

On Normativity, Meaninglessness, and the Centrality of the Self: A Reply to the Comments of Good and Brown

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We want to begin with an apology. The paper presented earlier in this edition of *Operant Subjectivity* was never really meant for publication. The first author didn't even intend to present it at the 18th ISSSS conference as eventually happened. This is not because we ever thought it unworthy of publication, nor that we now want to retract an iota of its content, it is just that we were worried that the *academically provocative* would be interpreted as the *socially divisive*. There is clearly a sense in which this has happened (the reader will note Brown's comments regarding the *aisles* of Reading in 1989). This is a shame, on the one hand because we only ever wished the paper to be *academically* provocative, and on the other hand because social divisions inevitably discourage the kind of open debate in which we are continually interested.

It may be useful, given that we have been accused of "not crossing the aisle," to provide a bit of social background which we hope will help to contextualise the emergence of the paper in its current form. The original version of this paper was included as chapter 3 of the first author's Ph.D. thesis. This remains, to our knowledge, the only Ph.D. that has ever been produced in the UK which is *entirely* Q methodology from cover to cover. Even the late Rex Stainton Rogers (who was, of course, hugely enthusiastic about Q) actually warned the first author of the dangers of following this path, particularly in the context of a British psychology department. Stephenson himself became weary of such contexts in 1947 and eventually chose to cross the metaphorical aisle. As Brown reminds, he did not return until 1989. This is sad. Still sadder is the extent to which Stephenson's name has subsequently disappeared from received *histories* of British psychology. The vast majority of undergraduate (and, indeed, postgraduate) students of

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psychology in Britain will not know Stephenson's name. We must hope that James Good's current efforts will begin to redress the balance in this sense.

It is also pleasing to note that the second author is responsible for reintroducing Q methodology to its spiritual home in the department of psychology at University College London. We take small steps forward. When Q methodology does make an appearance in British psychology, however, justification is almost always achieved via a linkage to discursive or social constructionist theories, rather than by reference to Stephenson's own work. Hence our use of the term *UK dialect*. We wonder why Brown sees this as "essentialist" terminology? There is certainly no paradox for any discursive psychologist in the employment of a term which the Oxford English dictionary defines as "the words and pronunciation that are used in a particular area and differ from what is regarded as standard in the language as a whole." If our language was standard, would he not have concluded that we *had* "crossed the aisle?" The point is that we do use different words to describe Q — distinct lines do exist in the concourse — and whilst we agree that other UK Q methodologists may not be "*chock-a-block*" with our position (and that there may be non-UK Q methodologists who are), the term *dialect* points only toward the sort of Q methodological language that is most familiar in *our area*.

This all means that it is quite possible (and arguably quite usual) to employ Q methodology in Britain without any detailed knowledge of Stephenson's own work. Again, this is a great shame for there is evidently much to be gained by reading Stephenson. The first author can testify to this, for he spent the best part of a year reading nothing but Stephenson during his doctoral studies, and when the vast majority of Stephenson's papers were finally gathered he took the time and trouble to read them again, this time in chronological order. The idea was to trace the progress of Stephenson's thought to his final works and to get a feel for his theoretical stance at that late stage in his life. This was a highly interesting period, during which one first had the feeling of having much in common with Stephenson, followed by a spell in which one became perplexed by a certain looseness and inconsistency in his terminology, and culminating in the distinct impression that *later* Stephenson's own take on Q methodology was indeed somewhat different to the view generally being espoused in the UK. Chapter 3 of the first author's Ph.D. thesis ultimately charted this process of engagement and highlighted the differences as perceived.

Things would hardly have progressed further if it were not for James Good's appointment as an examiner of the said thesis. Whilst Good certainly took issue with some of the points raised therein (as is evidenced by his introductory comments), his role as examiner led him to verify that Stephenson had indeed said all that had been attributed to him, and he was sufficiently taken by the chapter to encourage a presentation of its main

content at the 18th ISSSS conference at Durham, England in September of 2002. We are further encouraged and grateful for the positive way in which he has approached the paper here. We begin, therefore, by responding to some of his comments.

