Q Methodology as Qualiquantology: Comment on Susan Ramlo and Isadore Newman's "Q Methodology and Its Position in the Mixed-Methods Continuum"

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Qualiquantology versus Mixed Methods

As somebody with a long-standing interest in the question of Q methodology as a 'mixed method', I am naturally delighted to comment upon Susan Ramlo and Isadore Newman's (henceforth R&N) current contribution to this concourse of debate (this issue, pp. 172-191). An important strategic aspect of their argument concerns their understandable desire to engage a broader community of interest and practice such that Q methodology might become, in their words, "more than an isolated unique research method with a relatively small following" (p. 187). This is a concern that I very much share with R&N, and so I welcome their efforts to locate Q with respect to some of the features that characterize what they think of as a multi-dimensional *continuum* of scientific research methods. Indeed, some years ago, Rex Stainton Rogers and I referred to the self-isolating tendency of the Q community as the *Amish-effect:* a tight-knit community with many benefits, but one that few outsiders can join because of the high 'epistemic' costs of converting. In the same chapter, we also offered the rather monstrous new word "qualiquantology" to grasp the peculiarly *hybrid* qualities of Q methodology.[1] We described that hybridity as *discomforting* and suggested that:

> hybridity ought to be discomforting, since any genuine hybrid represents a significant reformation in the bodies that are brought together in forming it. Hybridity pierces the boundaries

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[1] To my surprise, this invitation to discomfort packaged in a monstrous neologism has not been completely ignored by the academic community (see Parker & Alford, 2010; Glynos, Howarth, Norval, & Speed, 2009; Melki, 2009; Størksen, Thorsen, & Berner, 2008; Dewar, Li, & Davis 2007; Middleton, 2007; Edwards, 2007; Eden, Donaldson, & Walker, 2005; Ockwell, 2008). New York based psychologists Michelle Fine and Selcuk Sirin (2007, p. 25) even confess that they "take humorous comfort in Stenner and Stainton Rogers' (2004) desire [to encourage hybrid methods]", and proceed to describe their own (non Q methodological) mixing of methods as 'qualiquantological'.

> of identity and opens up the difference of otherness. By contrast, merely adding a qualitative dimension to a quantitative study or vice versa does not constitute hybridity and may be far from discomforting. (Stenner & Stainton Rogers, 2004, p. 101)

R&N have added some much needed flesh to the abstract bones of this kind of position, situating Q methodology in relation to a series of dimensions relating to issues such as normativity versus value-freedom; openness versus closedness; explanation versus exploration, and so forth. However, I am not convinced that they have come to terms with the full discomforting implications of the qualiquantological nature of Q methodology. In particular, I am concerned that they might, in some respects, be advocating precisely the *mere addition* of "a qualitative dimension to a quantitative . . . or vice versa", which Rex and I warned against. There are therefore some important differences of viewpoint that I would like to outline, before suggesting that more care can be taken in distinguishing the qualitative from the quantitative.

A World Bifurcated into Subject and Object

First, as I understand it, the core of R&N's argument is that Q is indeed a mixed method because it fits very well into a 'mixed methods continuum'. Since nobody is likely to argue against the obvious point that Q entails a mixture of qualitative and quantitative aspects, I take R&N's main contention to be about the so-called 'continuum'. There is, however, something rather paradoxical about their continuum. As becomes clear toward the end of the section introducing constructivism, for instance, this continuum seems to rest upon a rather absolute *discontinuity* between a real world (to be tackled by post-positivists) and a world of people's perceptions (to be tackled by constructivists). Whilst a certain amount of diplomatic compromise is commendable, my worry is that this kind of position rests upon what Whitehead called the *bifurcation of nature*—a position that I tried so hard to describe and argue against in my recent paper entitled "Q as a constructivist methodology" (Stenner, 2008/2009).[2]

It must be admitted that this bifurcated conception of nature has some serious contemporary advocates and none more serious and credible than Rom Harré. Harré (e.g., 1997) argues for a dual ontology composed of a p-grammar applicable to persons and an M-grammar applicable to deterministic phenomena such as molecules. Since the first deals with rules and intentionality and the second with causality, Harré can plausibly maintain a constructivist stance as a human scientist and a critical realist stance as a natural scientist. A universe split into meaning

[2] Contra R&N, this argument has very little to do with Kelly's conception of the person-as-scientist.

