Q Methodology and its Applications: Reflections on Theory

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With James M. M. Good, Steven R. Brown, Eefje H. W. J. Cuppen, David Ockwell & Simon Watts

***Abstract**: The 2011 Annual Meeting of the International Society for the Scientific Study of Subjectivity in Birmingham offered numerous opportunities for reflecting on the role of theory in Q methodology and on further theoretical implications in applied Q studies. A summary is provided of the opening addresses from Eefje Cuppen and David Ockwell and of a 'roundtable' discussion organised by James Good, with comments from Steven Brown, Simon Watts and Amanda Wolf. Reflections on theoretical matters, both those indicated explicitly by presenters or otherwise suggested by matters discussed, concern theory in Q methodology as methodology, the role of substantive theory in applications of Q methodology and the continuum of theory from the concrete to the general.*

Introduction

The 27th Annual Meeting of the ISSSS at the University of Birmingham in September 2011 was the third in Europe (after Durham in 2002 and Trondheim in 2006), although William Stephenson travelled to Reading in 1989 shortly before he died as a speaker at what was referred to informally as the first British Q conference. Birmingham conference host Stephen Jeffares (School of Government and Society, University of Birmingham) and programme chair Rachel Baker (Yunus Centre for Social Business and Health, Glasgow Caledonian University) brought their Q-methodology experience, knowledge and creativity to bear on many aspects of the meeting. The meeting was the largest to date. It was the first to be preceded by a full two-day workshop (attended by 60 people, facilitated by six Q methodologists and masterminded by Wendy Stainton Rogers). It was the first meeting to include a poster session, and introduced a new prize for the best poster, awarded to Jacqui Greener of the University of the West of Scotland. For Q methodologists from various disciplines, experience levels and geographic homes, the

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meeting was redolent with the opportunities afforded when old and new meet.

In this review, I present some highlights and commentary on the opening plenary session and one of two closing sessions. The opening plenary convened in a dark-wood panelled theatre intended to evoke the feeling of an earlier era, when a young Stephenson might have stood at the podium. While the audience held that image in place, the speakers were in fact two early-career researchers, Dr Eefje Cuppen (Delft University of Technology) and Dr David Ockwell (University of Sussex), reflecting on the place of Q methodology in their policy-relevant research. The discussant was Steven Brown (Kent State University). A heady 48 hours ensued, with 28 paper streams, a dinner in the lavish Birmingham town hall, at which Prof Hung Kyu Kim's career was honoured with the Stephenson award. The final semi-plenary sessions offered participants a choice between considering in depth some developments and innovations with images in Q studies or reflecting on and discussing the role of theory in Q methodology. I was a respondent, along with Steven Brown and Simon Watts (Nottingham Trent University), in the theory plenary, which was introduced and facilitated by James Good (Durham University). I walked out of the conference thinking Q had truly turned a corner.

At an overall level, the metaphor of the corner concerns a maturing of Q scholarship, its appearance among scholars from an ever-increasing range of disciplines and countries and, importantly for these reflections, a vigorous attention to theory, as befits a group of scholars who share an interest in *methodology*. In addition, the metaphor of a corner in the context of intellectual musing appears to me as an aspect of a maze of sorts, with corners around which insights recede like swift shadows out of my grasp—insights I'd surely glimpsed in the sessions, but which were now rather dismayingly elusive. This review represents an effort to catch some of those shadows as they sprang to life in the final semi-plenary, and as the presentations during the conference, starting with the opening addresses, related to the currents of my own continuing exploration of theory and Q methodology.

Accordingly, in this 'guided tour' through the opening and theory plenaries, I have provided some commentary on theory aided by a two-dimensional structure. My aims are both to convey an accurate flavour of the two events and to stimulate further thinking and research in the theory of Q. The first theory dimension differentiates between philosophical and conceptual arguments as they arise as a matter of technique or methodology (the theory *of* Q) as distinct from theory associated with some substantive area of investigation (the theory of stakeholder participation, for example). The second dimension has three loose categories, depending on the level of generality of the theory at

issue. One level relates to a specific dataset or 'case', a second to 'middle-range' theories, less concrete than that applicable to a single case but also less abstract than the third category of grand or general theory. Nevertheless, in the same way that a sampling frame may be dispensed with once a Q sample is selected, or the triangle removed once the balls are set for a game of 8-ball, the two dimensions of theory are simply an initial aid to thought.

I am grateful indeed to the participants, identified above, for their collaboration with this review. Each generously offered notes to assist me, and agreed that I could quote from the audio recording I made at the time. Each graciously checked the accuracy of my reporting and offered useful additional comments. In keeping with the style of this article, the discussion is only lightly referenced, and liberal use of the presenters' first names avoids jarring formality, while assisting with correct attribution of text to contributor. Much of the text is direct quotation or close paraphrase of the presentations or direct quotations from Stephenson's work as selected and presented by James Good.

Opening Plenary

Eefje Cuppen: Three Functions of Q Methodology for Stakeholder Participation

Eefje expressed her motivation, and her guiding criteria for judging methodology, in the substantive terms of her interest in the design and evaluation of stakeholder dialogue. Stakeholder dialogue is a form of stakeholder participation in policy making. Specifically, she focused attention on the importance of 'the identification of the diversity of perspectives, the selection of participants, and the evaluation of the effects of dialogue'. Eefje's presentation touched on both theory in the concrete (applied to specific stakeholder dialogue exercises) and middle-range theory (the processes and qualities of stakeholder dialogue as a form of public participation in policy). She further claimed a 'similar rationale' between Q methodology and repertory grid technique. This comparison touches on the theory *of* Q, but of a more concrete nature, given the claim concerns technique.

Stakeholder dialogues are important, Eefje claimed, for focussing attention on the crucial stage of problem clarification in the most challenging policy situations, such as transitions to a sustainable energy supply, adaptation to climate change, and issues related to food, biodiversity and mobility. Because of the challenges, and the political imperatives to act, debate often centres on solutions, missing entirely important dimensions of the problem and, most importantly, not satisfactorily resolving different people's often deeply held ideas of what the problem is: 'Solutions are not the problem', Eefje told the audience, borrowing the punchline from a comedian well-known to

Dutch speakers.

