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Overly ambitious critics and the Medici Effect: a reply to Kampen and Tamás

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Abstract The critical audit of Q methodology by Kampen and Tamás contains many errors of fact and understanding—indeed, a resistance to understanding that is compared to the Medicis' stance toward Galileo. Following a brief historical summary of similar ill-advised critiques of Q methodology in the 80 years since its introduction, responses are presented to various of the points raised: on the nature of subjectivity, the universe of subjective communicability (concourse) and samples drawn from it, the role of factor analysis and factor interpretation, the forced Q-sort distribution, the ratio between the number of participants and the number of statements in the Q sample, and sources of researcher bias.

Keywords Q methodology · R methodology · Subjectivity · Factor analysis

Those who say it cannot be done should not interrupt
those who are doing it. (Chinese proverb)

1 Introduction

Kampen and Tamás (2014) have written a spirited but regretfully ill-informed critique of Q methodology modeled on Wittenborn's (1961) essay of a half century ago, "Contributions

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and Current Status of Q Methodology," from which they draw their subtitle and inspiration. Their choice is apropos since Wittenborn rode in on a crest of prior misunderstandings to which he chose to contribute rather than question, and Kampen and Tamás appear prepared to follow suit. Had Wittenborn taken the road less traveled by, Kampen and Tamás might have written a more enlightened essay. As it stands, they take their place alongside Wittenborn in a *faux critical* tradition that is worth brief examination before turning to the main points that they address.

2 The Medici Effect

Since its inception, Q methodology has been a recurring target of hastily assembled critiques that have been designed "to set the record straight," but which have instead left confusion and misinformation in their wake. There have been so many of these, in fact, that we have found it useful to gather them under the term *Medici Effect*, named after the famed family that ruled Renaissance Florence, but, among other things, denied Galileo's evidence while refusing to look through the telescope that he had fashioned and that provided his evidence. Q methodology's time-line is strewn with illustrations of this same genre. In Galileo's time, efforts were made to undermine the technology that produced the inconvenient truths of his age. The twinkling of stars so obvious to the naked eye, for instance, was missing when viewed through the telescope, which was therefore judged defective, but now we know that the twinkling was due to the diffraction of light in the moisture covering the surface of the eye (Brown 1985). Likewise in the case of Q methodology, Kampen and Tamás seek to argue that the technology that supports it constitutes an offense against the nature of things and cannot possibly function as claimed.

Q methodology was invented by British physicist–psychologist William Stephenson (1935a,b) and was immediately controversial, being criticized initially by Burt (Burt and Stephenson 1939), and then by various of his followers, most notably R.B. Cattell and H.J. Eysenck. It was Burt who introduced the reciprocity principle—that Q and R factors are necessarily reciprocal—which Stephenson found unacceptable on definitional grounds, and it is this principle that has been a part of the intellectual DNA of virtually every factor analyst since, including Kampen and Tamás, although they are probably unaware of the fact. World War II intervened and so the controversy was placed on hold for the better part of a decade, flaring up again after Stephenson had moved from Oxford to the University of Chicago to resume his career.

During the 1950s, criticism became more heated. Cattell (1951), following Burt, began introducing a variety of other techniques (P, O, Q_S, etc.), which Stephenson (1952) regarded as arbitrary, unnecessary, and as wide of the mark. Mowrer (1953), a colleague of Cattell's, echoed many of Cattell's views, but then later apologized privately to Stephenson for having done so (Stephenson, *ca*. 1965, personal communication). In their review of Stephenson (1953) *The Study of Behavior: Q-Technique and Its Methodology*, Cronbach and Gleser (1954), after criticizing several key ideas, wrote with great fervor that "*it is imperative to discourage students of personality and social psychology from copying Stephenson's designs…*" (p. 330, italics in original), which Stephenson (1954) dismissed as originating from erroneous first assumptions: Had he been trying to do what Cronbach and Gleser imagined, their criticisms might have held water, but he wasn't. Cartwright (1957) followed with a recommendation that Kendall's *tau* statistic be used to cover situations in which a forced Q-sort distribution was utilized, presumably because such distributions rendered Pearson's *r* inapplicable. By decade's end, Jackson and Bidwell (1959) volunteered to relieve Q methodology

of its presumed reliance on the method of paired comparisons, which for $N = 80$ statements would have necessitated $1/2N(N - 1) = 3{,}160$ decisions, requiring an estimated two to three hours of Q-sorting effort. Of course, since Q technique had never relied on paired comparisons in the first place, the modified procedure that Jackson and Bidwell had conceived was stillborn.