Good asserts that we are led to our conclusions regarding Stephenson's apparent reliance on a solipsistic inner world via a serious misunderstanding of the notion of schemata (as understood by Bartlett). Indeed, both critiques presented here argue (in their different ways) that whilst Stephenson may have said the things attributed to him, we have in some way failed to grasp the true *meaning* of what he is saying. We have not grasped the true meaning of the term *schemata*, for example, and hence we have incorrectly drawn conclusions about Stephenson's reliance on an individual and isolated mind. In fact, however, Stephenson led us to these conclusions by much more direct means. He simply tells us that this is what he is doing. Here are three examples, although the interested reader will find many more in Stephenson's later works. First, on a quantum theme:

If quantum theory had to be at issue, I argued, its concern had to be with a single mind, not with minds. (1986b, 532)

Second, with reference to the operancy of factors and their beginnings:

The operancy of factors corresponds to what history has called subconscious and unconscious mind. (1983, 226)

And finally, a clear statement as to the location of the operant structures which Q reveals:

The operant structures in Q point to lawfulness, notwithstanding their isolation in a person's mind. (1986b, 538)

Of course, and despite Brown's objections, we were at pains in the original paper to indicate that Stephenson *had* spent the majority of his career rejecting the conventional notions of concepts like *mind* and *consciousness*. Indeed, we directly cited his refusal (except on grounds of convenience) to divide the behavioural segment into *inners* and *outers* precisely in order to demonstrate this stand against dualism (although Brown for some reason seems to have interpreted this as an attack made for the purposes of our *own project*). Our contention, however, was that in his later *quantum papers* he had done much to disturb that vision. If Stephenson did not mean us to think of isolated minds in the above quotes then he was at least guilty of being very careless indeed, a number of times and in a number of different places. Stephenson repeatedly charges his critics with a "lack of care and explicitness" (a trend which Brown now continues), but we would argue that the failure of explicitness is repeatedly his own. We shall evidence this several times as we proceed. This should not be read as a criticism, however, for Stephenson's free thinking (and highly fluid) style is often joyous to read.

Yet we believe it ultimately disguises a number of theoretical inconsistencies which are worthy of attention and discussion. Part of the aim of the original paper was to begin that process.

For the moment, however, we must reiterate that our arguments about “isolated minds” do not rely on Stephenson’s use of the term *schemata*. Indeed, this quote was added to the paper as something of an afterthought, and only as a *further* demonstration of Stephenson’s apparent belief that the “confrontation” between person and cultural message systems was actually *based* on these schemata (which is again *what he says* in the original quote). Good is certainly right to point out that Bartlett’s use of the term carries *no mental representational* connotations (although we’re not completely convinced that it has *no mentalistic* overtones). For our purposes, however, it is enough to demonstrate that Stephenson sees schemata as being personally *owned* — “The proposal here”, he says, “is to look...at how the *individual* sees things” (1986: 52) — and that schemata are in his view “active systems which *determine* what the individual will perceive or react to” (Stephenson, 1986, 53, emphasis added). Hence, we followed Stephenson’s own words and arrived at an image of Q methodology in which individual minds perceive or react to Q statements on the basis of their personal schemata (“participants inject statements with their own understandings,” as Brown puts it), and further concluded that Stephenson viewed the outcome of this process (operant structures) as being *determined* by these schemata. We see no reason to change this view given the wealth of evidence that supports it.

Stephenson’s emphasis on “how the individual sees things” leads us neatly to Good’s second concern — namely, our apparent “unwillingness to acknowledge the central importance for Stephenson of self-reference and the centrality of the self.” It is true that our original paper does not deal with these issues to the extent that they deserve, although this was somewhat inevitable given the enormous amount of ground the paper attempts to cover. We do, however, clearly acknowledge that self-reference lies at the heart of Stephenson’s approach to Q methodology and that it is crucial to the Q methodological procedure in a more general sense.¹ On the other hand, we

¹ It should be noted, however, that Stephenson’s application of this term is quite inconsistent. On occasion he claims that the concourse (and hence a Q set) is constituted of ‘self-referent’ statements. The implication here is that a domain of self-referent statements can be qualitatively distinguished from other ‘classes’ of statements. Stephenson ordinarily argues that self-referent statements represent ‘statements of opinion,’ and he distinguishes these from ‘statements of fact.’ In this context, concourses, Q sets and Q Methodology concentrate attention on domains of opinion rather than domains of fact. Elsewhere, however, he claims that the concourse (and the Q set) is constituted of ‘self-referable’ statements. This change is subtle but significant. The concourse is now seen to be constituted of ‘statements’ (objects, smells, or indeed of anything that might ‘make a statement’ of any type), each of which is potentially self-referable and each of which can be *rendered* self-referent only via the Q sorting process. The domain of qualitatively distinct self-referent statements which seems central to the former approach is completely missing from the latter. In short, careful readers of Stephenson cannot simply cite the ‘historical record’ to resolve this inconsistency, but must instead make their own choice about the nature of ‘self-reference’ and its proper application in a Q Methodological context.

believe that Stephenson's tendency to view *individual* selves as central to Q (be that emphasis methodological or substantive) is precisely what causes the sort of conflicts we have highlighted above.² As a consequence, the paper briefly outlines our own views of self-reference, which indelibly tie such processes to simultaneous processes of cultural reference. In so doing, we deliberately *decentre* the notion of self and self-reference, ultimately seeing both as emergent properties of a *self-referent cultural system of communicability*. It seems superfluous to repeat these arguments here, although we should point out that they are not untypical of social constructionist or discursive approaches in British psychology (see Harré, 1998). Good is certainly right, however, to say that much further work is required if discursive notions of self are to be properly linked to Stephenson's ideas about self and self-reference.

