Rejoinders

Rejoinder to Brown and Dennis

Marten Brouwer

Let me first of all emphasize that I do not adhere to the position that Q and R should be mutually exclusive approaches to the scientific study of human phenomena. I disbelieve the believers, both on the Q side and on the R side. This leaves me, alas, with a rather small audience. One of my former teachers, Hubert Duijker, found himself in the same predicament. In Q circles, he has on occasion been labeled a "Q methodologist." If he was, I certainly am. Yet, that only means that both the late Duijker and I myself belong to the category of researchers with a high degree of tolerance for differing approaches. Dennis, on the other hand, accuses me of "intolerance" (sic) as to "ethnographic parameters" (whatever that may mean). This attitude of hers does not seem to contribute to the strength of her argumentation.

Dennis suggests that the data I used would allow for something like mixing up items about "being in love" with tennis scores or with Chevy Luv pick-ups; she also seems to think that I was applying some external criterion; and she implicates that my subjects were resorting to a "mechanical" sorting of the cards provided. All of this is utter nonsense. To reiterate: the data of my study consisted of quite ordinary Q sorts, with items culled from the concourse of political ideology (like many other Q researchers, including Stephenson himself, have been doing quite frequently); with the well-known instruction to sort according to self-reference; and in the analysis, comparisons were made only within this selfsame set of subjective data.

Next. An obvious, yet important criticism refers to Brown's well known example: measurements of human body segments in inches do indeed show a convincing structure when analyzed on the basis of person-by-person correlations, whereas an analysis on the basis of measurement-by-measurement correlations fails to do so. My point here is simply that we are dealing with nonsubjective data. If the example proves anything it is that the world of objective facts may sometimes be more fruitfully analyzed by correlating persons. The same would probably hold true when we would calculate the amounts of money individuals are spending on different sorts of purchases (food, drink, clothing, holidays, restaurant visits, cars etc.) -- we might predict a very strong factor with loadings from the very rich to the very poor. Now if some problems and/or data sets in the world of objective facts are indeed more suitably analyzed by way of Q correlations -- why should not some problems and/or data sets in the world of subjective feelings be more suitably analyzed by way of R correlations?

In the same vein, I have to disagree with Brown when he concludes that the boundary between Q and R effectively removes the issue of validity in Q. One does not have to equate validity with the existence of some indisputable external criterion with which to correlate the Q findings. Mutual confirmation (or disconfirmation) of subjective data may serve exactly the same purpose, at least if validity is thought of as "concurrent" validity. Moreover, the issue of validity is certainly also at stake in those types of reliability studies where one is not dealing simply with test-retest comparisons of separate Q sorts but with Q structures resulting from different sets of items offered to the same subjects. The boundary between validity and reliability is at least as permeable as the boundary between Q and R. Shouldn't consistency, or predictability, be a more adequate expression for what we try to establish when studying phenomena objectively (including the objective study of subjective phenomena)? Omitting any such criterion puts one outside the province of science altogether.

With most of Brown's comments, I must say, I find myself in agreement. His observation that my study is closest to a comparison between Stephenson's System 2 and System 4 (Stephenson 1953) seems to be quite appropriate; I wish I had thought of it myself. Yet, I think Brown is wrong when he states that my concern (like the concerns of Thomas and Baas) is to strenghten the conviction that something exists "as a matter of fact," comparable to the assessment of intelligence or blood pressure or audience segments. Even though the prediction of the failure of the Edsel car may be a memorable event in the history of Q, my study did not pretend to go beyond the comparison of different ways to look into the structure of subjective political ideology.

One last remark: Dennis is amazed that I should be disappointed in discovering that the metacorrelations between my Q analysis and my R analysis did not reveal similarities. First of all, my use of the word "disappointment" did not refer to my personal feelings, but to the expectations formulated in the hypotheses (but then, Dennis probably despises hypotheses anyway). Second, there are some modest similarities indeed, which cannot simply be disregarded. Third (and most important): I do hope that other researchers will take up the problems I try to tackle with my design and my findings *re* meta-reliability and validity, or more generally: the delineation of areas where Q and R might be more or less comparable and of areas where they are empirically different indeed. Only if that should not happen would I be really personally disappointed. For the time being, it is my estimate that, sooner or later, Q and R will be considered to be not mutually exclusive, but complementary.