The 1960s produced an abundance of criticism. Sundland (1962) recommended that Q technique be abandoned due to what he considered to be the inherent correlation among statements, and Loevinger (1965) was critical due to what she regarded as unresolvable problems of sampling from a population of statements, an issue about which Kampen and Tamás are likewise concerned. To be clear, Loevinger's comments focused primarily on Block's (1961) California Q Set in the context of test theory, but she painted with a broad brush that also implicated Stephenson as if he were likewise narrowly preoccupied with testing. Phillips et al. (1965) were skeptical of Q technique's capacity to detect systematic movement toward self-ideal congruence in psychotherapy and doubted Q's ability to meet the technical standards required. Nahinsky (1967) and Neff and Cohen (1967) converged in different ways on the issue of variance analysis and were critical of Stephenson in this regard, unmindful that he had mainly recommended variance designs for purposes of Q-sample construction but not ANOVA for the analysis of Q-sort data.

Interest in Q methodology began to wane within psychology from the mid-1960s onward, due in large part to Stephenson's departure from Chicago (and withdrawal from academic psychology generally) and acceptance of a distinguished professorship in advertising research in the Missouri School of Journalism, where publication of his *The Play Theory of Mass Communication* (Stephenson 1967) ushered in a new focus and an influx of mainly old criticisms, but now from an entirely new cast of characters. During the 1970s and 1980s, Johnson (1970) sought to correct a perceived defect by showing how Q methodology could be applied to hundreds of respondents, an initiative with which Kampen and Tamás (given their concern with external validity) would likely approve. Cragan and Shields (1981) also endeavored to bring Q methodology into conformity with the doctrine of large numbers. Thompson et al. (1983) promoted a similar idea—that Q methodology could be strengthened by combining it with R methodology—and something of this kind was attempted by Conover and Feldman (1984), who used the factor loadings from Q studies as predictors of other variables. Bolland (1985) resuscitated an old issue that received much attention in the 1950s and that continues to fascinate Kampen and Tamás—that of forced- versus free-choice Q sorts. (That Bolland's article is the most recent on this topic and is itself almost 30 years old adds a touch of quaintness to Kampen and Tamás's concern over what is undoubtedly the largest pseudo-issue in the literature of Q methodology.) Geographer Gould (1985) joined the chorus by contrasting Q methodology with Atkin's mathematical method of Q-analysis (which is totally different despite the similarity in label) and claiming that Q methodology could not faithfully represent anything since it forces complex realities onto Procrustean beds of linearity as required by linear correlation. Garrard and Hausman (1985), apparently unaware of the growing use of Q in the social sciences, recommended replacing the name "Q Sort" in favor of "Priority Sort," simply on the basis of the fact that the latter was focused on decision making rather than psychological assessment.

By the 1990s, Q methodology had gained a degree of notoriety in a variety of social sciences and had begun to attract larger audiences. *Operant Subjectivity*, the Q journal (now in its 38th volume), had begun publication, the annual Q conferences had been initiated (the 30th to be held in 2014), the Q-Method electronic discussion list was underway (beginning in 1991 and now numbering 800+ participants worldwide), and workshops and graduate

seminars were beginning to be offered.[1] Criticisms therefore took a different tack and began appearing as efforts to be helpful: Howard (1995–1996), for instance, offered a combination questionnaire and Q sort as a way to obtain responses more quickly and from a more respectable number of respondents; Billard (1999) suggested ways to help Q methodology to become more democratic; and Peterson et al. (1999) offered a modification explicitly for the study of group dynamics. What Howard missed is that a Q-sort session is not to be gotten in and out of as quickly as possible, and what Billard was apparently unaware of is that most of her proposed alterations (designed to make Q methodology more democratic) are routine procedures of most Q studies already and always have been. As to Peterson et al.'s "new method": it is a clumsy procedure and hardly recognizable as Q technique at all.

The first 15 years of the 21st century have witnessed a further increase in published studies[2] and a decline in published criticisms about Q methodology, and this has been due in part to the existence of the Q-Method electronic discussion list and the fact that differences and misunderstandings often get thrashed out there prior to publication. However, since Kampen and Tamás apparently have never subscribed to this list, they have not been privy to the debates and discussions that have taken place there. In fact, one of the commonalities of critics, like Kampen and Tamás, who have contributed to the Medici Effect, is that they have been unfamiliar with the subjective communicability—or what, in Q methodology, is referred to as *concourse*—concerning Q methodology itself. And there are other commonalities that these members of the Medici share:

- As noted, they do not and have not belonged to the community of scholars who have been interested in subjectivity and in the use of Q technique and method in its study.
- Generally, their critical essays, like Wittenborn (1961), constitute their sole "contribution" to the Q methodology literature. These critics have arrived on the scene without a history and they appear to have no future. They are of the moment.
- In those instances in which they have proposed an innovative modification, they have shown no further interest in it. Billard (1999), for instance, proposed democratizing Q methodology, but then never followed up. Bolland (1985) proposed a solution to the problem (as he saw it) that the Q-sorting task was cognitively too complex for the human mind to cope with, but then he never pursued it. Howard (1995–1996) similarly demonstrated no further interest in his Quest–sort, Jackson and Bidwell (1959) never returned to their modification, Nahinsky (1967) showed how to apply variance analysis to Q sorts and then moved on to other interests, and so forth. Garrard and Hausman (1985) have apparently never again used the Priority Sort that they introduced with such fanfare. Vogel and Lowham (2007) expressed certainty about the superiority of cluster analysis over

[1] We would be remiss were we not to mention two other Q journals (*Journal of Human Subjectivity* and *Q-Methodology and Theory*, the latter in Korean) as well as several other books on Q technique and its methodology—-by Block (2008), Iliescu (2005), Khoshgooyanfard (2008), Kim (2008), Kim (2007), McKeown and Thomas (2013), Prasith-rathsint and Sookasame (2007), Said and Stricklin (2013), Thorsen and Allgood (2010) and Watts and Stenner (2012)—-of which Kampen and Tamás are apparently unaware, as they apparently also are of the 60 chapters and dozen encyclopedia entries on Q methodology (e.g., Brown and Good 2010, Smith 2001, pp. 319–343), not to mention the more than 50 books that utilize Q (e.g., Kanra 2009).

[2] A search in SCOPUS using the search string TITLE-ABS-KEY(Q-method OR Q-sort OR Q-methodology OR Q-methodological OR "Q method" OR "Q sort" OR "Q methodology" OR "Q methodological") identified 1,922 articles. The number of published articles increased from an average of 10 per year in the years up to 1990, to 35 in the years 1991–2000, and 92 in the years 2001–2013. These articles were published in source titles from the life sciences, health sciences, physical sciences, and social sciences and humanities, with psychology (21.8 %), medicine (19.4 %), and social sciences (15.3 %) as the principal subject areas.

factor analysis, but were never heard from again. So far, this phenomenon—of a developmental arrest in interest in Q methodology following the introduction of some kind of reform—admits of no exception.

- If their careers in recent decades have overlapped the existence of the Q journal (*Operant Subjectivity*, 1977-present), they have not been subscribers. They have also not attended any of the annual Q conferences,[3] nor have they been subscribers to the Q-Method electronic discussion list. In short, they have not been *au fait* with the literature, theories, concepts, etc. of the conceptual framework that they so confidently and enthusiastically criticize.

Given the relative isolation of Kampen and Tamás from the group of scholars and practitioners who have devoted a disproportionate amount of their time and energy to the study of subjectivity and to the methodology that renders this study possible, it is not surprising that—as exemplar members of the Medici—they have arrived at uninformed positions on a number of issues, and it is to these specifics that we now turn.

3 Specific concerns in Kampen and Tamás's essay

Kampen and Tamás's paper covers a variety of criticisms of Q methodology and we do not have the space to respond line-by-line to each of them. Instead, we focus on a selection of criticisms that illustrate our larger concern: that these authors appear unfamiliar with or persistently misinterpret significant portions of the Q literature as well as normal practice within the community of Q practitioners.

3.1 On the nature of subjectivity

Q methodology is frequently characterized as a set of procedures, analytic methods, and conceptual and theoretical frameworks that provide the basis for the scientific study of subjectivity, but Kampen and Tamás express doubt that it can really carry through on this promise "of measuring the internal structure of subjectivity" (Sect. 5.1). They initially refer to a contention by Kagan that "social scientists have failed to measure human psychological states" (Sect. 3.1), and they come back around to this in their conclusion: "… the present state-of-the-art science does not permit direct measurement of mental states or subjective representations" (Sect. 5.3), after which they go on to say that the best success to date in this regard has been accomplished by MRI technology and that Q methodology is a somewhat ineffective analysis technique that combines the weaknesses of both quantitative and qualitative inquiry.