There are two middle-range theoretical issues in the claim that solutions are not the problem, which Eefje illustrated with competing framings of traffic congestion. Uncertainty or disagreement about the goal of policy and the means to achieve it can often reflect the strength or balance of the theories invoked. In economic theory, congestion is a failure of supply to meet demand for mobility (hence the solution is to increase the supply through more roads). In this framing, reducing demand to match existing supply is ruled out, since free mobility is essential for economic growth. In the environmental framing, however, congestion is a sign that there are too many cars. Since environmental proponents believe that new roads would lead to even more cars, solutions in their perspective centre on reducing the demand for car trips. The debate is one of competing theories, as well as one of the subsidiary evidentiary needs to back one or the other conclusion about the effects of a given policy on congestion.

Since empirical tests of many policy solutions are not feasible—one cannot build roads simply to observe their effects on congestion—Eefje claimed that shifting attention to better problem definition is warranted. Stakeholder dialogue can assist in improving relevant knowledge about problems, making it more likely that effective solutions will be identified and implemented:

> [Stakeholder dialogue] is a vehicle to set up a learning process. In a stakeholder dialogue, people with different perspectives on the problem and its potential solutions are brought together to discuss the issue and to jointly investigate strategies for problem solving. Scientists, policy makers, industry and NGOs engage in dialogue. The purpose of a dialogue is to exchange knowledge and ideas, in order to come to a better understanding of the problem and its solutions.

Thus, the second way that theory enters into the matter concerns the way stakeholder dialogue 'works' as a means to improve policy decisions and to better meet social objectives. When done well, stakeholder dialogue gives participants a 'better understanding of their own and others' perspectives'. It follows that applied research in stakeholder dialogue centres on how, and how well, it facilitates learning. Such research aims, it would appear, target a higher, more general level of theory, where explanations transcend particulars of policy sectors, and national settings for instance, but do not claim to explain all dialogues.

Eefje's *theoretical* approach to finding instrumentally effective ways to structure a dialogue so that participants can 'meet new ideas and insights' started with what is known about facilitating learning. If

people are to 'meet new ideas', they must know their own ideas and be able to exchange them with others. In addition, they must be able to move beyond the mere shared content of their ideas to consider content which is individually unique. The danger, however, of moving beyond shared ideas is that the dialogue can become 'confrontational', centring on different values, rather than unfolding 'constructively' by 'articulating assumptions' and 'looking creatively for ways forward'. Thus theoretically primed, Eefje concluded that she needed methods that could drill through the obvious generalities repeated in the literature, such as the need to bring people with diverse ideas together.

Therefore, while Eefje's 'field' encompasses both policy-relevant and public participation theories, her use of Q methodology was presented as method, instrumentally valuable in three ways. First, Q methodology provided the methods she sought for better dialogue. She used Q to find 'the full range of different perspectives' and 'taken-for-granted presumptions, or implicit assumptions'. Investigating assumptions can show 'where divergent perspectives converge'. Six perspectives were identified using Q in the illustrative example of sustainable energy from biomass in the Netherlands, three of which were not 'dominant' in the sense of being 'easily recognized in the political debate at that time'. Eefje also illustrated the way in which repertory grid technique (based on the work of Kelly, 1955) can aid investigators in identifying the full range of perspectives on an issue. As technique, both Q and repertory grids result in a limited set of holistic ways of seeing a matter or making sense of it, arrived at in an open fashion which allows individuals to show the investigator their points of view through a comparing and ranking exercise using stimuli drawn from the matter at hand (the Q set or a set of elements in a class, such as kinds of fruit).

Second, Q assisted in selecting participants with a diversity of perspectives for dialogues. Diversity is, of course, a characteristic of a collective, not an individual. Accordingly, Eefje drew on Stirling (1998) to 'specify diversity along three dimensions: variety (number of categories, such as nationalities), balance (distribution of participants in the categories) and disparity (similarity or difference among categories)'. From this description, it is apparent that Eefje applied theory—the bearing of diversity on group learning—to information derived *from* her Q study: She sought a 'balanced representation of the variety of perspectives, making sure that disparate perspectives were included'. Participants were selected based on factor loadings, with equal numbers per factor. It was a simple exercise with a Q study at hand, but resulted in an outcome different from the conventional way of seeking diversity by inviting participants from a range of actor types to participate. To reinforce the point, well-known to Q methodologists, Eefje graphically displayed factor loading by actor type,

revealing significant heterogeneity in perspectives among each type.

In presenting these first two uses of a Q exercise, Eefje hoped to persuade the audience, with justifications based in the theory of stakeholder dialogue, that the ensuing dialogue was likely to be better. She said, 'what I really like about Q is its ability to surprise me as an investigator' with that surprise apparently linked to the revelation of previously under-recognised perspectives. For the most part, however, any justifications for improved stakeholder processes based in the theory of Q methodology were simply taken as given. Nevertheless, in her closing remarks, Eefje commented that 'in the context of the stakeholder dialogues in which we used Q, it was very insightful for the stakeholders as well'. Here is, it seems, an opening to advance theoretical appreciation of the value of Q methodology, since the emphasis is on the participant, not on the discourse or the finding of a common problem definition.

Eefje's third use of Q in evaluating the effectiveness of a dialogue in achieving a better understanding of one's own and other's perspectives also cries out for more Q-theoretical consideration. With the perennial evaluation challenge, Eefje demonstrated a clever design using Q, which offers some benefits over the usual self-report method. In particular, the Q approach was better able to isolate the effect of the dialogue from other influences (such as media attention) to which participants were exposed. Eefje administered a post-dialogue Q sort involving dialogue participants and a control group, matched as closely as possible to the dialogue participants based on Q-sort correlations in the study that derived the initial six perspectives. The evaluation centred on the 'average factor loading', which Eefje claimed is a measure of whether the 'dialogue group shows more acknowledgement of the six perspectives' and has 'used the six perspectives to structure the complex biomass issue'. She found that:

> Participants significantly increased in terms of their average factor loading, whereas the control group decreased. The difference between the dialogue group and the control group before the dialogue is not significant, which means that they are comparable groups. The difference after the dialogue is significant. This means that on average the agreement with the six factors increased as a result of dialogue.

