRIPTION

>> I sometimes wonder why you want to continue to address some of the
>> audiences you do.
>>
>
> because I want to rearrange the deck chairs on the titanic; what else is there
> to do?
>
> --> join the orchestral ensemble?

I don't like the music.

This is a representation of an email conversation between Jeff Vass (the first speaker) and myself. It subsequently occurred to me that the response, though accurate, was not entirely complete. It is true, I do want to rearrange the deck chairs, but I first want to demonstrate that we are sinking without hope of salvation. Only then can we feel comfortable playing musical chairs.

As I am writing this, the Intergovernmental Panel on Climate Change (IPCC) has just published a report following a meeting in Brussels. This report does indeed seem to indicate that we are sinking. Professor Michael Parry, co-chair of the working group that authored the report outlined its findings.

> "What [scientists] have done now is finally establish at the global level there is an anthropogenic, a man-made, climate signal coming through on plants, animals, water and ice," he told reporters. "This is the first time, at the international level, and for the IPCC that there has been confirmed this signal." (Reuters Television News, http://rtv.rtrlondon.co.uk/2007-04-06/1f0b2858.html)

We are not entirely without hope yet, though, it seems: Parry's speech included reference to conclusions to the effect that we may still be able to attenuate or delay the impact of our destructive activities. But what precisely is the origin of the 'signal' that is 'coming through'?

A while ago, Soh-young Chung—my co-author in Chapter 8—gave me a working paper titled 'Towards Methodology' (Chung, 2005). She began the paper by juxtaposing extracts from two poems, one by Samuel Taylor Coleridge, the other by Wallace Stevens. The distinction that she made between them comes very close to addressing the question about the origin of the 'signal' and very close too to the transition that I am trying to establish in this book. Chung selected an extract

from Coleridge's *Dejection: An Ode*—well chosen, because the Aeolian lute—the poet himself—played by nature is presented in contradictory duet with Stevens' guitarist. But I'll replace it with a section from *The Nightingale*, only because it seems to demand less in terms of knowledge of poetry.

> And hark! the Nightingale begins its song,
> 'Most musical, most melancholy' bird!
> A melancholy bird? Oh! idle thought!
> In Nature there is nothing melancholy.
> But some night-wandering man whose heart was pierced
> With the remembrance of a grievous wrong,
> Or slow distemper, or neglected love,
> (And so, poor wretch! filled all things with himself,
> And made all gentle sounds tell back the tale
> Of his own sorrow) he, and such as he,
> First named these notes a melancholy strain.
>
> (www.online-literature.com/coleridge/642/)

'Nature as it really is' is for Coleridge, potentially tarnished by subjectivity, which must be eliminated if nature as it really is, in all its glory, is to shine through to us. Stevens strikes a different chord in *The Man with the Blue Guitar*.

> I
>
> The man bent over his guitar,
> A shearsman of sorts. The day was green.
>
> They said, "You have a blue guitar,
> You do not play things as they are."
>
> The man replied, "Things as they are
> Are changed upon the blue guitar."
>
> And they said then, "But play, you must,
> A tune beyond us, yet ourselves,
>
> A tune upon the blue guitar
> Of things exactly as they are."
>
> II
>
> I cannot bring a world quite round,
> Although I patch it as I can.
>
> I sing a hero's head, large eye
> And bearded bronze, but not a man,

Although I patch him as I can
And reach through him almost to man.

If to serenade almost to man
Is to miss, by that, things as they are,

Say it is the serenade
Of a man that plays a blue guitar.

(www.writing.upenn.edu/~afilreis/88v/blueguitar.html)

For Stevens, it is precisely the poet's imagination that produces the poetry. Professor Parry probably meant to index the signal of science—he was, after all, celebrating an agreement, forged out of hard, all-night (he hadn't changed his clothes since the day before), international bargaining and the initially cacophonous strumming of guitars, resplendently decked in their respective national colours. But for this signal to have its intended political impact, the orchestrated version would have had to be claimed to be playing at least roughly in tune with the planet. The scientific signal would have to be pointing at somethings as they are and, more importantly, at the culprits who/that have made things as they are, as they are; this is what I am calling *forensics*. So, too, Coleridge and Stevens, though privileging different voices, both retain the idea of 'things as they are', undisturbed by subjectivity.