The concept of subjectivity has myriad connotations, as Sabini and Silver (1982) have documented, and the version that Kampen and Tamás have adopted, without apparent awareness of alternatives, is mentalistic in nature: Subjectivity for them is substantive and has an "internal structure"; i.e., it is a psychological or mental state that lies buried somewhere in the body and is accessible, if at all, only indirectly or by way of MRI or related technology. This is a 19th century conception with traces of medieval thought that dates back to a time when human conduct was regarded as bifurcated, with behavior at the surface conceived as controlled by hidden and usually indwelling forces, and it was precisely this mentalistic

[3] In September 2013, the 29th annual Q conference was held in Kampen and Tamás's country of residence, the Netherlands (see http://qmethod.org/), offering them an easy opportunity to share and discuss their concerns with the international Q community (among attendants, scholars from 12 universities and research organizations in the Netherlands).

understanding that was rejected by (Stephenson 1953, pp. 96–99) as well as other more modern thinkers, such as B.F. Skinner and J.R. Kantor. For Stephenson, subjectivity was not a subterranean activity, penetrable only indirectly, but a phenomenon that is mainly out in the open (i.e., in monistic rather than bifurcated space) and available for direct confrontation. People obviously have opinions and points of view, about everything under the sun—Kampen and Tamás no doubt have opinions and viewpoints of their own—and these opinions can be collected and examined much in the same way that a botanist might collect and examine the leaves of trees. Subjectivity is nothing other than natural behavior (Brown 2006), and its study is therefore a natural science. It is also methodological: Q samples are comprised of subjective opinions rather than matters of objective fact, and when a person performs a Q sort by ranking these statements from agree to disagree, this operation, too, is subjective, and what renders it so is that it is *my* Q sort rather than *yours*. Your Q sort is subjective to you, and in neither case need an inaccessible mental entity be invoked.

3.2 Concourse and Q samples

Concourse in Q methodology consists of all that has been or might be said as a matter of subjective communicability about anything. Kampen and Tamás wonder whether a concourse might be infinite (Sect. 5.1), which, if true, they seem to regard as something of a death knell; however, they have apparently not read Stephenson's (1978) main statement on this, or at least they have not cited it. Had they, they would have discovered that concourse is indeed infinite since there is no end to what can be said about anything, but this is far from a fatal flaw.

The dividing line between R methodology and Q methodology turns on the fundamental distinction between what is objective and what is subjective. The first line of Kampen and Tamás's essay, for instance, states that "In 1961, Wittenborn published an update of the contributions and current status of Q methodology in *Psychological Bulletin,*" which we can at least provisionally accept as a statement of objective fact, which necessarily falls in the domain of R methodology, but Kampen and Tamás's essay is also full of opinion (although much of it is passed off as fact), of which the following direct quotes and paraphrases are illustrative:

> The present state-of-the-art science does not permit direct measurement of mental states or subjective representations.
>
> The point of focus in QM is the transposed matrix.
>
> Thurstone's centroid method of factor analysis is obsolete.
>
> The number of statements should be at least twice the number of subjects.
>
> Respondents must be allowed to communicate the content and the strength of their views, but this is violated by the structure of most Q sorts.
>
> The claim of measurement validity of QM requires that the rankings given in the interview correspond to rankings that would be given outside of the interview context.
>
> . . . and so forth.

We can take Kampen and Tamás's essay as a broad and representative collection of subjective opinions about Q methodology down the years, but we could easily complement their comments with similar comments from Burt, Cattell, Eysenck, Cronbach, Sundland, Wittenborn,

and all others referred to above, and these critical comments could be supplemented on the positive side with statements by Danielson and Brown (who reviewed earlier incarnations of this essay[4]) as well as from previous publications by Stephenson and the legions who have been drawn to Q methodology for the very reasons that have escaped Kampen and Tamás. The next problem, of course, is how to winnow this volume of communicability down into a manageable subset that is serviceable for purposes of experimentation.

Toward the beginning of their critique of Q methodology, Kampen and Tamás claim that "the QM literature remains uncomfortably silent with respect to how to assemble and verify completeness of a concourse, and how to verify or falsify the representativeness of a sample drawn therefrom" (Sect. 2.2), but this point of their critique merely betrays that the authors have not relied upon a representative sample from the QM literature; rather, they have settled for a limited collection of publications appearing in 2010.[5] If they had searched the literature about Q methodology more carefully, they could have come across a number of sources providing clear guidance on these issues—for instance, some of the key theoretical literature on Q methodology (e.g., Brown 1980; McKeown and Thomas 2013; Stephenson 1953; Watts and Stenner 2005, 2012). Serious researchers using Q have at least one of these on the shelf.

On top of this, a number of the studies that Kampen and Tamás retrieved in their semi-systematic review of Q practices describe in some detail the procedures that various scholars have followed for assembling the concourse and sampling of statements, whether by structured sampling (e.g., Vermaire et al. 2010) or unstructured (e.g., Jedeloo et al. 2010). Many of the authors of these studies also state that they have pilot-tested their research materials to check with participants to see if they recognize any important aspects of the concourse that are not covered by the Q sample (and often repeat this as part of the post Q sort interview). This is good practice—perhaps not always common practice, but then the same could be said for any research methodology.