I applaud the creativity and adventurous streak in this application of Q, which I believe ascertained successfully that learning had occurred. More precisely, I suggest that the higher average loadings of the dialogue participants show their increased awareness of reified perspectives, to which they were exposed in the dialogue. They may also reflect their acknowledgement of the merits of others' positions, as a result of the

various perspectives being discussed and defended. But care is needed to distinguish this evidence of learning from any implications for advancing efforts to find a broadly acceptable policy solution. For instance, higher average loadings may indicate an increase in ambiguity or ambivalence following exposure to different views. On this reading, the dialogue participants may indeed have 'used the six perspectives to structure the issue'. Yet, they may have achieved a more fully dimensioned problem definition while not advancing toward a resolution of different conceptions of the problem.

Eefje closed on a note about the dual 'richness' of Q: 'Not only is it rich in the information it provides about perspectives, but also it is rich in terms of potential applications'.

David Ockwell: Q Methodology and Reflexive Approaches to Climate Change and Energy Policy

David described his entry point to Q methodology in the context of controversial, culturally charged issues, which are also characterised by uncertainty and the necessity of 'hard choices'. He studied fire management in northern Australia for his PhD, an experience that alerted him to features of Q methodology which, he argued, could also contribute to advancing energy policy given climate change challenges. David's attraction stems from his appreciation of Q for analysing different stakeholder positions, an appreciation of the policy benefits from Q that nicely aligns with Eefje's presentation on stakeholder processes. David claimed that Q 'forced me to be extremely thorough'. Echoing Eefje, his Q study surprised him with a fire-management discourse that he had 'completely missed', notwithstanding that he'd been studying the issues for many years. He attributes the value of Q to its role in 'facilitating some proper reflexive analysis of the options that we face as a society in dealing with problems like climate change', and in particular of helping to 'open up' the appraisal of options, borrowing the term from Andy Stirling, his colleague at Sussex. Stirling argues, in David's words, 'that it is not participation in and of itself that is important, but what's important is that we start a process of policy appraisal by opening it up to the full spectrum of perspectives that exist on the issue'.

David noted the increased interest in participatory approaches in policy appraisal, fortified by various legal requirements and fleshed out with a wide variety of theoretical and normative arguments (citing the work of Habermas and Dryzek in particular; see Reference section for indicative sources). Reflexivity, according to David, entails 'exposing the values, the interests and subjective assumptions that underlie different opinions and subjecting them to critical reflection'. It is often cast in terms of democratic 'voice', on the grounds that the 'lay public' and

people lacking power as conventionally conceived have knowledge that is essential for understanding problems and developing solutions. The very statement of the argument carries the challenge, since effective reflexivity must be achieved in the face of very real power asymmetries and difficult decisions on representation. More to the point, as David illustrated in the case of the UK government's consultation on the future of nuclear energy, the methods used to involve the public may 'close down' the exposure of opinions before any appraisal gets off the ground, by 'framing' them—intentionally or not—in ways that foreclose the full expression of views people may have.

The matter is partly one of method. But David's politically attuned treatment clearly went beyond the merely technical or instrumental. He cited the influential work of Daniel Fiorino (1990) to make the point that an 'instrumental' motivation dominates efforts to find better solutions in politically charged situations, or to simply do the right thing by offering people a say. Q, he claimed, can achieve all three objectives, instrumental, substantive and normative. In his fire-management study, he found two expected 'discourses' well-articulated, one of 'rational fire management' and the second, 'pragmatic locally controlled burning'. But he also 'teased out' two additional discourses, 'fire-free conservation' and 'indigenous controlled land management', which he was not able to find reflected in policy discussions..

From a theoretical perspective, David reached that same conclusion as Eefje, that Q enabled an increased range of perspectives to be available for consideration in a policy discussion. He further reinforced her view that Q offered a way for marginal voices to be heard. The contribution of Q to the instrumental objective, however, is not so evident. According to Fiorino, governments consult the public in order to meet a legal requirement or to achieve some (minimal) level of public acceptance for a course of action. A Q study would seem a poor choice on both counts: it would seem that too few people are involved to satisfy a legal need to 'consult the public' and, by raising 'new' discourses, would seem to complicate a desire for some minimal acceptance of a policy proposal.

David's primary theme, however, was to suggest the promise of further applications of Q, and for this he used the example of low-carbon technology transfer to developing countries. He offered an assessment of the effects of the current 'clean development mechanism', ostensibly designed to transfer low-carbon technologies to developing countries. The mechanism is held to simultaneously help developing countries to achieve bmore efficient economic and human development, to benefit the global environment and to benefit the developed countries that transfer the technology through 'offset' credits under the Kyoto Protocol. David provided data to suggest that, unfortunately, 'technology' had

been framed very narrowly and transfers had ignored the essential underlying knowledge components of technology. Moreover, benefits were apparent in only a handful of countries over a very limited set of 'hardware' technologies, hence serving the interests of only a few businesses and a few countries. Instead of solar panels for sub-Saharan Africa, for example, five 'top technologies are attracting almost 75% of the investment' and of the billions of dollars involved, the least developed countries have received 0.2%, while the developing world as a whole, less China, India and Brazil, has received just 17%.

David argued that breaking out of the framing and the skewed outcomes requires reflexive analysis of the kind Q can achieve:

> Whether or not a low-carbon technology is going to be taken up in a particular context is defined by the culturally subjective perceptions of the people in those areas; the cultural and ecological spaces they move in; their relationships with these technologies which are co-produced between technological innovation and social innovation.

