NOTES AND DISCUSSION

A response to Tom Barney

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I appreciate the time Tom Barney has spent reading and writing about my work: his review of my book *Language and Literature* 2 (1) and now his response to my article *Language and Literature* 3 (2). Barney is sympathetic with the general thrust of my work, but he objects to many of my suggestions.

Barney's biggest objection to my work derives from his philosophical orientation. Barney is a behaviourist. He assumes that the better part of rhythmic experience derives transparently from (1) convention or (2) the quality of an external stimulus. Therefore he rejects out-of-hand my 'natural', 'top-down', 'cognitive' approach to rhythmic experience. To respond with a 'strong' beat, Barney claims, we *must* be stimulated with a 'strong' syllable. To experience rhythmic hierarchies, we *must* be exposed to language with similar hierarchical ordering. To claim anything else is 'simply ridiculous.'

The relative contribution of nature and nurture to human experience is complex. Our rhythmic capabilities derive from both sources. Therefore Barney's out-of-hand dismissal of my 'cognitive' approach to rhythm is philosophically naive. There is nothing 'ridiculous' about claiming that we have certain innate rhythmical capabilities; that rhythms, as we experience them, have certain canonical shapes; and that this rhythmic experience diverges widely from the stimuli that elicits it. The value of this suggestion comes from its explanatory power, its ability to account for our rhythmic intuitions; and if it is to be rejected, it must be rejected on these grounds, not by philosophical dogmatism parading as argument. In both my article and my book, I document extensively the explanatory power of this cognitive approach to verse rhythm. There has never been a comparably organised and explanatory account of these matters. If Barney has in hand such a rival account, then I will be very excited to hear it. But at present he gives no evidence of this whatsoever (i.e. he scans no poems; he represents no rhythmic intuitions).

Barney's next biggest objection derives from his (different) aims. Barney's major concerns are (1) to describe the physical/linguistic structure of individual performances of the text and (2) to validate and stabilise rhythmic interpretation in terms of those physical/linguistic descriptions. My major concern is to provide detailed rhythmic representations that can be used productively in verse criticism, history, and theory.

Barney is right that I do not establish the phonetic sources of my intonational intuitions, but this lack has little bearing on my aims or claims. My work on verse rhythm is based on my rhythmic intuitions, not the phonetic sources of those intuitions, and it is evident that many rhythmic intuitions are only obliquely represented in the phonetic record (e.g. the preference for strong-beat early in metre and all of prolongation).

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Barney's claim that my intonational segmentations are 'dubious' because not 'certain' both misunderstands my claims and extends his (promised!) results beyond their scope. The only way he can refute my intonational claims is to show that they are either impossible or unpreferred. It is no refutation to point out that they could be otherwise. My theory of grouping does not dictate/prescribe hearings; it motivates preferences. I recognise repeatedly that rhythmic hearing can vary from time to time (in the experience of one person), from person to person (in one interpretational community), and from interpretational community to interpretational community (e.g. between experienced readers of poetry and the poetically naive).

The logic of Barney's argument here is questionable. How can I experience something, prefer it, demonstrate why I prefer it, and use it to explain many things about other aspects of my experience, when, in the end, that something is impossible? Traditional approaches to verse experience (e.g. foot-substitution prosody) have been inadequate because they have provided weak, partial, and contradictory accounts of verse experience (Cureton 1992: 11–117), not because they have provided strong, complete, and consistent accounts that are (really) impossible. If Barney is to maintain this second objection, he would also need to explain how others both during and since the writing of my book have been able to experience what I have claimed. There is already evidence that others experience just what I do and, in doing so, can use my theory as a powerfully productive critical tool.

Barney also objects to my representation of metre. I claim that our metrical response to language parallels our metrical response to music – i.e. that linguistic metres are also 'natural', unsegmented (i.e. un-'footed'), steeply hierarchical, afterbeating, oppositionally related to grouping, obliquely related to the phenomenal surface of the text, and so forth. Barney rejects this view without argument, presumably for a more traditional theory, in which linguistic metres are conventional, flat, footed, upbeating phrasal norms embodied fairly directly in phonetic detail.

I am sympathetic with this objection. My ability to fully experience and analyse metre also came late and with great difficulty. Within language study, there is an enormously long and pervasive theoretical tradition that regards metre as a repetitively shaped and segmented *voicing*, that is, that conflates metre and phrasing rather than bringing forward their 'natural' (and principled) differences. This theoretical tradition (together with many other matters, e.g. our historical distance from a poetry in which metre is rhythmically 'dominant') consistently distracts those who work on these issues from even accessing the basic perceptions needed to develop a workable theory of metre for language.

The fact that has been missed by the theoretical tradition is that metre is basically *gestural/tactile* rather than *vocal/aural*. Its locus is in the body, not the voice/ear. This locus in the body gives metre a distinct cognitive source and expressive range. By trying to approach verse through the phonetic record, Barney distances himself from these bodily reactions.

Once we distinguish metre from vocal figuration (i.e. from phrasing), it is not at all unreasonable to claim that metrical onsets to even vocally weak phrases are preferably experienced as large rhythmic gestures, which then oscillate in a decaying pattern until exhaustion, reinvigoration, or termination. Given this perspective, it is also not unreasonable to suggest that this bodily gesturing is used to organise larger metrical spans: the hemistich, the line, the stanza, the poetic form, and so forth. As I have recently outlined in some detail (Cureton manuscript, forthcoming a, and forthcoming b), this account of 'hypermetrical' gesturing can explain a range of previously unexplained facts about both the standard inventory of our traditional verse forms and the interactions between these verse forms and particular phrasal gestures. Most of my explicit thinking and writing on these issues has been done just recently and has not yet been published, but I illustrate and discuss many of these metrical effects in the 150 pages of explicit analysis I present in chapter 5 of my book (Cureton 1992: 277–422).