Are We All on the Same Factor?

Larry R. Baas
Dan B. Thomas

At Q conferences (the International Society for the Scientific Study of Subjectivity) over the years, Don Brenner has raised the question as to whether or not we were all on the same factor. To anyone who has plowed through the papers by Brouwer and by Thomas and Baas, as well as the comments by Dennis and Brown, and now these rejoinders, the obvious answer to Brenner's question is: No -- we are not all on the same factor. Even within the confines of this symposium there appears to be some bipolarity, orthogonality, and some who are mixed on different factors depending on the condition of instruction. This is not to say that we consider ourselves to be on the "correct factor." We are well aware that if William Stephenson had been alive when we presented an earlier version of our paper at a Q conference, he would have publicly admonished us for even entertaining the idea that a paper such as ours was needed. For as Brown has reminded us, Stephenson thought matters of reliability and validity -- as used in R methodology -- had no applicability for Q. Of course one has to conclude that Stephenson was on the right factor and all judgmental rotations should focus on his loadings, for as he once somewhat humorously (we think) noted, it was his method, after all, and he could do anything he wanted with it.

While Stephenson would have publicly rejected our studies as irrelevant to issues of Q, we believe (hope) that perhaps he may have at least reluctantly acknowledged that we had demonstrated an important property of Q. For we know we occupy similar factor space with Stephenson when we note that it is not the items per se that are important, but it is what people do with them that matters. Additionally, we believe Stephenson would agree that giving different Q samples (reasonably selected from the same concourse) to different persons (reasonably selected from the same P sample) would generally yield similarly interpretable results. Of course, as Professor Brown has reminded us, the expectations of constancy of this sort are a bit more complex than is the case with simple test-retest assessments of reliability common to R method psychometrics. Given genuinely comparable XYZ situations, and matching conditions of instruction, it is fair to expect findings that pass the muster of "reliable schematics."

Why we would do a project such as the one presented here, knowing that in a theoretical sense it might be considered irrelevant, is an interesting question and it demonstrates the different worlds which the scientist must traverse. The world of Stephenson -- and, as indicated by his his comments here, sometimes Brown as well -- is a theoretical, abstract one, where issues of reliability and validity in Q have no relevance, except in a limited set of circumstances. Or so it would seem from our own somewhat orthogonal point in factor space. From our vantagepoint, fueled surely by unmitigated self-reference, the world assumes a slightly different character; or so it seemed as we found ourselves approaching these matters as worthy of extended treatment. Informed not only by self-reference, but no doubt a fair measure of egocentrism mixed in with a little or more defensiveness, we were inclined to regard this "different world" as more realistic perhaps and definitely "dirtier" than the one we saw Professor Stephenson and generally Professor Brown inhabiting. This was (and remains) the world in which journal editors, as well as reviewers, have been trained to see the world and to evaluate research through the lenses of R methodology and its constituent doctrine on matters of measurement. In our view of this world, one meaning of the "law of large numbers" is that R methodologists do and will for the foreseeable future outnumber Q methodologists by a factor of magnitude that guarantees that every Q study submitted to a journal other than this one will be reviewed by more of them than us. That is, our world was one peopled by persons of power who failed (or refused) to consider that there is a "fundamental incommensurability between objectivity and subjectivity" (Brown, 1972) that admits of no reconciliation at the level of measurement.

In our less abstract world, the rules change somewhat and the issue becomes one of strategy: What do you do when a reviewer says you have a potentially publishable article if only you produce the same results utilizing a different Q sample with a different group of subjects? Under that condition of instruction, we chose to do the study again -- the second Reagan study -- as a means of verifying or replicating our study to demonstrate the results were not unique to the first study. We had no doubt it would work, and it did. The article was accepted and has since been published. To confirm that our verification was not unique, we did it again under even more stringent conditions -- the Bush tandem studies -- and the results have been reported herein.