As a result, Q 'can help to understand these subjective constructions and help analysts know which technologies would best deliver against the needs of different people in different places, including the needs of poor and marginalized people'. This illustrates the 'substantive' motivation, as Fiorino (1990) expressed it. Further, when used with poor and marginalised people, the normative motivation comes to the fore. Finally, however, more than in its initial presentation, David appears on the cusp of making an instrumental methodological argument, namely that with Q methodology, a researcher (presumably working closely with the affected people) can find a solution that is at least minimally acceptable to many or most of the affected people. There is a substantial set of Q studies demonstrating just such 'conflict resolving' applications. A full theoretical treatment would be welcome.

The theoretical treatment would be most valuable, however, if it fully engaged with David's claim that Q is a method for reflexive practice, with some of the normative benefits claimed for that practice. David's definition of reflexivity emphasises critical awareness of a range of opinions, values and the like. He appreciates Q's ability to crystallise a range of 'discourses'. Yet, there is something underemphasised in this presentation, since the core definition of 'reflexive' captures the circularity entailed in some cause–effect relationship, some sense of a person's opinion being formed by, and forming, opinions in a larger group or, more abstractly, that one explains one's own norms and desires by turning back to their shaping influences in social interaction. Theories of Q methodology, such as Stephenson's theories of concourse and consciring, and an understanding of factors as operant

structures of concourse can be drawn on to make a plausible case for using Q 'reflexively'. In the absence of such theoretical argument, the instrumental distinctiveness of Q will remain unclear. For me, in any case, as much as I'd like to see more effective technology transfer, the power behind the allocation of billions of dollars seems a rather formidable challenge to a Q-wielding counterattack.

Steven Brown: Plenary Addresses Discussant

Steve's commentary carried one main message: he encouraged researchers not to stop with technically sophisticated work, but to attend to Q methodology as methodology. He drew on a distinction between technique (data gathering; the Q sort), method (procedures for analysing data) and methodology—'the broader conceptual, philosophical frameworks that justify the application of technique and method in the context of whatever is the subject matter'. In Q methodology, the study of subjectivity—'whatever the subject matter'—justifies technical work. There must *be* subject matter, of course. Stephenson's subject matter frequently was of the most everyday sort, such as making a cup of tea or buying a loaf of bread.

It is fair to say, I believe, that no such subject matters make it into the published literature today. Instead, as exemplified in Eefje's and David's work, the subject matters are themselves theoretically challenging and practically important. If we are to bring Q *methodology* back into technically sophisticated work, it seems there are two options. First, researchers might conceive of some rewarding pathways for Q methodologists to undertake studies along the lines of the tea and bread studies. Or, second, they might seek to augment with a further Q-theoretical dimension studies that are already making theoretical and practical advances. Thus, taking the second path, the central question Steve posed, while recognising the disciplinary and publishing motivations operating on the choice of subject matter, is whether and how we might again see pursuit of the study of subjectivity in these more complex subject areas. Yet, if career kudos and professional satisfaction can be achieved, or so it would seem, by drawing out the implications of different perspectives for a situation at hand, why bother with the 'more' of 'ferreting out and understanding the subjective aspects within substantive areas—forestry, public health, education, whatever it might be'? Why, indeed, press even further to putting prominent, if not primary, interest on the 'idea systems of the people who provided the factors'?

Taking this question to David's fire management example, Steve readily accepted the value of David's publications (including Ockwell, 2008, which won an award from *Policy Sciences*). He noted the policy impact David could achieve by assessing the implications of his four

factors, as perspectives on fire management, especially as two of them were not otherwise articulated in the policy arena. Although David intentionally offered only a brief description of each factor as this was not the main thrust of his presentation, Steve pointed out that the factor 'profiles' may have overlooked the elements that did not as easily cohere in the main storyline (such as 'rational fire management' as expressed mainly by scientists): Surely, Steve pressed, there was the 'unexpected' even in the 'expected' discourse? Steve used an illustration from the paper: In the rational fire management discourse there are statements under +4, +5, and +3 that 'indicate devotion to science and using science as a basis for making decisions'. But there is also a statement with a high score of +3 that, Steve stated,

> was mentioned only in passing: 'Aboriginal knowledge of fire regimes is important and should not be overlooked by the scientific and wider community'. Now that's interesting for scientists to say: that local knowledge, aboriginal knowledge that is so different, is important. In fact, it has its own factor. But here was a central point about aboriginal knowledge that was popping up in the middle of this scientific factor, and it was mentioned only in passing, and not otherwise remarked on.

With the aid of an audio recording, it is easy to check that just after reporting that his study revealed two factors that were not reflected in policy discussion, David went on to elaborate what was 'missing'. Missing was attention to matters such as the purpose of fire, human-environment relationships, and cultural fire-management—all matters that had been masked, in his assessment, with a 'nod of the head' to 'indigenous/aboriginal' fire practices. So, the policy discussion masks aboriginal knowledge with a nod and 'indigenous controlled land management' has its own factor. In this juxtaposition David and Steve have given a lovely example that bears reflection for, it appears, the value of aboriginal knowledge is *not* masked in Factor A, rational fire management. We note that David's detection of what was missing was enabled by comparing the content of 'discourses' identified with Q to the content of the policy discussion, which David asserted he had studied extensively (without Q). On the evidence before us, we might wish to look more closely at the masking or evasiveness David detects in the policy discussion. We might further raise a cautionary note (not for the first time) against reductionist tendencies in factor interpretation.

Steve continued to elaborate on the point.

> Puzzles that don't seem quite right can be used to modify the interpretation to make it more tentative in character. Is there something in this rational factor that indicates something really different? I know nothing about the context, but might there be

> something about guilt, about having ignored the indigenous knowledge, that caused them to rank this statement, even though they don't believe it, because there is some kind of ambivalence guilt, shame or whatever that is also part of their factor. Might it be not just about rationality?