Brown's commentary is evidently less appreciative. Much effort is expended to show that we have misrepresented Stephenson and that, in fact, we are doing and saying nothing different in the context of Q methodology. We assume that Stephenson considered Q statements to be non-normative, which apparently couldn't be further from his viewpoint. We assume that he regarded Q statements as meaningless when he explicitly took the opposite stand. We assume that Stephenson's position implies a solipsistic inner world, but he seemingly spent a lifetime opposing this position, and so on. The fault of misunderstanding is laid squarely at our door.

One of these charges has already been dealt with. We have offered additional evidence in the context of this reply which suggests that the last of these conclusions is highly contentious in the context of Stephenson's later work. We believe the first two conclusions are similarly questionable. Brown asserts that we have misunderstood Stephenson in relation to the normative/non-normative issue because he is (always?) using the term *normative* in a statistical, rather than a sociological, sense. When Stephenson

² Which need not mean that single-case studies are precluded, nor that factors which emerge from such studies cannot tell us something about the individual involved. Whilst Good sees the factors revealed by the 'Case of Martre' study as being indicative of 'natural segregations in a person's mind,' however, they are also understandable in socio-cultural terms: simply as possible ways in which *anybody* might 'relate to' and 'deal with' the difficult and changing circumstances confronting Martre. Martre's situation evidently precludes him from dealing with his circumstances in either of the 'adjusted' ways captured by factors A and D (and note that 'all might have been well' if he had been able to follow either of these 'normative' routes). Instead, he is forced to take up a hostile, confused and somewhat ambivalent relationship to the world around him (as defined by factors B and C). This deviation from the 'norm' is no doubt what leads us to view his behaviour as disturbed. Evidently, we can learn something about Martre (and his personal circumstances) from this single-case study. Single-cases studies are worth pursuing. Equally, however, we could conduct a larger study which invites multiple Q sorters to tell us how *they* might deal with Martre's situation. Since they represent culturally acceptable and 'adjusted' solutions to this type of problem, it is highly likely that factors A and D would then reappear as major factors in the new factor structure, whilst the relative 'unacceptability' of factors B and C may limit such expressions. Either way, the 'natural segregations in a person's mind' would likely reappear on a second level - as 'human-made segregations in a socio-cultural system.'

tells us that “no normative dimension” exists within the concourse or the statements of a Q set, therefore, as he does in the quote from the original paper (and elsewhere), Brown wants to argue that this is purely statistical statement. Yet a brief glance at Stephenson (1982, 239) will show that the quote we used is drawn from a discussion which mentions (amongst other sociologically related ideas) “natural classifications,” “culture,” and the fact that concourses are “*common knowledge*.” If he has made a purely statistical statement in this context, therefore, he has done so without warning and without qualification, and immediately concludes his single-sentence statistical discussion by returning to the subject of language as an aspect of culture.

If Brown is correct, therefore, Stephenson is once again guilty of a remarkable *lack of care and explicitness*. A simple qualification of the type: “In statistical terms, no statement has a normative dimension,” would have cleared up this issue (or at least left the sociological possibilities open), but no such qualification is made. Nor have we been able to locate one *anywhere* in Stephenson’s work. Brown does subsequently present an early quote from Stephenson which makes mention of a social milieu (and its influence) and this is put forward as evidence that Stephenson *did* recognise a normative (sociological) dimension in the concourse. But *norms* or the concept of a normative dimension are never mentioned in this quote. Indeed, were they to be mentioned we would surely have to assume that such comments were of a purely statistical nature? Again, we were quite clear in the original paper that Stephenson *did* acknowledge some sort of inherent structure in the concourse. Where *normative dimensions* are mentioned directly in the context of Stephenson’s work, however, we are told that *no* normative dimension exists within the concourse or the Q set, and given the lack of contrary evidence or qualification the careful reader has to assume that “no” means *no*. No normative dimension exists in the concourse, be that dimension statistical, sociological, or otherwise.