3.3 Factor analysis and interpretation

Kampen and Tamás's lack of knowledge about the mathematical undergirdings of Q methodology is most readily apparent in their conventional understandings of factor analysis and specifically in their assertions that "the point of focus in QM is the transposed matrix" (Sect. 2.3) and that centroid factor analysis is "an obsolete method" (Sect. 2.3). As was made clear from the outset (e.g., Stephenson 1936) and pressed vigorously thereafter, "There never was a single matrix of scores to which *both* R and Q apply" (Stephenson 1953, p. 15). The focus in Q methodology can therefore never involve a transposed matrix because the scores in Q and those in R methodology constitute two separate and incommensurate matrices: The transposition of an R matrix does not result in a Q matrix, and vice versa. This fundamental distinction is widely known (to the point of commonplace) among those with even the slightest familiarity with Q methodology.

The assumption that centroid factor analysis is obsolete (particularly when compared to principal components and cluster analyses) can only be maintained by Kampen and Tamás by their being unaware of Stephenson's (1953, pp. 38–42) reasoning, by not understanding it, or by ignoring it. Stephenson was the last assistant to Charles Spearman, the inventor of factor analysis, who regarded his protégé as the most creative statistician in the psychology

[4] Unknown to one another, Danielson and Brown were reviewers of two separate earlier versions and rendered critical commentary that Kampen and Tamás have apparently elected not to take into account.

[5] By our count, Kampen and Tamás's search procedure missed at least 30 Q publications appearing in 2010 (10 of them in an edited book on Q methodology) and that were announced on the Q-Method electronic discussion list, which has a subscribership of more than 800 scholars (but not Kampen or Tamás).

of his time, and so at least polite attention should be paid to what Stephenson had to say on this matter. Centroid factor analysis produces an indeterminate solution, which is why Thurstone's principle of simple structure (eventually via varimax rotation) was introduced: to inject determinacy. But Stephenson valued centroid analysis precisely because it had no right answer, which enabled the experimenter to use the machinery of factor analysis (especially at the stage of factor rotation) to explore factor space in search of discoveries. Prior to receiving his doctorate in psychology under Spearman, Stephenson received his first Ph.D. in physics and so may be assumed to have had sophisticated rather than naïve or outdated views about science, and these included the idea that experiments carry a logical specificity with them that requires factor solutions to have more than mere statistical properties to recommend them. Procedures such as principal components analysis and varimax pursue right answers in the same way in each and every study and they are in this sense oblivious to context. That centroid analysis is statistically obsolete is therefore a trivial matter. What is crucial is that from a *scientific* as opposed to a *statistical* standpoint, centroid analysis (combined with theoretical rotation) is miles ahead of the competition. If Kampen and Tamás are not familiar with key ideas such as specificity (J.R. Kantor), psychological cues (Egon Brunswik), abductory logic (Charles Peirce), tacit knowledge (Michael Polanyi), and operantcy (B.F. Skinner), as well as the statistical ideas of Spearman, Thurstone, et al., then they are probably not well positioned to understand Stephenson on factor analysis.

Kampen and Tamás go on to claim that it is unclear how factors in Q methodology are interpreted to reveal viewpoints (Sect. 2.3). The authors' discussion of this issue is idiosyncratic and confuses factor interpretation with later operations (such as matching viewpoints to subject characteristics) that some authors have carried out (e.g., Baker et al. 2010, 2014; Danielson 2009; Van Exel et al. 2008). Again, guidance for how factors can be interpreted is found in the literature, not the least being Stephenson (1983) informative discussion. More recently, Watts and Stenner (2012) devote no fewer than 20 pages to this topic and, among other things, describe their crib sheet method for factor interpretation that many researchers have found useful. Regarding the comments on numbers of respondents and explained variance, as the statement by Cuppen et al. (2010) emphasizes, Q is not the appropriate method for making claims about the number or proportion of people holding a certain view, i.e., for "counting noses." Killam et al. (2010) make a similar point. Contrary to what Kampen and Tamás suggest, however, neither Cuppen nor Killam seems to have made the point about numbers of participants or explained variance in relation to how they interpreted their factors.

This brings us to another confusing aspect of Kampen and Tamás's critique—namely, some of the referencing that they use to substantiate their critique. They say, for instance, that "Other authors arrive at interpretations by correlating rankings and other participant characteristics with factor loadings (see e.g., Vermaire et al. 2010)." Closer inspection of Vermaire et al., however, reveals that they interpreted their factors as follows: "Each factor was interpreted and described as an attitude towards oral health using the composite sort, with emphasis on (1) the statements that characterized the factor … and those that distinguished it statistically significantly from other factors, and (2) relevant interview statements made by respondents defining the factor to explain their Q-sorts" (p. 514), which is entirely appropriate and different from what Kampen and Tamás claim. An additional search of the full text of the Vermaire paper using the keywords *ranking*, *characteristic*, and *loading* (taken from the above sentence in Kampen and Tamás) did not lead to any fragment of text that could be interpreted in the way that Kampen and Tamás assert.