So the point is very simple. In the theoretical worlds in which Stephenson and Brown generally discuss these issues -- and we would like to as well -- one can play the absolutist game of irrelevancy. In the real world where political forces often run contrary to your own interests, different strategies may be necessary. One sometimes may have to slide onto a different factor for a portion of time and discuss the matter on a different level. To those who may consider this a form of prostitution, we beg forgiveness. To go beyond self-reference into the realm of self-servingness, we think this may be a case where the road to heaven (where Q is an acceptable methodology) is paved with less than pure intentions.

Accordingly, we believe that if given similar conditions of instruction -- describe the world as you would like it to be -- we would share factor space with Steve Brown. We are not so sure, however, about Karen Dennis. Dennis speaks of looking through the world with Q-colored glasses, and she certainly seems to do this at times. However, some of her suggestions leave the impression that her Q lenses maybe clouded somewhat and prevent her from fully appreciating the Q methodological perspective. In her remarks, Professor Dennis begins by suggesting that we have made a "convincing argument," but then faults us for not presenting more items so that better comparisons can be made between the two factors in the different studies. On the surface this seems like a harmless request with which we have no problem. But when looked at in the context of her other remarks, we see submerged here the commencement of a debate -- more common to R methodologists -- as to the precise meaning of individual statements as if these items actually had some predetermined meaning irrespective of the meaning ascribed to them by the subjects. She overtly decries focusing on "linguistics," yet isn't that precisely the direction her counsel takes us? We believe it is and that such endless disputations are best left to the world of R methodology.

As for Professor Dennis's suggestion that it might have been worthwhile to undertake a second-order factor analysis of the data from the Reagan studies -- presumably to tighten the case for comparability in the two sets of factor structures -- we are not exactly sure how we might have done so given the fact that different statement samples are employed with different P sets in the two studies. More fundamentally, our differences seem to center on our belief -- a premise shared neither by Professor Dennis nor Professor Brouwer -- that issues of reliability in Q are indelibly hermeneutical at their core and not reducible to the level of a simple coefficient or statistical index.

It is one thing to flirt with R for strategic purposes -- and on this score, we concede, we feel slight pangs of guilt for even countenancing the act of infidelity -- but it is quite another thing, as Brown suggests, to carry the romance too far. It is quite simply the case that many, if not most, R-methodological ideas have no applicability in Q. It is not going too far to suggest that different methodologies may require different measures to assess their scientific worth. To take a crude analogy from sports, it would be absurd to suggest that assessments of Michael Jordan's career as a professional athlete include an inventory of the number of home runs he hits or his current ERA. Even within the same sport, like baseball, different measures are necessary to assess the abilities of pitchers and hitters. So it is with Q and R. Measures that make good sense in R make little sense in Q, and we believe it does neither method good to force the other to utilize its perspective to evaluate the respective claims of virtue. Reliable schematics, we would argue, fits Q quite nicely. It most certainly would not, however, satisfy most R methodologists.

Finally, Sylvan Tomkins (1963) once noted that there were basic "right-wing" and "left-wing" dimensions that permeate most aspects of life. We generally understand that distinction as applied to politics, but Tomkins noted it could also be applied to science. The right-wing scientist is concerned with controlling error whereas the left-wing scientist is more concerned with discovery. In many respects such a distinction also differentiates the R and Q methodologist on this subject: R methodologists are concerned with the control of error (validity and reliability), whereas the Q methodologist is concerned with discovery, or "a quest for concepts of importance" (as quoted in Brown, 1992/1993). Hopefully the discussions reported in this symposium will put to rest some of the right-wing concerns and allow us to rejoin the quest for "concepts of importance." Personally, we are tired of being right-wingers and look forward to returning to our old left-wing ways again.

References

Brown, S.R. (1972) A fundamental incommensurablity between objectivity and subjectivity. In S.R. Brown & D.J. Brenner (Eds.), *Science, psychology and communication: Essays honoring William Stephenson* (pp. 57-94). New York: Teachers College Press.

Brown, S.R. (1992/1993) On validity and replicability. *Operant Subjectivity, 16,* this issue.

Tomkins, S.S. (1963) Left and right: A basic dimension of ideology and personality. In R.W. White (Ed.), *The study of lives* (pp. 388-411). Chicago: Atherton.