Parry himself seemed to be producing a discourse that would mediate that of science, something closer—despite the graphs and tables—to everyday language, what we might (and I shall) call the *public domain* of science. He didn't get it quite right. As I recall from the BBC World live broadcast, for example, at one point Parry was unsure whether it was a build up of 'carbonic' or 'carbolic' acid that was being generated by the dissolving of CO_2 in the sea. Nevertheless, Parry's *public domain* discourse would seem to be a necessary mediation, because natural scientists often seem to generate rather unnatural ways of talking about the world. This is how at least one group of astrophysicists look at the night sky:

Photometric redshifts can be routinely obtained to accuracies of better than 0.1 in Deltaz/(1 +z). The issue of dust extinction, however, is one that has still not been well quantified. In this paper the success of two template-fitting photometric redshift codes (IMPZ and HYPERZ) at reliably returning A_V in addition to redshift is explored. New data on the 2nd Canadian Network for Observational Cosmology (CNOC2) spectroscopic sample of $0.2 < z < 0.7$ galaxies are presented. These data allow us to estimate A_V values from the observed Balmer decrements. We also investigate whether the empirical value of gamma= 0.44, the ratio between gas- and star-derived extinction, as determined by Calzetti, is necessarily the best value for this sample. (Babbedge et al, 2005; p. 1 of pdf version)

This looks more like (what I shall call) the *esoteric domain* of science and will certainly need mediating for many people. As I understand the situation, though, there is definitely a forensics of the universe going on here.[1]

Let's look at it like this. For the most part, we routinely and earnestly engage in everyday discourse about our surroundings. This everyday discourse is generally quite loosely defined, quite context dependent—I will develop this general idea in Chapter 4. The physicists and climate change scientists have effected a sceptical separation from this everyday discourse via generally quite extended apprenticeships into their respective *esoteric domains*. These domains, in the case of the natural sciences, may be presumed to be strongly *institutionalised*. That is to say, we might expect there to be a high degree of regularity in their deployment within any given field; Bernstein (2000) describes these fields as characterised by 'hierarchical knowledge structures', though I will also engage with this in Chapter 4. Further, these domains also incorporate instruments or, as Latour and Woolgar (1979) famously described them, 'inscription devices', that mediate (or construct) scientists' perceptions of the world. This strong *internal* institutionalisation of the scientific esoteric domain discourse and the claimed (as it has to be) reliability of its inscription devices entail the claim of the elimination of subjectivity—a claim that, as I shall argue in Chapter 6, is also strongly made in the area of school science. These are instruments that, played by any competent musician, will each always produce consistent melodies. This, perhaps, is the music of Coleriedge: each Aeolian lute acting selectively, but not otherwise transformatively on nature.

Many areas of the natural sciences are imbricated into diverse state and commercial institutions and practices within society including, for example, health care, engineering, the military and funding by the state and other sources for 'big science' is clearly very substantial compared with, for example, funding for the social sciences and the humanities. It is also worth pointing out that the mediated, public domains of the natural sciences, whilst far more weakly institutionalised, are nevertheless very widely and frequently elaborated in the mass media as well as on the school curriculum. This constitutes a strong *external* institutionalisation of the natural sciences.

So, the strong internal and external institutionalisation of the esoteric and public domains of the natural sciences has effected the making of our most secure truths about the world. There is a sense in which we cannot think beyond them. Yet at the heart of our most secure truths, there are fractures. We need alternative (and, at least in part, contradictory) discourses of physics for the everyday (classical mechanics and electromagnetic theory), the very small (quantum mechanics) and the very large (general relativity) (see Penrose, 1997). It seems also to be the case that many medical procedures performed routinely are not actually backed up by scientific 'knowledge' and are quite often ineffective (see The *Guardian*, 7th April 2007). The position that asserts that that which is true is that which is socially