As with his other objections, Barney's response to these metrical claims are weak: undemonstrated, incoherent, or just wrong. He spends most of his time claiming (1) that hypermetrical beating in music is just conventional and (2) that language, therefore, cannot be hypermetrical (because it lacks this convention). But he has no evidence for the first claim and therefore no logical justification for the second (which depends on the first).

Barney then goes on to reiterate Lerdahl and Jackendoff's claim that metre is relatively local in structure compared to phrasing, certainly more local than I claim for verse, even though Lerdahl and Jackendoff's metrical responses in their standard examples, such as Mozart's G minor symphony, have almost exactly the number of levels I claim for verse: six (Lerdahl and Jackendoff 1983: 250–77). Recently Jonathan Kramer (1988: 81–136) has also argued that Lerdahl and Jackendoff's hypermetrical gesturing is far too weak. Kramer claims that he responds to the G minor symphony with *nine* levels of metrical beating, a hypermetrical span than covers almost 50 measures. In his other analyses, Kramer claims metrical gesturing in response to music of eleven levels covering spans of almost 200 (!) measures (for example, see Kramer's analysis of his metrical response to the first movement of Beethoven's String Quartet in F Major, pp. 123–36). Given that phrasal organisation can embrace an entire text/piece, such extended metrical gesturing is still relatively 'local' compared to phrasal hearing. But contrary to Barney's unsupported claims and innuendos, the extent of this gesturing can be considerable both for music and language, certainly long enough to embrace the seven levels I claim for the sonnet. The extent of Kramer's metrical response and the differences between his response and Lerdahl and Jackendoff's response also argue strongly against any view of musical metre as (1) coventional or (2) derivable transparently from the phonetic record.

Barney also objects to my linguistic sources, claiming that my unquestioning use of the results of the metrical phonologists overlooks the weaknesses in those

results. This objection is also unfounded. As I specify repeatedly in my book, I am not adopting without alteration the view of the metrical phonologists (Cureton 1992: 256–57). The hierarchy that I posit for grouping at the intonational level and below differs in various ways from their prosodic hierarchy (for example, I omit the level 'word'). Throughout my book, I also point out repeatedly how my interest in rhythm differs from the metrical phonologist's interest in phonological organisation in language and versification in poetry. The levels in my hierarchy are derived from my intuitions of rhythmic shaping, not from evidence of phonological organisation. In fact, I explicitly suggest (1992: 257) that it is more likely that constraints on grouping condition the prosodic hierarchy than the other way around. How am I trusting linguists unquestioningly if (1) no linguists have ever constructed a phrasal hierarchy exactly as I do and (2) no linguists have ever suggested a system of preference rules for phrasal hearing that even remotely resembles the system of rules I suggest in my book? I quote:

> Given the preferential nature of grouping and the diverse functions of prosodic organization in language, this does not claim that the prosodic hierarchy is a rhythmic form. It just claims that the rhythmic effects of the prosodic hierarchy are part of what constrains [rhythm's] structural organisation.
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> (Cureton 1992: 256)

Finally, Barney objects to the explanatory power of my theory, claiming that the theory cannot mediate between the text and the phrasal preferences I report (e.g. in response to the example I use in my article, the second quatrain of Shakespeare's Sonnet 29). This objection is also unjustified. In my article, it was not my aim to motivate my claims about grouping preference. The focus of the article was on rhythmic representation (i.e. a definition of rhythm). These preferential motivations for my hearings can easily be given, however.

I prefer to hear 'more rich' as stronger than 'in hope' because 'in hope' is old information (GPR1 Information): the first quatrian is about the sin of despair – lost hope. 'More rich' is also strong informationally; it indicates comparative degree (GPR1 Information). This hearing also produces an amphibrachic tone unit that is rhythmically parallel (GPR16 Unity) (1) to the tone unit in the same position in line 6 ('like him with friends possessed') and (2) to the tone unit in the first position in line 8 ('With what I most enjoy'), a tone unit that also has a comparative ('most'), this time one that is unambiguously the peak of its tone unit because it contrasts minimally with another comparative ('least'). I also find that much of the rhythmic felicity in the quatrain as a whole (GPR17 Schemes) results from the play of the three waved amphibrachs at the tone unit level with the strong-final (and dramatically foregrounded) anapests ('this man's art / and that man's scope') at the phonological phrase level in line 7. The demotion of 'more' by the metre also foregrounds 'rich', delivering it after a formal change

(GPR5 Return). I agree with Barney that the various paired conjoins in the quatrain ('this man's art and that man's scope' etc.) are very balanced informationally. But I read these units with final peaks, first, because all rhythmic parallels to these units have this shape (GPR16 Unity) and, second, because an iambic shape is canonical form for a group and therefore is the default in these situations (GPR3 End-focus). Contrary to Barney's claims, such principled motivations for phrasal hearings are exactly what my theory of grouping makes possible.

I appreciate Barney's interest and energy and look forward to the publication of his own work. But at this point his own results are hypothetical, and his objections to mine unfounded.

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