Oh dear. Puzzles, ambivalence, tentative interpretations: not really what career-preservation instincts of academics tend to embrace! If Q is to be of instrumental, substantive and normative value, it seems that researchers have to find a way to wade through such morass, and come out intact on the other side. As it stands, and as Steve observed, there was no interest in the Q results beyond the contribution to the policy discussion. That is, the best we can conclude is that the study made some substantive contribution by surfacing 'alternative views'. Cynically, the instrumental and normative objectives were, it seems, dispensed with a passing nod. It's not as bad as all that, however, since through David's academic publication, he can generate interest in the results beyond the immediate northern Australia fire-management situation. That interest falls into contextually allied domains, such as improving the quality of public participation in policy, achieving substantively improved policy solutions, and even—extending to theory—augmenting our appreciation generally of the ways in which the substantive values are achieved and why.

In my view, Steve subtly drew out the overbalanced focus on substance in his discussion of Eefje's argument for the similarity of Q and Kelly's repertory grid technique. Steve argued that there was little overlap between the two because 'repertory grid is primarily a matter of logical capacity', with little or no self-reference involved. There are only so many logical distinctions that people can think of, and thus saturation is reached. However, when it comes to 'self-involvement', logic is not involved, feeling is. Although some stable operant factors may eventuate, we would never conclude that we've reached saturation, or that the factor solution is logically complete. (Of course, once people are asked to *rank* categories, then the repertory grid technique requires some level of self-reference, but likely still of a more structural kind.)

No doubt both keynote speakers will go on to make even more important middle-range contributions to public participation theory. They may well do so by arguing that it was via Q that they were able to discern the refinements they report. But more is needed, in Steve's assessment, achievable by drawing on Q *methodology*. Only then will we come to understand better 'how subjectivity works'.

Steve closed his remarks with advice for those who would influence policy. He suggested that the best Q studies are the result of:

exercises by academics who are very good at analysing cause and effect relationships, and examining perspectives. But then, they magically expect society somehow to do better because now they've shown them that there are factors A, B, C, D. . . . But policy makers aren't always willing to take the baton from us and go and implement what it is that we have found. . . . . So if you aspire to doing more than just analysing problems, I recommend the policy sciences and also using the leveraging advantage that Q methodology provides in other steps and other phases of the policy process.

Semi-Plenary: The Role of Theory in Q Methodology

James M. M. Good: Theory in William Stephenson's Science of Subjectivity

James remarked that the session on the role of theory in Q methodological research and the place of 'subjectivity' in such research was prompted by exchanges on the Q-method listserv over the past few years about the links between technique, method, and methodology, but with a special focus on the place of subjectivity. Unlike the opening keynotes, following which Steve Brown raised the matter of the distinctions between technique, method and methodology in the context of applied studies, in this session the intention was to look at some of 'Stephenson's central ideas about theory', with a focus on the links among them. 'Theory' adds to the progression technique-method-methodology the 'philosophical and conceptual arguments' that Stephenson brought into play to justify his approach to the study of subjectivity. Theory is the 'idea content' in a justificatory exercise. As an historian of ideas, James provided an exploration of Stephenson's subjectivity as expressed in Stephenson's own words, after which there were some short remarks from me, then Steve Brown, followed by Simon Watts, before the discussion opened up to the wider audience for comment.

I have included most of James's selections, consistent with his intention to represent a reasonable 'reconstruction' of the theoretical underpinnings of Stephenson's work. The value of the exercise lies in the light it sheds on the purposes of research using Q. James, therefore, did not direct his remarks to any specific applications. He sets up, however, a point of comparison between Stephenson's aims and the aims of many contemporary Q researchers. Thus, he began his reconstruction of Stephenson's theory by noting, as Steve did, that Stephenson used Q methodology as 'part of his life-long attempt to develop a science of subjectivity', whereas today users of Q 'often proceed without much reference to some of Stephenson's central concerns'. Like Steve, he spoke of the 'more': 'Q involved much more than just providing a means

for studying a person's point of view, central as that was to his enterprise'.

He had 'loftier ambitions'. In a 2010 email conversation, both James and Steve agree these ambitions were present at the outset, in his 1935 letter to *Nature*. The ambition is crystallised at the start of *The Study of Behavior*: 'Our concern, however, is not to be with Q-technique alone, or even principally. Rather, it is with a challenge to psychology, in certain of its aspects, to put its house in scientific order' (Stephenson, 1953, p. 1).

As is well-known, Stephenson wrote over more than 50 years. Whether one is sympathetic to, or dismayed by, the numbers of restatements, apparent shifts in language and numerous backward reflections often tinged with an air of complaint, especially in the later writings, Stephenson's works remain a record in his own words. James opened his tour with some of Stephenson's personal career 'musings' from a very late paper, 'Old Age Research', written in 1989. In that paper, Stephenson wrote,

> Since the early 1930s, Q, and by association myself, had been held to be controversial.... my logic was far ahead of the times. I knew what the new science meant, of indeterminism, quantum theory, relativity, and inductive inference, long before anyone else of my peers and mentors. The hypothetico-deductive method was everywhere *de rigueur*, and remains so in psychology, when I denied it substance as early as 1930. . . . I had prospered in England, becoming Reader in Experimental Psychology.... All of this had to be abandoned when I resigned (of my own will) the bounties of Oxford. I was not interested in lecturing about psychology—I needed to work at what I knew was important about it, its subjective basis.... For this I was tempted to America, where for ten years (1948–58) I was without a fixed appointment, wandering from one University to another as Visiting Professor, or directing research in a commercial research organization. Up to 1972, and continuing up to now in 1989, I have had to face denial of a place in the profession I stand for. (Stephenson, 1989/2011, p. 227)

For other evidence that Stephenson felt motivated to 'be his own man', James cites Stephenson (1988/2006, p. 103): 'It is absolutely certain that in the early months of 1935, before my son was born .... something quite different was involved. I had to throw off dependency upon my father figures, Professors Spearman and Burt.... *I had to be responsible for something uniquely my own creation, that neither Spearman nor Burt had commanded*'.