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[1] See Dowling (2006, cc. 4 & 5) for my interpretation (not authoritative) of what's going on here and in this article.

institutionalised as true is often referred to as 'social realism'. But truths are always open to challenge on the basis of at least three strategies. Firstly, shifting between discourses, as my reference to the alternative discourses of physics illustrates, but more radical redescriptions would result from a move from physics to, say, sociology. Secondly, shifting between levels of analysis. We can also use the physics example here: quantum mechanics might be interpreted as a consistent discourse; physics inconsistent. The third shift entails demanding elaboration: there would probably be general agreement on the statement that defenestration from a twenty-first floor apartment is likely to prove fatal, but there are innumerable ways to re-textualise this 'truth'.

What holds together these various and often contradictory truths is generally some kind of claim to the fixity of 'things as they [really] are'; at the centre of all forms of realism is a longing for the unobservable. Let me illustrate with a description of the game, 'Mastermind', an online version of which can be found at www.irt.org/games/js/mind/.[2] The original game consisted of a rectangular plastic board with rows of (four or five—I can't remember) holes that would accommodate coloured pegs. The row at one end of the board was or could be concealed by a mask. Behind the mask, the first player would arrange a row of coloured pegs. The second player—would guess the arrangement and fill the first row at the opposite end of the board with an arrangement of coloured pegs. The first player would 'mark' the second player's guess by indicating how many pegs in the second player's arrangement were both of the correct colour and in the correct position and how many were of the correct colour but in the wrong position. On the basis of the new data from the first player, the second player would fill their second row, which would, in turn, be marked by the first player, and so the game would proceed until either the second player made a correct guess, in which case they would win, or the board filled up, in which case the first player would win: a clear example of a forensic challenge.

I used to play a variation on this game with my friend, Parin Bahl. In our version, one of us would think of a five-letter word and the other would try to 'guess' the word via a similar process of trial and response. Following the first move, which would, generally, be a simple guess, the second player would deploy one of two strategies; I'll illustrate with an example. Suppose Parin had thought of the word, QUARK and suppose that my first guess was MESON. Parin would inform me that there were no correct letters correctly placed, nor, indeed, were there any correct letters, wherever placed. At this point, I would know that none of the letters in my initial guess were correct. I could formulate my second move by producing a 'theory', which is to say, a guess that is consistent with the totality of the information that I had. An example of a theory would be LIGHT, though this

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would yield another null response. If the rule is that a theory has to be a recognisable word, then my options have become considerably limited, because I have only A, U and Y left as possibly vowels. I could, of course, continue to formulate theories. But I might decide to try an 'experiment'. The word, GLEAM, for example, has four letters that I know are incorrect, so trying this word will tell me if the letter A is in the target word. As it happens, QUALM would be a better experimental word, but I know this only because I'm playing against myself! In any event, the second player would continue to offer a sequence of theories and experiments until the target word was identified; sometimes the sequence of theories and experiments was quite extended, especially if the first player had chosen a word that was unknown to the second. In principle, though, the target word will always be found.

Suppose that we extend the game. Suppose that we imagine this entire book as the target 'word'. Now, I think I'll assume that this book accommodates all twenty-six letters of the English alphabet, but do we tell the second player how many words it contains? Whether or not we do, playing the game has now become something resembling another game involving a sufficiently large number of monkeys, each with a typewriter and having a sufficiently extended period of time to produce the works of Shakespeare (I'm not comparing my writing with that of Shakespeare in any other sense). But the book also contains some Japanese and Greek and mathematical expressions and various forms of emphasis and punctuation, diagrams, images, and so forth. In principle, these might all be coded and digitally rendered (as, presumably, my wordprocessor does for me) and the whole produced as a single binary number. I'm now wondering what number it would be! I'm also wondering whether it would be easier or more difficult to play.