and matter, in other words, requires a comparably divided knower. R&N suggest something similar in identifying the qualitative with constructivism and 'perception' and the quantitative with post-positivist realism and material causality. The methods continuum is thus cracked in the centre by an unbridgeable fault-line. On one side of the divide a constructivist theory of knowledge guides its qualitative techniques through an ontological territory of something like p-grammar, whilst on the other side a post-positivistic epistemology engages an utterly different ontology of something like M-grammar. This kind of distinction appears to lie behind R&N's (this issue, p. 175) rejection of positivism on the grounds of its supposed "belief that the social world can be studied the same way as the physical world". That is, from their perspective, positivism violates the implicit principle of *discontinuity* lying unacknowledged beneath their methodological *continuum*. Post-positivism is embraced because it renounces the totalitarian pretensions of positivism and accepts "the concept of the social construction of *parts of* reality" (R&N, this issue, p. 176; my emphasis).

The values of compromise notwithstanding, elsewhere I have argued at length for an extended constructivism (also known as 'deep empiricism') based on a unified (rather than divided) ontology (see Stenner, 2008, 2008/2009; Brown & Stenner, 2009). R&N's vision of a harmonious division of knowledge into two camps is therefore very much the opposite of my own position, since for me the qualiquantological aspects of Q concern precisely its *challenge* to the divided settlement between qualitative and quantitative approaches based on a tacitly accepted 'shallow' empiricism:

> Shallow empiricism . . . assumes a splitting between a knower (who knows on the basis of sensory experiences) and a known (an objective terminus for such experiences). For shallow empiricism, 'the subject' and 'subjectivity' are terms that pertain to the knower (and not the known) and the 'object' is that which is known (preferably 'objectively'). The subject is thus associated with adult human beings undertaking difficult tasks of knowledge (and, as a corollary, with 'less than adult' human beings who fall short of the desired objectivity when undertaking such tasks), while the object is associated with the externality of brute material thinghood. Shallow empiricism thus leaves us with a highly distorted and limited conception of subjectivity (which is considered as separate from nature), coupled with a rather partial and superficial account of nature (as an objective externality with no subjective depths). (Stenner, 2008, p. 95)

We need, I believe, precisely to *unsettle* this settlement[3], and I also think that William Stephenson had just such a challenge in mind when he developed Q. One major influence on Stephenson here was the great William James, who based his new vision of psychology and philosophy upon a form of process thinking that entailed a complete rejection of the idea that subject and object are fundamental substances. It seems to me that it is very hard to understand Stephenson's views on subjectivity, and hence his rationale for Q methodology, without having read quite carefully James' two key essays from 1904 on *radical empiricism:* "Does Consciousness Exist?" and "A World of Pure Experience" (both republished in James, 2003).

In the former essay, James advances his theory that consciousness cannot serve as a first principle, since it does not exist *as an entity*, but merely as a *function* (namely, the function of knowing, or what Stephenson liked to call 'self-reference'). Neither subject nor object is to serve as a first term in James' radical empiricism, since the first term (the 'primary stuff' of the world) is *pure experience* and this pure experience is only secondarily differentiated into something 'known' and someone who 'knows' it. Nature is thus not, for James, originally bifurcated such that any experience can be analysed into its subjective and objective aspects in the way that paint can be analyzed by subtraction into pigment and size. On the contrary, the bifurcation is the effect of an *additive* process associated with specific functions, much as paint can function in a shop as saleable product and also function on a canvas as the colour of an artwork.

In the latter essay, James elaborates on the primary notion of pure experience as the keynote of a radically non-foundational metaphysics. For James, radical empiricism is a contribution to a new *Weltanschauung* that involves a considerable rearrangement of western thinking. As he put it, "I seem to read the signs of a great unsettlement, as if the upheaval of more real conceptions and more fruitful methods were imminent, as if a true landscape might result, less clipped, straight-edged

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[3] An unsettling observation: One online definition (http://www.answers.com/topic/post-positivist) of post-positivism defines it, contra R&N, very much along *constructivist* lines as "A school of thought which values qualitative over quantitative research, questions the possibility of objectivity, and draws upon the methods of *deconstructionism.*" I am not endorsing this definition, but use it merely to point out that it is rather partial to identify post-positivism with scientific realism. Very few people who understand the philosophy of science debates that have taken place in the last century or so would now consider themselves 'positivists'. Most serious scientific philosophies are therefore post-positivistic—the question is whether one takes post-positivism in a transcendentalist (e.g., critical realism) or constructivist (e.g., social constructionism) direction. An overly clear-cut distinction between constructivism and post-positivism can thus encourage rather than attenuate the diremption of qualitative and quantitative.