James illustrated the way Stephenson argued the relationship between technique, methodology and theory in *The Study of Behavior*

(1953), where his use of 'methodology' is closer to 'method' in the distinction offered by Steve. But, as quoted by James, the passage also carries the full sense of 'methodology' by its implications for the purpose of the 'tools': 'There is evidence that new methods exist for probing into man's subjective behavior. For these theory is essential, to help us discover what is observable in principle. The methodology we have tried to outline offers some tools for any such theory' (Stephenson, 1953, p. x). In a 1962 interview, Stephenson argues that his efforts are directed to what we might now refer to as a research programme 'in the study of psychology and all its subjectivity, that is, from the standpoint of the self', as distinct from the investigations that comprise it. From the interview, James quotes Stephenson:

> It is not just a question of giving a Q-sort, just factoring it in a few little ways .... it is not application of a test; but rather an actual piece of work, a research work such as I would imagine a Pasteur or a Freud undertaking ... with many efforts and many mistakes made, but gradually the theoretical viewpoint emerging which anybody could then test at any time they liked. ... so that one is undoubtedly dealing with broad theory in areas of interest.

From this statement, James was led to an unpublished fragment (circa 1969), in which Stephenson clarifies that theory 'enters Q at two main points'. First, in order to undertake exercises in the research programme, Q samples are required, and theory is involved in their structure 'usually as balanced block designs for designated main effects and their respective levels'. Second, theory enters in the abductive stage, in what 'emerges' of a theoretical (testable) nature from the investigations. That is, theory is the aim of the rotation and interpretation of factors, approached abductively, as described in his 1961 'scientific creed' (Stephenson, 1961b).

Most of us accept that 'subjectivity' is a 'slippery notion', and not only in Stephenson's work, where little explicit attention can be found. Ronald de Sousa (2002) has documented some dozen different usages reflecting different aspects of agency and self-reflection and so on. Watts (this issue) questions the pragmatic wisdom of continuing to use the term given the 'baggage' and potential misunderstandings it engenders.

Nevertheless, James holds that *The Study of Behavior*, despite the lack of explicit attention, is 'saturated with the notion of subjectivity, as it is there that Stephenson systematically sets out the theoretical and technical foundations of his science of subjectivity, providing examples of both single case and multiple participant studies'. When we look in detail to detect how Stephenson approaches the concept, we stumble on some challenging statements. One passage James singles out as particularly troubling:

> We are to deal with concrete behavior as such, of the kind described by humanists, historians, playwrights and novelists. . . . the total person-in-action is our concern. . . . what is subjective, such as thinking, and what is observable to others, such as playing golf, are in no way distinguishable for scientific purposes. Dreaming is as much behaviour as is jumping a stile or dashing a hundred yards. All is a matter of interacting with this or that situation. Inner experience and behaviour are thus alike. Both are matters for objective, operational, definition and study. (1953, p. 4)

The distinctions between objectivity and subjectivity are approached in the context of single case studies in psychology. According to James, Stephenson draws attention to two related oppositions in the traditional usage of the terms objectivity and subjectivity. Objective implies testable and reliable, and hence dependable and predictable, operations and events. By implication that which is subjective is likely to be inherently unreliable, undependable and unpredictable. Further, objective implies something which is external. It suggests something in the world existing beyond the inner experiences of any single person. Such a phenomenon can only be considered objective if it can be made public, observed by others, and shared in a reliable way. That is, subjectivity is tied to the inner experience of a person and cannot easily be observed, except by that person.

James went on to note, 'what Stephenson is trying to do is to undermine this dualist separation between subjectivity and objectivity, influenced by John Dewey and Arthur Bentley's transactionalism, which tries to demonstrate the inseparability between what is known and the knower.' The distinction is one of conceptualisation, not one, in Stephenson's words 'indicative of a fundamental bifurcation in the nature of the world', into an 'inner domain wholly separated and closed off from an outside, objective world'. The conceptual distinction is retained only insofar as one can or cannot operate dependably on what is observed.

While subjectivity is of prime theoretical interest, the larger suite of important notions includes the centrality of the self, concourse, consciring, play theory, the cultivation of subjectivity and its quantum aspects. For each of these, James selected a flavourful sample from Stephenson's works.

Stephenson's concept of self was not essentialist; it was 'functional and processual', influenced by gestalt psychologist Kurt Koffka and by social theorist Erving Goffman (Stephenson, 1979).

For his theory of concourse, anticipated in chapter 4 of *The Study of Behavior* (1953) in discussions of structures of samples and populations,

Stephenson evoked a notion of 'shared knowledge' 'as a psychological field . . . the individual's cultural heritage, born of history. . . . All Q-sorts dip into it, as an empirical field out of which new subjectivity grows' (Stephenson, 1982, p. 242). Concourse reveals strong affinities with Moscovici's social psychological notion of social representations (1961/1976) which sees people dipping into the shared knowledge that's available and mediated through magazines and conversation and newspapers and so on.

'Conscire' describes Stephenson's 'thoroughly social ideas of communication'. It is an intersubjective conception of knowledge: 'there is no "mind" in any substantive sense; there is only conscire, the sharing of knowledge in a culture. . . . This takes two forms, one with self-reference for which we should reserve the term *communicative*, and one without self-reference, which we should distinguish as *informational* (1980, p.24).

Building on the work of Johan Huizinga and Wilbur Schramm, Stephenson also developed his play theory of mass communication that focussed on the social and pleasurable aspects of communication as opposed to the exchange of information (1964, 1967).