The presentation of statistical information about the book (how many words, how many pages and so forth) or its coding as a binary number are forms of textualising that are, in some sense, equivalent to the quantification of the heavens by Babbedge *et al*. Attempts to 'guess' the book in its original form would be more 'qualitative' in nature. In all cases, however, the game is constructed as a closed system that also incorporates *the* truth and a mode of interrogating *the* truth that will yield perfectly valid, forensic information about it. The theories and experiments produced by Babbedge *et al* operate within a system that is open because it cannot include the pre-coded universe, which is unobserved/unobservable.

Nevertheless, the proponents of realist approaches find it necessary to attempt in some way to capture the unobserved/unobservable. A particularly sophisticated realist approach and one currently much in vogue is *critical realism*, the principal exponent of which is Roy Bhaskar (1997, 1998). Bhaskar distinguishes between the ontological *intransitive dimension* of knowledge and the epistemological *transitive dimension*. The conflation of these two dimensions is what Bhaskar refers to as the *epistemological fallacy* and is tantamount to taking what we 'know' (transitive) to be what really is (intransitive). It is the separation of these dimensions that puts the 'critical' in critical realism: we must always maintain a degree of scepticism about what we 'know', however secure it may seem to be in

terms of making predictions. Indeed, for Bhaskar, prediction itself is problematic, even in the natural sciences. Bhaskar's reality consists of three levels. The 'real' consists in 'structures' and 'mechanisms' that give rise to 'events' in the natural world and 'relations' that give rise to 'behaviours' in the social world. Events and behaviours constitute the 'actual' and produce our 'experiences' in the 'empirical'. Here, it is important to note that the events of the 'actual' are produced whether or not they are experienced, so the answer to old question of whether a sound is made by a tree falling in the forest when no one is around to hear it is, 'yes'. Bhaskar claims that the real world is generally 'open', so that regularities in events and behaviours are not generally produced; they may occur locally in the natural world, but not at all in the social world. Thus reliance on the constant conjunction' of events and the inference of laws on the basis of regularity is inappropriate and prediction, certainly in the social world, is not possible.

Now this last point may sound a bit strange and susceptible to the same kind of jibe that Alan Sokal was making about jumping from his twenty-first floor window (see Chapter 8). After all, I can predict pretty reliably that a whole bunch of students will turn up at my institution on the first day of term. But, of course, this is focusing entirely on the transitive dimension in dealing with the knowable; it isn't getting anywhere in terms of what actually 'exists', the structures, mechanisms and relations of the 'real', to assume that it is, is to commit the 'epistemological fallacy'. This kind of criticism is also applied to quantitative forms of research. In general terms, quantification must presume qualitative regularity (the word count for this book treats all of the words as the same kind of entity) and so, in a sense, presumes what the 'real' does not generate. Social constructionist approaches that understand reality to be constructed socially also, quite clearly, commit the epistemological fallacy in failing to recognise the need to investigate the underlying structures, mechanisms and relations. 'Triangulation' is an approach that is consistent with critical realism. Here, different strategies are combined in order to reveal some of the limitations of each.

This is very clever stuff. However, to insist on its relevance to my project would seem to entail a tacit claim that I am doing philosophy and I want to insist that I am not. Not everyone would agree; this is from an introduction to critical realism:

> A good part of the answer to the question 'why philosophy?' is that the alternative to philosophy is not *no* philosophy, but *bad* philosophy. The 'unphilosophical' person has an unconscious philosophy, which they apply in their practice—whether of science or politics or daily life. (Collier, 1994; p. 17)

Now I've had this kind of argument before with the exponents of 'ethnomathematics'. These are educationalists, such as Paulus Gerdes (1985,1988), who believe that anybody doing anything that can be described in mathematical terms—such as building a traditional African house—is actually doing mathematics and that, furthermore, revealing this to them is an act of emancipation. This is what Gerdes suggests:

'Had Pythagoras not ... *we* would have discovered it'. The debate starts. 'Could our ancestors have discovered the "Theorem of Pythagoras"?' 'Did they?' ... 'Why don't we know it?' ... 'Slavery, colonialism ...'. By 'defrosting frozen mathematical thinking' one stimulates a reflection on the impact of colonialism, on the historical and political dimensions of mathematics (education). (Gerdes, 1988; p. 152)

And here is the mechanism of emancipation:

The artisan who imitates a known production technique is—generally—not doing mathematics. But the artisan(s) who discovered the techniques, *did* mathematics, *developed* mathematics, was (were) thinking mathematically. (Gerdes, 1985; p. 12)

As I argued in Dowling (1998), Gerdes is constructing the practices of those he observes as the public domain of his gaze that is a mixture of European school mathematics (Pythagoras), Fordist production techniques (production is imitation), and European historiography (industrial practices were invented by 'great men'). He is also prescribing his own version of conscientisation therapy. What he is not doing is allowing the African cultures that he surveys to stand as values in and of themselves and with their own voice: they are not doing mathematics, they are making their own culture. Similarly, I am not doing philosophy just because what I do can be described in philosophical terms. To claim otherwise is to engage in what I refer to as *mythologizing*: treating the public domain as if it were 'real'. But it's not, its recontextualised practices. I am not doing what Roy Bhaskar seems to think that I ought to be doing. I am quite unashamedly operating in his epistemological 'transitive domain'. However, I deny the charge of epistemological fallacy on the grounds that I am not looking for or claiming to have found truths or real mechanisms, structures and relations. Rather, I am attempting to build a culture and this entails producing some kind of regularity in the same ways, in some respects, as the builders and designers of other artefacts: I am a theory/research engineer, providing an organisational language that potentially allows people to see the world in new ways that may be of interest or may be productive for them. I use the tools of sociology and methodology because they are the ones I have to hand and I have developed a small fluency with them—they are a part of my language, philosophy, in the sense of its problematics that span millennia, is not.

My position is explicitly anti-realist, but not in the naive sense ridiculed by Sokal and other realist critics of postmodernism that are discussed in Chapter 8. Indeed, I strongly suspect that earnest adherence to naïve realism is an empty set—at least within the academic field. I don't deny the existence of Bhaskar's ontological, intransitive domain, I simply do not feel that faith in it has any clear implications for what I do. I suppose I lack the conviction that science or society is in any clear and general sense improving, though local 'improvements' (and the opposite) are palpable. I guess I can just work on the arrangement of my little corner of the world without the need to be sure that I am doing something of

ineffable, but certain value. Walking past the cemetery near where I (sometimes) live in Yokohama, my friend noted that her brother had stated quite explicitly that he did not want any of the Japanese, Shinto-Buddhist pomp and ritual performed after his own death, but rather wished his ashes to be simply scattered, Hindu-style (he didn't mention running water, so this didn't seem to signal an actual conversion). My comment was to the effect that death rituals were for the living, not for the dead and it was not really his place to dictate the preferences of others. Unless, that is, he had an ontological commitment in some kind of afterlife or other after-death mechanisms, structures or relations that would justify his intrusion into the grief of those who survived him. Personally, I find the cemetery a rather attractive, peaceful place to get away from the traffic and to remember. If all I'm building is a cemetery, that's fine by me—I guess this bears some similarities with rearranging the deck chairs on the Titanic.

So, anti-realism does not necessarily entail a direct challenge to either the *esoteric* or *public* domains of natural science knowledge. All that it must do is reject the need to dwell in the mythical land of the unobservable. It is my contention that, at best, the insistence on the existence of a state of 'things as they are' is a political or marketing strategy. Indeed, Robert Alan Jones (1999) describes Émile Durkheim's 'social realism' as precisely a political strategy. It provides an alibi for errors and contradictions, by positing an ideal state of perfect knowledge, unobtainable, but something that we are all trying to close in on. In respect of the natural sciences, the success of their predictions is often understood as evidence of precisely this, whereas their failures to predict are generally allowed to fade (or not published in the first place).

Unfortunately, realist pronouncements can have more damaging effects, limiting reflective thinking and debate. The constitution of the dual realms of knowledge and reality allows the bigoted debater to hop between two stools in an argument, thus, in response to a sociological analysis,

"But that's not the way the world works."

"How do you know?"

"Science tells us."