and artificial" (James, 2003, pp. 21–22). There seems little doubt in my mind (especially since he said so in practically every publication) that William Stephenson saw himself as responding to exactly this kind of radical challenge and that he saw Q methodology as one of these "more fruitful methods". Nothing could be further from this project than a retreat to the old dualistic view that "there is a 'real' world that exists (e.g., Newton's Laws of Motion are true for all people and objects) as well as one that is constructed based upon people's perceptions (e.g., non-Newtonian views of motion that are strongly held by physics students despite evidence to the contrary)" (R&N, this issue, p. 174). Such a long-established compromise, in which "the constructivist view is not actually in conflict with the post-positivist view" (p. 174) thus seems to me to evade the true issue. Again, my views about the true issue at stake are clearly laid out—or rather laid out as clearly as I can currently manage— in the publications mentioned above.

Troubling Qualitative/Quantitative Distinctions

If we are to trouble the accepted 'qual/quant' dichotomy rather than accept it carelessly it is necessary to probe a little more deeply into these concepts. Doing so enables us to see more clearly just how radical and challenging Stephenson's Q methodology is, and why it is often still not properly understood after so many years of use.

When words such as 'method', 'data' and 'analysis' are qualified with the word 'qualitative', nowadays often no more is at stake than simply the absence of numerical expression (see, for example, Haslam & McGarty's [2003, p. 119] definition of qualitative methods in psychology as procedures "for studying psychological and behavioural phenomena that do not involve their quantification"). In classic research statistics texts, by contrast, a distinction was often drawn between quantitative and qualitative variates, both of which can be *measured*. I wish to dwell on this during this last section of my response. The former would include variates such as length, weight, and pressure that can be measured *quantitatively* on an interval scale. The latter, also known as *categorical*, would include variates such as colour, shape, texture or taste that can be classified into categories and measured on a *nominal* scale. Variates that can be measured on an *ordinal* scale such as the level of experienced pain from 'absent' through 'moderate' to 'severe' were also categorised as qualitative. Such measures describe the *qualities* possessed by the data (see Krzanowski, 1990, p. 5).

Here, then, there is an interesting sense in which the quantitative (and not the qualitative, as in Haslam & McGarty's case) is defined by negation. Something is quantitative if it is *not* qualitative. The reason for this is that quantitative variables must remain *qualitatively identical to themselves* and must change, or vary, *only quantitatively*. If temperature

is to be a variable in an experiment then it must vary only in terms of *amount* as measured by way of an agreed metric (e.g., degrees centigrade). Temperature 'itself', as it were, must remain qualitatively constant. The quantitative variations, to put it in a nutshell, must show up against the stable background of qualitative invariance. If these conditions hold, then the relations between this variable and others can be established and *functions* described (i.e., relations between variables).

Experimental designs which fail to screen out quality (i.e., qualitative variation), and which include would-be quantitative variables that in fact appear to change qualitatively, are usually considered flawed. They are flawed because any functions discovered may prove to be spurious and based on artefacts. We thought we were measuring one thing, but in fact we were touching upon several things that we had failed to adequately distinguish and control. What we thought were differences of degree were actually differences of kind. We thought, for example, that we were dealing with a continuum of methods that vary by *degree* on a series of clear dimensions, but in fact we were dealing with two very different *kinds* of method.

Those familiar with the history of psychology will recognise that this has been an endemic problem for a discipline attempting to develop quantitative measures of troublesomely subjective variables such as intelligence, attitude, personality, self-esteem, attachment status, and so on. The price of accepting a definition of qualitative methods as merely 'not quantitative' is acceptance of the questionable idea that most psychological variables *are in fact quantitative:* that they do in fact measure different levels of a unitary and stable quality. Re-opening the idea that quantitative variation is definable as an absence of qualitative variation serves a useful critical function here, since much of psychology deals with variables that fail to meet standards that are basic in physical sciences.

The problems hinted at above ultimately derive from the fact that psychological variables are not directly observable and measurable, and hence not amenable to genuine quantitative analysis in the sense defined above. Methodological pioneers in psychology such as Spearman could thus justify using new inventions such as factor analysis to seek latent (hypothetical) explanatory variables on the basis that psychological variables are in principle unobservable and hence identifiable only statistically. In the hard sciences it is harder to justify seeking latent variables given the expectation that these should be sought physically and demonstrated experimentally (although this issue took on a new complexion with the advent of quantum physics). In short, it is only because concepts such as intelligence, memory, verbal ability and so forth are *not* directly and precisely measurable that a procedure like factor analysis (in its original *R methodological* form) was developed

and used in the first place. Krzanowski (1990) puts this well:

> It is accepted that all humans possess characteristics such as 'intelligence', 'verbal ability', 'numerical ability', 'memory', and so on; but none of these characteristics can be measured exactly. The best that can be done is to offer an individual a battery of tests and measure their scores on each. Whereas each test might be designed to measure just one of the traits, in fact all of the traits will generally contribute to the score obtained by an individual on any test. Thus, while a mental arithmetic test might be intended to measure 'numerical ability', clearly 'intelligence' and 'memory' will also play a large part in determining the score on the test. Consequently, the observed inter-correlations among the tests for a set of individuals will be explainable by the presence of each trait in each test. (p. 476)

In reducing a correlation matrix to a set of factors, R methodology makes the assumption that these factors are real but unobservable latent variables. The factors are taken as the traits that underlie what is being measured inadequately by the actual variables. This enables a statistical separation of how much of each trait is present in the measurements of each test based on the assumption that each of the participants possesses a value (unobservable) for each of the factors. The paradox here is that factor analysis, seen by many as the ultimate quantitative method in psychology, was actually a response to precisely the *absence* of genuinely quantitative methods in psychology. It is, in other words, only because the variables *do not* in fact vary only quantitatively (but qualitatively too) that it is necessary to posit the ghostly Platonic essences that are the latent variables generated by factor analysis. The absent quantitative variables are thus in fact replaced by two kinds of ersatz variable: one which is all too profane (the actual variable inadequately measured) and the other all too sacred (the ghostly latent variable estimated through factoring).

It is in this context that we can better understand the intervention made by Stephenson's Q methodology. Rather than eliminating qualitative variation, Q methodology maximises it and makes it the main focus. Through an inversion of the R methodological data matrix in combination with Q-sort data, Q methodology opens up the possibility, not just of a useful technique, but, simultaneously, of a thoroughgoing internal critique of the quantitative assumptions of R methodology, and of a novel ontological basis for inquiry. Q thus entails far more than a simple inversion of R. Different data, different data-collection techniques, and different ontological assumptions hold sway. Stephenson (1936b) captures what is distinctive about this in the following description of his inverted (or Q) factor analysis:

> Factor analysis . . . is concerned with a selected population of n individuals each of whom has been measured in m tests. The (m)(m-1)/2 intercorrelations for these m variables are subjected to . . . factor analysis. The technique, however, can also be inverted. We begin with a population of n different tests (or essays, pictures, traits or other measurable material), each of which is . . . scaled by m individuals. The (m)(m-1)/2 intercorrelations are then factorised in the usual way. (pp. 344-345)

The table of intercorrelations factored in Q methodology is thus composed of a population of tests (items) scaled by a number of participants. The numbers at play are not measurements of the externalities of a public object or phenomenon, but expressions of value or feeling—of qualitative intensity—that result from a number of subjects concerning themselves with a number of objects. The basic data are thus strikingly different. Each data point is the result, as it were, of a controlled explosion of experience, since each patterned Q sort is the expression of an experience of Q sorting. We are not dealing with proxy indicators of otherwise inaccessible variables, but with the stuff of experience, as it were, itself.

Contrast this with R methodology. Table 1 (based on Stephenson, 1936a) illustrates a standard R methodological data set containing data from N persons (persons *a*, *b*, *c* . . . N) on M tests or items (tests 1, 2, 3 . . . M). The aim of R-factor analysis is to explain the observed associations among these M variables by means of the identification of a smaller set of latent variables or factors. The score (or other variate) of person *a* for test 1 is represented by *a*X1, and so on throughout the matrix. Test 1 might be a memory test,test 2 a measure of verbal ability, test 3 a measure of socio-economic status, test 4 of intelligence, and so on (see Watts & Stenner, 2007).

Table 1: R-Methodological Data Set (Based on Stephenson, 1936a)

	Tests							
Persons	***1***	***2***	***3***	***4***	***...***	***...***	***...***	***M***
a	*a*X1	*a*X2	*a*X3	*a*X4	...	...	...	*a*XM
b	*b*X1	*b*X2	*b*X3	*b*X4	...	...	...	*b*XM
c	*c*X1	*c*X2	*c*X3	*c*X4	...	...	...	*c*XM
...	...	...	...	...				...
...	...	...	...	...				...
...	...	...	...	...				...
N	NX1	NX2	NX3	NX4	...	...	...	NXM