Stephenson's interest in the 'cultivation' of subjectivity reflects a lifelong interest (starting in Durham with a diploma in the theory and practice of teaching) in education. His unpublished *Quiddity College* (Stephenson, 1970/1980) contains a review of higher education in the United States in the late 1960s followed by a sketch of the features of an ideal college of higher education. He writes, 'My concern . . . is with one's culture as the essence of an undergraduate's experience—taking care to emphasize the professionalizing function, without losing sight of the important need for the student to find his own identity'. As James noted, the 'plans for Quiddity are to be fashioned in the light of the premise that culture is fashioned in play', or as Stephenson writes, 'in the act of communication, of societal conversation, of composition, of intelligent writing and speaking, not in vacuo . . . but about matters of significance' (p. 111).

Finally, Stephenson describes the quantum aspects of subjectivity as an exercise in drawing together several components:

> It was only late in the 1970s that I could satisfy myself about the pragmatics of quantum theory in subjective science. . . . It required the putting together of communication theory, concourse theory, the operantcy of factors and Newton's Fifth Rule, to make tangible what had previously been mainly an exciting analogy between physics and psychology, for matter and mind. (Stephenson, 1981, p. 132)

Comments: Amanda Wolf, Steven Brown, and Simon Watts

After James's introduction, three sets of comments were offered. My own were very brief, reflecting on my efforts to appreciate and understand Stephenson's subjectivity (Wolf, 2008). It helped me to know that the consensus among people who have studied Stephenson's work is that the theory of subjectivity to be pursued with Q methodology came to Stephenson as a whole, and was then elaborated over the course of his subsequent 50-year career. Therefore, we can confidently consider the concepts James has explored as a set, and we can study how they relate. In my 2008 article, it was through an analogy between concourse and Q sort and subjectivity and essentially the interaction of the person who is doing the Q sorting and the concourse that I saw the fundamental coherence of the various elements of Stephenson's theory.

Somewhat to my surprise, the hinge point in that investigation turned out to be the Q sort—merely technique or data collection in Steve's definition—but of crucial theoretical interest when one reflects on just what is going on for the Q sorter while sorting, combined with just what is of import to the investigator, given that the Q sample is devised for some purpose. We can make a useful distinction between (a) the scientist and the work involved in enabling Q sorters to show something of theirs to the scientist, and (b) what the Q sorters do as participants in measurement and producers of data for the scientist. The scientist has some theoretical interest (to study subjectivity as Stephenson would have it, but these days to study some aspect in another substantive field in some domain of the everyday, as in the work of Eefje and David described above). The Q sorters engage in a concourse according to their viewpoint, and thus complete the sorting. We take into account that concourse isn't a 'thing', not a reservoir that exists independently of exercises to engage in it. Concourse has a set of characteristics such that a person who is engaging in it is engaging in social talk, as in a sea of communicability that is emerging even as the person is engaging—there are quantum elements to it. So, when there is Q sorting, there is a special human capacity to exercise the faculty of everyday judgement or, in a non-pejorative sense, common sense. Consciring is about the common sense, it is about what is commonly known and communicated among people who do that communicating. That is what subjectivity engages in and with.

I have further been intrigued with the connections Stephenson was elaborating throughout his writings with the quantum sciences on the one hand and with the American pragmatists, notably Charles Peirce, on the other hand. Iris Hutchinson (2011) has sketched out an explanation for why Q methodology works, for why we are correct to be enthused by what it appears to offer if not uniquely, then certainly powerfully and with some reliability—again, as captured in the enthusiasm for it by

Eefje and David. The explanation has to do with its engagement in the pragmatics of everyday life that is concourse and consciring.

Steve Brown thanked James for setting up the panel, which offered him an opportunity to cast around for some memories to share of things Stephenson said which have coherence, however hard they might be to bind together. Steve notes that the 1961 trilogy (Stephenson 1961a, b, & c) is as close as Stephenson came to being explicitly philosophical, but for those who knew him personally, 'flights of theory' are scattered about. In his remarks, Steve ranged widely.

Steve returned to a theme from his comments on the opening addresses: that many studies these days 'bear the same relationship to Q methodology as engineering bears to science. That is, many studies are applications of technology.... you put in responses, you get some factors out; you characterise factors A, B, C, and D; you write a summary and conclusion and the end'. This observation led Steve to a distinction between 'ad hoc' theory and 'genuine' theory, in which the former is sufficient to explain the case at hand but no further than the data which were used to test it, but the latter transcends the specific case that may have given rise to the theory. Thus, the theory of gravity occasioned by the falling apple transcends the specifics of the apple to explain the tides. The distinction is, in my view, particularly pertinent in the case of applied research.

If we take the case of fire management in northern Australia again, we can articulate a cascade of theoretical levels, more than one of which may be targeted with a Q study. At Steve's ad hoc level, the data are processed to 'explain' the discourse space as comprising four distinct divisions, applying to the case at hand and serving to reduce and clarify a cacophony of opinions into some manageable storylines. The study also allows some movement toward a 'middle-range' theory, claims by the investigators to have produced evidence for the way natural-resource related differences fall out, or ways in which customary resource management ideas are controlled or displaced or ignored in dominant perspectives, or what have you. Stephenson, I am led to believe, would have argued that middle-level theories were simply feeders to the larger research project, which was about the subjectivity collapsed in the several factors. He would, I imagine, pursue an interpretive line of questioning to develop a theory—probably guided by some psychological idea or another—about the inclinations of the factor loaders to place themselves as they did vis-à-vis their understandings (or not) of aboriginals' fire-management practices. At this level, northern Australia (case) and natural resource management (middle-range) both give way to 'genuine' theory (or 'general' or 'grand' theory).

Steve noted the parallel between the theory of gravity and Stephenson's play theory, which 'doesn't just explain one particular phenomenon, like reading the newspaper, but covers all the field of human subjectivity'. Susan Kleine's paper (S. Kleine, R. Kleine, & Allen, 1995) on 'me and mine in consumer behavior', was 'ready made for the application of play theory' but stopped short of that, and consequently, what was a 'good study was quite forgettable, with no broader implications'.