An alternative example would be the attempt to hold on simultaneously to both modes of legitimation of anthropological commentaries that are offered by Clifford Geertz (1988): 'being there' and 'being here'. 'Being there' legitimates statements on the grounds that the anthropologist has lived in the setting about which they write; 'being here' grounds legitimation on the anthropologist's apprenticeship into the anthropological discourse discourse of the university—'here'. As Geertz elegantly points out, the nature of 'having been there' is of course constituted by having 'been here' (see Chapter 8). You cannot have your cake and eat it, but the attempt is precisely what I want to avoid. I find it pervades so much of the discourses of the social sciences and educational studies, in particular. We might speculate that it is precisely the comparatively weak internal and external institutionalisation of these discourses that allows their infection by the virus of everyday naïve realism and, perhaps, contributes to their failure to develop strong institutionalisation.

So, what I mean by demonstrating that we're sinking without hope of salvation is that we must not depend on a real real—'things as they are'—as a lifebelt (or if we encounter it as an iceberg, then were really sinking). Having established this—to my own satisfaction, at least—I can concentrate on rearranging deck chairs, or on my performance on my blue guitar. As Chung (op. cit.) has pointed out, Stevens aims at something beyond mere blueness. This extension is precisely Stevens/my subjectivity. There is more than one option. I might work to produce fiction. I might stay close to the weakly institutionalised, analogue discourses of the everyday and play with the empirical. Alternatively, I might engage in intertextual dialogue with other fiction and treat the empirical with a degree of abandon—some social theory seems to operate like this. I will not choose either of these options, though I share with both fictive forms the initial assertion that my (any) work is only a (re)textualising of perceptions: this is precisely the critical dimension of my discourse. I share with the first option the concern to engage with the empirical. I share with the second the concern to dialogue with, shall we say, the theoretical. I am concerned to establish a degree of pedagogic potential in my discourse and so I need to make its central principles explicit—as explicit as possible. This might ease, but certainly not ensure, its institutionalisation beyond the academic activities of myself and my students.

The pedagogy may not be easy or quick, however. Here is an extract from another Stevens poem, my use of which also follows Chung:

One must have a mind of winter
To regard the frost and the boughs
Of the pine-trees crusted with snow;

And have been cold a long time
To behold the junipers shagged with ice,
The spruces rough in the distant glitter

Of the January sun; and not to think
Of any misery in the sound of the wind,
In the sound of a few leaves,

Which is the sound of the land
Full of the same wind
That is blowing in the same bare place

For the listener, who listens in the snow,
And, nothing himself, beholds
Nothing that is not there and the nothing that is.

(Stevens, 19XX; p. XX)

The snow man acquires the gaze of winter after a long time in the cold, shedding the everyday apparatus that shivers against it. My metaphor, here, is the winter as my own discourse, that—simple and inadequately developed as it may seem—has been a long time in the making and has arisen out of dialogue and discussion with interlocutors and with texts and cultural settings. The result is neither closed nor complete, of course, but, more particularly, it is not realisable as a linear programme of development. All of this is the case with any culture capable of more than trivial descriptions and the failure to recognise these features of cultural systems permits mythologisings such as the school curriculum, with its steadily developmental structure.

So this book has not been arranged as a developmental curriculum, other than in the inclusion of this introduction, a concluding chapter (Chapter 9), that I hope is more than a summary, and the positioning of Chapter 2 before Chapters 3 to 8. Each chapter is its own departure—Chapters 2 and 3 depart from the same point, the former to the introduction of the idea of a sociological analysis of text, the latter to the presentation of aspects of and commentaries produced by my organisational language. Each chapter has arisen out of and is constructed as a conversation with one or more key figures, ideologies, texts, or places. They were all written with the current project in mind, though most can be traced back to more local events and one—Chapter 6—has been published elsewhere in a slightly different form.