In correlating and factoring this data, the raw scores are transformed into standard scores, meaning that the data in the columns

are standardized with respect to the population of persons. The columns can then be correlated since each column contains standardized scores. This yields a test-by-test correlation matrix showing the associations between the variables. A factor analysis applied to this matrix reduces it to a smaller number of latent variables that explain those associations. Each test would then be expressed in terms of its loading with each of the resulting factors. R-factor analysis thus results in the identification of groupings of highly inter-correlated tests or items, which factors are taken as estimates of underlying latent variables. Despite being intimately associated with the so-called 'individual differences' tradition, it should be clear that R methodology is in fact unable to incorporate any qualitative variation stemming from the distinct perspectives of different individuals. Such variation figures only as error variance to be subsumed by the law of large numbers. Through standardization, the tests or items are detached from the responses of any specific individual and are instead attached to a statistical aggregate based on the responses of all persons in the sample. It is this generality that provides the impression of unity and objectivity, but in fact it is a purely statistical unity that disguises a good deal of empirically real qualitative variation.

As suggested earlier, through Q methodology, Stephenson devised a way of maximizing qualitative variation in the raw data (through collecting data in Q-sort form) and of deploying the resources of factor analysis to systematize that variation. As has been suggested elsewhere (Stenner, Watts, & Worrell, 2008) it is relevant in this respect to contrast Q sorting with more familiar quasi-quantitative psychological procedures such as Likert-style questionnaires, attitude scales and personality measures, which aspire to be quantitative in the sense that they aspire to provide objective *measures* of variables which should vary only in quantitative terms. An 'attitude', for instance, is conceived as an actually occurring psychological object with aspects that are, in principle, measurable just as the weight of a dog is measurable. Leaving aside the many valid criticisms of this aspiration (see Stainton Rogers et al., 1995), a participant completing an attitude scale or personality measure is, from the psychologist's point-of-view, a 'subject' in the full sense of being passively subjected to measurement. What the participant *feels* about the items they check is, in principle, irrelevant to the process, just as a dog's attitude to the weighing scales is irrelevant to the question of how heavy it actually is.

The Q sort as a data-collection form, by contrast, is designed to maximise the expression of *qualitative* variation and to record it in numerical form, and it is *assumed* that one person's understanding of a Q set will differ qualitatively from that of another. It is a vehicle for the controlled expression of subjectivity since it is a framework through which a large and formerly heterogeneous set of items is rendered

homogenous by *this* individual at *this* moment. In Q sorting, potentiality is rendered actual *via the perspective of the Q sorter*. Participants are not passive subjects but *genuinely active participants* who operate on a set of items from an explicitly self-referential (what *I* believe from *my* perspective) point of view. This is why I dramatized this earlier as a controlled explosion of experience. In contrast to standard quasi-quantitative techniques in which individuals are *scored* by tests, in Q methodology "the tests get . . . [the scores] instead, due to the operation of the individuals upon them" (Stephenson, 1935, p. 19). The beauty of the technique is that, in so doing, the participant's perspective acquires a numerical representation that makes it directly comparable to that of another, and hence qualitative variation is rendered mathematically tractable.

In sum, my quibbles with R&N's important contribution stem from the importance I give to thinking through the ontological and methodological commitments at play in the use of Q methodology. I see in Q the possibility of a methodology compatible with a quite radical rearrangement of the scientific *Weltanschauung*. Such a rearrangement must inevitably pose a *challenge* to the status quo, and Stephenson did not shirk from this challenge when he identified Q as the methodology of a *quantized* psychology that "dispenses altogether with the old probabilistic of objective methodology in social, psychologic, and psychiatric disciplines" (Stephenson, 1988, p. 187). These are not compromising sentiments. This rearrangement demands a rethinking of the dualistic settlement in which nature is bifurcated into the basic principles of subject and object, and my concern is that R&N are suggesting, on the contrary, a retrenchment of that dualism. If this radicalism complicates the understanding of Q for newcomers, then so be it—let's try to find better ways of dealing with complexity rather than denying it. The data at play in Q, I have suggested, are radically different from that of R methodology, and they are radically *experiential* in a manner consistent with William James' suggestions. Strictly speaking, it would be as incorrect to name the data 'subjective' as to name them 'objective', since what is at play are the 'pure experiences', in James' sense, of a subject (the participant at time T) concerning him-or herself with objects (the items). The methodology is thus consistent with the 'quantum theoretical' process philosophies of James and Whitehead in that it operates with an ontology in which the ultimate realities are neither subjects nor objects, but *actual occasions of experience* (see Stenner, 2008, 2008/2009; Brown & Stenner, 2009). This is no ordinary 'mixing' of methods and it is precisely *not* a matter of an objective 'natural world' being contrasted with a socially constructed and subjective 'human world': it is a *qualiquantology*.

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