All in all, Kampen and Tamás do not render a single correct and traceable argument substantiating their bold statement at the end of this section—"The lack of consistency with respect to appropriate interpretation is not unusual between (and sometimes even within)

research reports discussing QM"—and this leaves the strong impression that neither author, at any level, has ever engaged with Q methodology.

Kampen and Tamás also devote a significant amount of time to the limits in the number of factors that Q can produce, claiming that it "defies logic" that Q can identify no more factors than there are Q statements, so that "if for instance, 7 billion people sort 100 statements, these 7 billion people are allowed no more than 100 different SRVs" (Sect. 3.2).[6] Here, Kampen and Tamás seem to have gotten so caught up in the certainty of a mathematical proof that they have lost track of the point of Q method. First, all the authors have really demonstrated is that any given Q study has finite precision. This is hardly unique to Q: A Likert question with a 1–5 scale can identify no more than 5 levels of feeling, a thermometer with 1-degree markings can measure temperatures only in whole degrees, and Landsat Multispectral Scanner data can only identify features on the Earth's surface larger than 30 m by 30 m. If we wanted more precision, we could use a 1 to 100 Likert scale, a thermometer with 0.1° markings, or SPOT satellite data (1.5 m by 1.5 m). Likewise, if we deemed a 100-factor solution too crude a representation of the viewpoints about an issue, we need do no more than add additional statements to the Q sample to enable additional viewpoints to emerge. (If there are not more possible statements available to be culled from the concourse, then we can conclude that the concourse itself is limiting the variety of viewpoints that people can have, and there's thus no reason to worry that Q will artificially limit our ability to identify viewpoints.)

From a practical perspective, the threat of being able to identify *only* 100 viewpoints is irrelevant to the aims of a typical Q study. In our extensive reading of the Q literature, we have yet to see a study identify a number of viewpoints even approaching the size of the P sample, much less the size of the Q sample. Much more typical is a set of between 2 and 6 factors. This arises from the goal of most applications of Q method: to reduce the number of viewpoints to be studied by identifying inclinations shared by multiple people. Kampen and Tamás seem enamored with the idea that there is a fixed number of discrete viewpoints existing "out there," with the goal of Q being to identify them all (as if we were chemists aiming to fill in a periodic table). In actual practice, the goal is to help us navigate the complex field of the concourse by highlighting some of the important themes in it. It is also for this reason that Q relies on factor analysis, which allows each individual to load to some degree on multiple factors, rather than cluster analysis, which would assign each individual completely and uniquely to one group.

3.4 The forced Q-sort distribution

Kampen and Tamás go on to question the use of a forced normal distribution for Q sorts. This is a common topic of discussion on the Q-Method listserv by newcomers to Q, and the authors would have benefited from these discussions. They assert that because individuals' actual distribution of feelings about the Q statements may not follow a normal distribution, forcing them to sort the statements in a normal distribution distorts the representation of their views, and thus distorts the resulting factors: "the method used does not appear to be able to detect if respondents would have been clustered if the Q sort was unstructured" (where

[6] We have never before encountered this argument and have no idea where Kampen and Tamás got it, but it is important to note that the data of Q methodology are not responses to individual statements alone, but more importantly in their relationships, as when they are rank-ordered (as in Q sorting). In this connection, Brown (1980, pp. 265–267) has shown that for $N = 33$ statements (which is below average in size), there were more than 44 trillion different Q sorts possible, or more than 6,000 times as many different Q sorts as there are humans on Earth. Not all of these different ways are uncorrelated, of course, but the numbers do seem to leave enough maneuvering room for the usual study, which typically employs fewer than 50 participants.

by *unstructured* they mean *unforced*), but the reverse is more nearly the case. It is only by distinguishing those statements that are preferred over others that those associated with the greater strength of feeling can be revealed.