The central figure with whom I am dialoguing is Basil Bernstein, my former mentor. This dialogue has been ongoing since 1986 and is the point of departure for Chapter 4—in more ways than one, the central chapter in this book. The chapter incorporates its own history, so I'll not elaborate on this further here. The chapter is an attempt to mark out the key points of departure of my constructive description from Basil's forensics. The dialogue with Basil is extended in Chapter 8 in which Soh-young Chung and I engage with some recent (and some not so recent) work that has sought to develop Bernstein's sociology of knowledge. This chapter has its origins in my involvement in Soh-young's work in the sociology of literary studies and also in my own methodological work, in particular, in the production of a module for a Master of Research degree for the University of London external programme (Dowling, 2006).

Chapter 5 and also Chapter 2 include parts of an important (to me) dialogue with my colleague Gunther Kress. Here, I engage in some practical textual analysis in attempting to establish the points of departure of my constructive description and sociological organisational language, on the one hand, and Kress's linguistically motivated approach. Chapter 5 has its origins in a presentation that I gave at the European Systemic Functional Linguistics Conference and Workshop, held at King's College London in August 2005. Some of the text analysis in Chapter 2—including the analysis of a work by the Florentine, Cimabue—began with a workshop on text analysis that I ran as a part of the pre-conference proceedings at the Southern African Association for Research in Mathematics and Science Education conference in Cape Town in January 1995, I used it again in a presentation at the University of Lisbon in 1999.

Chapter 6 began as, first a seminar that I ran as a part of a masters programme in 1988 and then as a chapter in an edited collection (Dowling, 1991). Here, it constituted a dialogue with the discourse of technological determinism—I would say a dialogue with the ideology of technological determinism, but I am summoned by the utterance by Deleuze and Guattari, 'There is no ideology and never has been' (1987; p. 4)—this discourse is distinctly forensic in nature. In Chapter 6 I have picked up this dialogue and developed it into a critique of critique (I suppose). This chapter, in a slightly different form, is also an invited contribution to an international collection on mathematics and science education (Atweh *et al*, 2007).

Chapters 2, 3 and 7 and also Appendices I and II dialogue, in different ways, with places and cultures. I have begun both Chapter 2 and Chapter 3 with the same anecdote that stands as a dialogue with India, though the chapters themselves move swiftly away from their starting points. Chapters 2 and 3 were both initiated as presentations that I gave at Yonsei and Sogang Universities in Seoul in the autumn of 2003. Chapter 7 is a more substantial dialogue with the Western Cape region of South Africa. The chapter is an original presentation and re-analysis of an empirical study carried out by Andrew Brown and myself in the mid-nineteen-nineties. The two appendices are of a different nature from the other chapters, perhaps more in the public domain. They stand as representative of an ongoing dialogue with Japanese culture. As is the case with all of these chapters dialoguing with places and cultures, they are not constructed, primarily, as commentaries on these places and cultures, but rather the products of transactions between a method and cultural texts that serve as much to introduce and develop the method as to relay their own tales.

Chapters 3 to 8 all include relational spaces that establish various strategic modes. For example, Chapter 3 introduces a fourfold strategic space constituting a modality of interaction and Chapters 3, 6 and 8 all deploy a space constituting a modality of authority action. All of these relational spaces have emerged out of the constructive description of empirical texts (though not necessarily the texts introduced in this book). Chapter 4 includes my schema for constructive description itself that was first introduced in Dowling (1998). The particular form that the constructive description takes is sociological and by this I mean that I am understanding the sociocultural terrain to consist in the formation, maintenance and destabilising of alliances and oppositions through strategic, autopoietic action. It is the transaction of this foundational proposition with diverse empirical texts that generates the strategic spaces that I shall introduce and their commentaries on the empirical texts.

This chapter and Chapter 9—not begun at the time of writing this—bookend the main chapters in the book and, are intended to mediate them in respect of the central line of argument of the book as a whole. I shall not anticipate Chapter 9 here. I began the present chapter with a snatch of a(n ongoing) conversation between myself and Jeff Vass about audiences and the deck chairs and orchestral ensemble on the Titanic, pointing out that I don't like the music of the latter. That music is the music of forensics, of dualisms. Michael Parry's presentation of the