The reflection on Kleine's paper brought Steve to what he'd always taken as 'an article of faith', namely that Q samples be constructed according to some kind of theory, since that 'connects what you are doing to some grander body'. In other words, Steve suggested that studies that do not employ such a structure are unlikely to offer explanations that go very far beyond the case at hand. Steve went on to illustrate the interplay of the theory that goes into the Q-sample structuring and the second site of theorising, abduction in factor interpretation, which James had highlighted. He referred to a Q-sample structured by introversion and extraversion on one dimension, thinking, feeling, sensing, and intuiting on another dimension (which are different manifestations or domains within which introversion or extraversion express themselves), with the eight categories further differentiated by conscious and unconscious. Crucially, however, when people Q sort items selected according to the various categories, and when factors are interpreted, the researcher is not tied to the introversion–extraversion structure, but ought to be investigating in the field of 'some kind of broader theory within which these things take meaning'.

Steve went on to reiterate the same point I had ventured, that 'the person is not only doing a Q sort, but is also doing something theoretical'. By saying that the investigator is not bound to theory, Steve was also clear that the initial structure provides a 'backdrop' which enhances the chances that the investigator will indeed see something new, something that the sample-structuring theory doesn't even address. That is, if there is a factor that is 'neither introverted nor extraverted, nor fixed on thinking, feeling, sensing, intuiting' it is helpful to see it against a 'theoretical structure that won't explain it'. Possibly being able to saying something at the level of the theory that informed the sample structure is an added bonus: 'Among the various things that you might have to say about the results might be something to do with the theory that originally informed you'. This is not likely with the alternative structuring in terms of 'ingredients'. Thus, Steve illustrated, if one has some statements that have to do with Obama and some with Rick Perry; some with the Democratic party and some the Republican party and so on, the statements don't have any particular theoretical interest. He went on to claim that 'those Q studies are best, richest, are

most useful, which are structured according to some kind of theory that has some purchase power in the current intellectual and academic world'. The clear implication is that one needs to look hard for a reason not to use a theoretical structure: it offers no constraint *and* the potential for a bonus for the academically situated researcher who is expected to 'add value to the field'.

A further theme concerned valuing theory for the number of propositions it generates. The 'value of a theory is not in its testability but in the ideas that it produces'. Testability and number of ideas produced may occur in an inverse relationship. For example, 'cognitive dissonance may not generate that many things that you can test; those like Marxism or psychoanalysis may be difficult to test, but generate lots of propositions that will keep a generation of social scientists and psychologists busy'. The warning, it seems, is to avoid treating the narrow and testable as if it were also productive of many ideas.

Steve introduced a new theme in the discussion of theory, touching on the nature of factor analysis as a means, not a reification of some 'entity with independent existence'. The impetus for this reflection was Stephenson's response to Cronbach and Gleser's 'devastating critique' of *The Study of Behavior*, which was published in *Psychometrika* in 1954. His critics, Stephenson claimed, had 'made the mistake of thinking about factor analysis as something that has proved something; that has shown the existence of this factor'. Rather, factors ought to help a person to think, and to theorise. They were instrumental, like 'bubble chambers, signals that turn up on computer screens when subatomic particles go whirling around; stimuli that help the scientist to think about what must be the nature of the subatomic world that it would produce those kinds of things'. What, in other words, must subjectivity be like, if such and so are the factors?

In a final comment, Steve offered some thoughts on abduction, which tied neatly into his comment on the ad hoc and genuine levels of theory. There is abduction at the ad hoc level, which was Peirce's earlier use (and the unavoidable accompaniment to any move from a discovery to an explanation): 'I get a surprising result and I call that an abduction because that's a discovery and so I have to come up with an explanation for that discovery'. But, in judgemental rotation of factors, the abduction is of a vaguer kind, of 'having hunches and impressions, intuitions' (one is not engaged in the direct explanation of a puzzle in data). It follows, Steve claimed, that 'we need to think more about theoretical rotation; that's where there are theories that may not even be completely formulated as theories; not yet condensed to a coherent thought—but nevertheless able to guide us to look in some places rather than in other places; to give us a different slant on reality'.

In the third set of comments, Simon Watts picked up from his earlier conference presentation (revised and published in this issue). He reminded us of the twin challenges motivating his presentation, namely that Stephenson's theoretical writing is very complicated and, at times, somewhat inconsistent and that the concept of 'subjectivity' as conventionally conceived—which is emphatically not Stephenson's conception—finds no point of connection in academic psychology. If Stephenson was concerned to 'challenge psychology' and 'to put its house in scientific order', then subjectivity is not the concept to achieve those aims: 'it's like throwing cotton wool at it'. More than the rest of us, perhaps, Simon is attuned to the difficulties of presenting Stephenson's 'deep and clever' theory accessibly, since the efforts undertaken in writing the forthcoming Sage publication *Doing Q Methodological Research* (2012).

At least in the case of the various workshop and other audiences with whom he's tested the ideas, he has concluded that the complexities and nuances of Stephenson's 'theory prevents people from comfortably engaging with the method'. It's actually serving as a barrier, rather than as a help. Following this same theme, Simon felt the promotion of practical or methodological skills to be more important and was concerned about the need to improve people's knowledge and applied skills in factor analysis. His strong preference for a theoretical perspective was for researchers to ensure that their Q-methodological publications engaged comprehensively and successfully with theory relevant to their chosen subject matter, rather than the method itself. Q is an unusual method, which will be unfamiliar to many audiences, and this means that simple, unobtrusive and non-theoretical explanations of its practical and analytic procedures are of the utmost importance. The idea, Simon suggested, is to explain what Q has to offer that other methods don't, but otherwise to keep focused on the subject matter and on showcasing the findings of the study. Simon concluded by stating that although 'holistic interpretations and holistic subjectivity' were a 'really interesting' feature of Q methodology, one of the biggest challenges for the Q researcher was to find a means of linking these 'first-person holistic' findings to often inhospitable subject literatures, that are generally organized in a 'third-person thematic' format. Finding an effective means to resolve this problem is often the key to producing a successful Q-methodological publication.

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