A similar argument can be employed in relation to Brown’s claim that Stephenson was opposed to the idea that the concourse was meaningless. If he was, then employing the phrase “the concourse is meaningless” as one of his three basic postulates of concourse theory was probably a poor idea (see for example Stephenson 1988/89, 9). A reader might easily be accused of carelessness if he were to creatively interpret this statement as, say, “the concourse is highly meaningful,” or “the concourse has meaning, but no *specific* meaning.” This would probably have been especially true in the context of the original paper, given that a contrary statement of Stephenson’s would effectively have been altered such that it seemed to *agree* with our ideas. Stephenson says the concourse is *meaningless*, however, and he provides no qualification to the contrary. Far from being careless, therefore, we have taken him at his word.

Indeed, the *meaninglessness* of the concourse is entirely consistent with the idea that no normative dimension exists in the concourse, it is entirely consistent with the second of Stephenson's basic postulates of concourse theory, namely that each statement is for any Q sorter "equiprobable a priori and equipotential a priori" (1988/89, 8), and it is entirely consistent with his Copenhagen quantum theory inspired interpretation of Q methodology. On the other hand, Brown's claim that Stephenson had little investment in the Copenhagen theory is entirely at odds with Stephenson's own words. In 1935, Stephenson recognises that the *probabilistic* Q-technique directly corresponds to "that upon which quantum theory is based," in 1953 his *Study of Behaviour* "laid down further guidelines for these developments," but, he says, "it was not until the 1970s that the pieces were in place for accepting quantum theory as the *modus operandi* for a real science of subjectivity". This "new science," he continues, "is put into operational form in a series of current papers," and it is these papers which we used to ascertain the theoretical stance of the later Stephenson (all quotes Stephenson 1988/89, 2). We wonder why Brown wishes to render Stephenson's *modus operandi* so peripheral?

It seems to us that the greatest irony of Brown's piece is that, were he to be correct, Stephenson would effectively agree with us and we with him. Surely, then, he would have supported much that we have said? It is a shame in such circumstances that Brown has refused the opportunity to engage with so many of the original and progressive theoretical points raised in our paper. We accept, however, that the rendering peripheral of Stephenson's *modus operandi* does rather deflect the need for such engagement. We also have to accept, despite all that has been said, that Stephenson *would not* have agreed with much that we have said (as Brown evidently does not). In this sense, we stand by our initial readings of Stephenson's work (although mutual *agreement* would be most welcome). Stephenson *does* suggest the involvement of isolated minds in Q methodology, he *does* state directly that the statements of the concourse are "meaningless," and he *does not* explicitly recognise any kind of normative dimension at work in the concourse or the Q set (and he certainly does not admit their impact on an individual Q sorter).

Once again, therefore, we are left with a Stephensonian view of Q methodology in which the statements of the Q set are all "equiprobable" *a priori* and in which a "Q-sorter [duly] projects probability distributions upon an *otherwise undifferentiated* concourse" (1982, 258, emphasis added). This contrasts with our own view, which suggests that the concourse is always-already meaningfully differentiated and structured, and that it is these structures which effectively *probabilise* the conduct of those human beings that use and are used by them.

In fact, this process of probabilisation has precious little to do with *social control* or social causation, and it has even less to do with *envirocentrism*, for

we clearly stipulate in the original paper that a Q-sorter retains the freedom to project probability distributions upon the concurrence in any way that they wish. We are neither *enviro-* nor *self-centred* in the context of Q methodology, but concentrate instead on the primacy of the *socio-cultural system of communicability*. We might, in this sense, properly be said to be *system-centred*. In sympathy with this position, we contend that the *pre-differentiated* nature of that concurrence (or system of communicability) means that when any Q sorter engages with the statements in a Q study, certain distributions (or patterns of statements) are simply more *probable* than others. This is then reflected in the appearance of a limited number of factors, the existence of which is evidently dependent upon a certain consistency emerging in the sorting behaviour of participants. Hence, our paper clearly outlines an explanatory mechanism which allows us to see why consistent (and often predictable) sorting behaviour occurs in Q methodological studies, despite their being “hyper-astronomical” numbers of possible sorting patterns available to our participants. In other words, a hyper-astronomical number of patterns are possible in a *statistical* sense (which makes no factors a distinct possibility), but a far more limited number are *probable* in a sociological sense. We believe it is this sociological probability that makes consistent and reliable factors emerge from Q methodological studies.

We hope that these discussions will encourage others to offer alternative readings of Stephenson (or indeed to offer alternative aetiologies of Q methodological factors), for despite our careful analyses, the inconsistencies we have mentioned in Stephenson’s work mean the *historical record* is unlikely to yield any straightforward and less than contentious solutions. Such discussion would surely be mutually enriching. On this note, we would like to end by offering our thanks to Bob Mrtek who was generous enough to offer us the opportunity to express our opinions in this candid and open way.