IPCC report on climate change opens a dualism in respect of the origin of the signals that it identifies: is this the voice of science, or is it the voice of the planet. Similarly, are Babbedge *et al* presenting the voice of science or the voice of the universe. In a discourse of the social sciences, Clifford Geertz articulates the same category of forensic dualism: is anthropology legitimated by being here (in the university) or by being there (in the field)? In the arts, Coleridge longs for the voice of nature, Stevens for the voice of the poet. We seem quite easily to be able to recognise the dualisms of the social sciences and the humanities as themselves constituting legitimate fields of contestation. The internal institutionalisation of these discourses tends, of course, to be quite weak, having nowhere a strong grip on language. Their external institutionalisation—their public domains—are even less strongly instititutionalised; how many general readers have any clear idea of the commentaries generated by sociology or by educational studies? The natural sciences are different. Here, we might speculate that the strong external institutionalisation of their discourses supports their already strong internal institutionalisation (though perhaps this was less the case in the days of the amateur man [sic] of science).

The strong dual institutionalisation of the natural sciences seems to conceal its own dualism, so that science and nature seem naturally to be singing from the same hymn sheet. But the ventriloquising of the voice of the natural world by the voices of human discourse—what Hayles refers to as 'the platonic backhand' (see Chapter 2) and what I refer to as forensics—always creates a doubled space where finding an alibi for incomplete theoretical development is always possible. I have presented the game, Mastermind, as an ideal type for scientific investigation. Here, experiment and theory are meaningful because the game itself incorporates its own solutions as, in a sense, does science, which, in a sense, makes the world that it investigates. This, incidentally, is not a cynical voice—quite the contrary.

I am cynical, though, about the forensic postulation of 'things as they are'. But this cynicism is not a challenge to philosophies such as critical realism. It is a challenge to the hegemony of philosophy. I am no more doing or dependent upon philosophy than is the engineer, the architect, the watchmaker or the novelist. I am doing sociology and, from time to time, think of myself as a theory engineer—a maker of theoretical machines. I start with the assertion that the sociocultural consists in the autopoietic formation, maintenance and destabilising of alliances and oppositions as a theoretical installation. I proceed via the transaction of this installation with culture, text and knowledge as pointers to empirical settings. The outcome is the generation of commentaries of these settings—constructive descriptions—and, at the same time, the development of an organisational language, currently referred to as social activity theory. Philosophy may participate in dialogue in a kind of a pastiche arrangement, but may not hegemonise my discourse (see Chapter 3).

I am attempting to address a very diverse audience, including those for whom the central message of this chapter is not news, but also the forensic sociologists and educationalists that Jeff Vass thinks I may as well ignore. I am also addressing the antitheorists (a more difficult audience, perhaps). At a recent (successful) viva

of one of my doctoral students, one of the examiners asked what the point might be of all this technical language, why couldn't the thesis be written in a language that would be intelligible in the settings investigated. But, of course, if the thesis had been in any meaningful sense intelligible in these settings, then it could not have stood meaningfully within an academic discourse—it should have failed! Some antitheorists operate a different version of forensics, a monism rather than a dualism. For them, 'things as they are' are transparently available to us. These are the precisely the critics of Stevens's guitarist. Other antitheorists, perhaps, favour fiction and 'playing with the empirical'. I'll address them anyway.

In another conversation with Jeff Vass (as it happens, the other examiner of the very same thesis) I suggested that it occurred to me that theory is very often regarded much as are the biscuits passed round on a plate at committee meetings: delicious or not, it is/they are no part of the real business of the university. Perhaps I'll ignore those who think in this way.

As for the others, part of the message that I want to address to you concerns the importance of focussing on the matter at hand—constructive description, commentary, and organisational language—and avoiding mythical transport to the discourse of the real (whether or not you are a believer). The other part of the message consists of a marketing strategy. I am presenting you with a technology—an organisational language—and some illustrations of how it might be deployed and developed and also of what it is capable of producing in transaction with the empirical (which commentaries I hope are and I am presenting as new perspectives on a whole range of settings). In presenting all of this I am, of necessity, having to adopt a pedagogic tenor, but, at the end of the day, the evaluation is yours: it's really a matter of whether or not you find tuneful my particular style of blues guitar.