Moreover, Kampen and Tamás allege that the Q literature is inconsistent about the importance of a forced distribution. As they write, "both statements (forcing matters versus forcing does not matter) cannot simultaneously be true" (Sect. 3.3), but these apparently contradictory statements can, in fact, both be true once context is taken into account. The articles to which Kampen and Tamás refer demonstrate that if participants in a Q study have significant and pure loadings on the *same* factor (i.e., have essentially the same point of view), then whether the statements are forced or freely sorted will have only minimal effect on the factor. In this case, forcing (or not) will not matter. But suppose that participants under free-choice conditions end up dichotomizing the statements (half at the positive end of the Q-sort distribution and half at the negative end), as sometimes happens when the subject matter is highly polarized (e.g., concerning abortion rights or gun control in the U.S.). And suppose further that all participants have placed the same statements at the positive pole and the same statements at the negative pole, an unlikely situation that would result in perfect positive correlations between all pairs of individuals. But imagine that there are actually two separate points of view among these participants and, when forced to follow the usual quasi-normal distribution, that some of them reveal a greater preference for certain statements while other participants reveal a preference for yet other statements, resulting in two orthogonal factors. In this case, forcing will matter and will reveal two distinct points of view that would, in the absence of forcing, remain otherwise (and erroneously) undetected.

The distribution shape of the Q sort is largely irrelevant so long as Q sorters can rank the statements in a way that is roughly homologous with their own viewpoints. This they do by translating their feelings into rankings, and the purpose of the forced distribution is to encourage careful thought, and thus more accurate self-modeling, on the part of the sorter. It is this goal of promoting definite decision-making, rather than any assumptions about the actual shape of a preference distribution, that drives the use of a normal distribution in Q studies. In practice, practitioners of Q methodology have found that most participants have little trouble using the forced distribution, and many report that it was helpful in facilitating the clarification of their thinking. Moreover, it is standard practice (and endorsed by the methodological literature) to allow deviations from the forced distribution when a participant has particular difficulty since, after all, it makes little difference in the analysis whether or not the forced distribution was adhered to.

3.5 Items:persons ratio

Kampen and Tamás devote some attention to the ratio of number of participants to number of items, a topic of dispute in R methodology,[7] but of little if any importance in Q methodology. The situation can immediately be seen if we imagine a Q sample with an unrealistically small size of $N = 4$ statements, which can only be sorted in $4! = 24$ different ways. With $n = 25$ participants, at least two of them must correlate $r = 1.00$ even if their viewpoints are quite different. However, the possibilities increase exponentially as the number of statements increases. For $N = 5$ statements there are $5! = 120$ possibilities, …, $10! = 3{,}628{,}800$, and so on. As indicated in footnote 6, by the time we reach a more realistic Q-sample size of

[7] Pett et al. (2003, pp. 47–48) state that there is no empirical evidence on the ratio of number of subjects to number of items that is required for undertaking factor analysis and that little agreement exists among authorities in factor analysis about rules of thumb to be used.

$N = 33$, the number of possibilities (even taking into account the arbitrary number of ties introduced by the quasi-normal shape of the forced distribution) has metastasized to more than 44 trillion. In R methodology, by way of contrast, a multiple-regression model might contain only a dozen variables that (1) are not representative of the variable population and (2) have likely been selected according to the subjective whims of the investigator, as constrained, of course, by the dictates of prior research. From a methodological standpoint, therefore, Q is in a much stronger and justifiable position.

But what removes this items:persons ratio from the list of important issues in Q methodology is the difference in phenomena in Q and R. Consider a vector of numbers in R methodology (say, $N = 100$ of them) that represent final exam scores for a class of college students. These are objective measures, we assume, and more importantly the scores are independent of one another. Exam monitors may have walked the aisles keeping an eye on the test-takers and parallel versions of the test may have been distributed, all designed to make sure that no one could copy from anyone else, thereby ensuring the independence of each score.

Now, assume that these same scores are for $N = 100$ items in a single person's Q sort. Rather than being independent of one another, these scores now represent interactions that have taken place from within a common frame of reference; namely, the perspective of the Q sorter. Each score is now implicated in all the other scores, each at least implicitly having been compared with all the others, none being independent of the others. In short, the scores are in a state of *entanglement*, which is a signature feature of quantum dynamics[8] that renders Q's phenomenological situation not well suited for consideration of the ratio between items and participants.

3.6 Researcher bias

Finally, Kampen and Tamás claim that Q method is vulnerable to the introduction of researcher bias at a number of points in the research process. It is true, in theory, that an unscrupulous Q researcher could (among other things) select Q statements in an unrepresentative fashion, or pressure subjects to sort the statements in a particular way, or make unjustified choices in factor interpretation, all with an eye to producing certain predetermined results, but the same could be said for any research methodology. If Kampen and Tamás had evidence to suggest that such bias is common in the practice of Q method, this would indeed constitute a serious challenge; however, the authors offer nothing more than innuendo and assertions of their lack of confidence in Q researchers as compared to other researchers:

> Conventional "R type" research is, of course, like QM susceptible to researcher bias. However, in most empirical "R" research, meticulous effort is undertaken to identify, minimize and, when that is not possible, account for these sources of bias. QM, by contrast, is dismissive of such concerns. (Sect. 5.1)

This is purely circular logic: Q method lacks validity because we can't trust Q researchers, and we can't trust Q researchers because the method lacks validity. However, Kampen and Tamás's list of presumed openings through which error might enter (Sect. 3.4) is more than anything a testimonial to their lack of knowledge of the research process in Q methodology. If they had a

[8] Kampen and Tamás elect not to examine Q methodology's parallels to quantum theory (Sect. 1), which is a pity since their implicit commitment to a Newtonian conception of science (as was the case with Wittenborn before them) goes some distance in explaining their inability to achieve a substantial grasp of Q methodology. A glimpse into Q's quantum connection can be gotten by examining the series on "William James, Niels Bohr, and Complementarity," beginning with Stephenson's (1986) first of five articles in *Psychological Record*.

better understanding, their list would shrink and perhaps even disappear. Ultimately, lacking specific accusations against Q on this point (or any specific evidence of R practitioners' virtue), there is little more that can be said on this matter.

3.7 Miscellany

In their lengthy critique, Kampen and Tamás raise more issues than we have the space to respond to, but we would like to assure them that the following points are not only quite addressable, they have for the most part already been addressed in the voluminous literature on Q methodology with which they are apparently unfamiliar:

- The assertion that Q methodologists are "epistemologically naïve" in believing that the method removes researcher bias (Sect. 1) is based on a faulty premise—i.e., that *researcher* and *observer* are one and the same. In a science of subjectivity, the observer is the Q sorter, who is the only person in direct contact with his or her own point of view and therefore the only person who can directly inform on it. Ironically, it is R methodology that combines researcher and observer and thereby injects researcher bias by assuming that the measurement scale carries meaning known only to the researcher and that is independent of the person providing the response. This is a grave error that Q methodology would never make.
- Kampen and Tamás claim to provide a methodological audit of best practices (Sect. 1), but they provide little evidence that they actually know what the best practices are. They cite the usual texts—Brown (1980) and Stephenson (1953), of course, but not McKeown and Thomas (2013) or Watts and Stenner (2012)—but throughout their essay give little indication that they have actually read these works or, if they have, have a glimmer of understanding of them.
- It is asserted at various points (e.g., in Sect. 3.1) that Q methodology has no interest in generalization—more forcefully, that it *rejects* matters of external validity—but Q factors are themselves generalizations in which Q methodologists take the greatest interest; i.e., the factor arrays show how, *in general*, that persons of a particular type think about the issue under consideration. In a science of subjectivity, generalizations refer to the universe of subjectivity communicability, not to the facts of respondent characteristics. In this regard, Kampen and Tamás would benefit from reading Thomas and Baas (1992–1993).
- It is also asserted that Q sorts provided in an interview situation should correspond to Q sorts provided outside the interview situation (Sect. 3.4), but this runs contrary to the specificity principle and the function of the immediate situation in Kantor's interbehavioral psychology (see Brown 2006). A throat swab may return in vitro laboratory results indicating that a particular virus would succumb to a particular medication, but there is no guarantee that the same thing will occur in the specific in vivo situation of the sick person's throat. Following Kampen and Tamás on this matter would not only negate a science of subjectivity, but much of medical science and other life sciences as well.
- Kampen and Tamás doubt that social constructivists would view Q methodology with favor (Sect. 5.2), to which we can only refer them to the many studies to the contrary, such as Graaf and van Exel (2009), Kitzinger (1987), and Stainton Rogers (1991), and the special issue of *Operant Subjectivity* edited by Stenner (2008–2009).
- Kampen and Tamás's claim (Sect. 5.3) that Q methodology has inherited the weakness of reductionism is truly astonishing given Q's four-score years of opposition to reductionism and its championing of Gestalt holism and synthesis over analysis. We're speechless.

4 Conclusion

In the eight decades since it was first proposed by William Stephenson, Q methodology has spawned a vibrant community of active practitioners, and during that time it has also attracted a steady stream of criticism from authors who show little familiarity with the literature and community of Q method and whose criticisms repeat substantial misunderstandings of its mathematical and practical aspects. Like the Medici refusing to look through Galileo's telescope, these critics have failed to engage personally with Q in order to see if their abstract critiques hold up in practice. Unfortunately, the recent article by Kampen and Tamás is little different in this respect. We invite anyone with concerns or doubts about Q to look through the telescope by reading the Q literature, joining the Q-Method listserv, and most importantly conducting Q methodological research. We are confident that such engagement will lead, if not to a love of Q, at least to a better-informed strain of criticism.

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