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Fit for Purpose? A Response to *An Overview of the Statistical Techniques in Q Methodology*

Bob Braswell
Global School of Theology Western Cape

The article under review suggests, in its title and subtitle, that in addition to reviewing statistical techniques currently used in Q methodology, it intends to present a better way. This prompts the question, better in what way?

The article's introduction presents a problem: that the popularity of Q methodology is limited by the lack of some mainstream factor extraction and rotation options in dedicated Q software, and the lack of inclusion of specialized Q routines in "normal" statistical packages such as SPSS, SAS, and so on. Our author suggests that old methods such as CFA are used because of loyalty rather than technical merit, while sexier alternatives are ignored. This introduces a review of statistical techniques based less on issues of technical fitness than on mainstream acceptability among R-method statisticians. If one objects (as I do) that such popularity is of little relevance when choosing tools for scientific research, one is tempted to dismiss the whole article. Yet popularity is not a bad thing, if it doesn't come at too high a price, so it is worthwhile to spend time with the article looking for meritorious suggestions along the way.

The first major section is an overview of factor extraction methods. There is nothing new or surprising here until the issue of non-orthogonality is raised with regard to CFA. Nothing is said about how this affects Q results, and for good reason. This barely noticeable artifact has no effect on Q results. Raising the issue serves only to add fuel to the emotional resistance to CFA based on the other major problem raised: CFA's really unforgivable shortcoming (when fashion and popularity are at issue), of being old. Presumably, the cool kids don't use CFA.

The view that CFA is out of date is, itself, out of date. If we research uses of CFA since the turn of the millennium we will notice, perhaps with some shock, that CFA has been rediscovered as an ideal tool for Latent Semantic Indexing (Chu & Funderlic, 2002; Cardoso-Cachopo & Oliveira, 2006), the technology behind data mining in text databases. Although Google is extremely secretive about the algorithms it uses to index the internet and serve up search results, it seems likely that a major component of its index of the internet is the lowly centroid factor algorithm. CFA is particularly useful in semantic indexing of a growing corpus such as the internet because unlike PCA and similar least-squares based methods, with CFA the whole matrix (i.e., the index of the whole internet) doesn't have to be recalculated when new observations are added (ibid), and without this attribute of CFA, Google's computing power, though enormous, would not be able to keep up.

I will admit that this is perfectly irrelevant to CFA's fitness for purpose in Q, but this admission takes effect only after CFA's critics admit that its age and history as a PAF substitute are equally irrelevant to whether it is the right tool for Q. Meanwhile, I think

Contact author: bob.braswell@gstwc.org.za

we should all use CFA because, according to rumors, the really cool kids like Sergey and Larry are way into it.

In the author's comparison between CFA and other extraction methods, the adequacy of CFA-based results is not brought into question – again, for good reason. A different question is raised: Why have PAF and ML not been added to Q-specific programs? It was perhaps asked as a rhetorical question, but it is a question that has a practical answer. These methods rely on multiple linear regression for communality estimates, and the regression step (and thus the extraction routine, and thus the software) will fail with a large subset of Q data sets. Existing PAF and ML approaches will fail with any data set that doesn't respect R-centric expectations about the ratio of observations to variables. These routines have historically been seen as unsuitable for inclusion in Q-oriented software for the very practical reason that it would fail with many otherwise acceptable data sets, and users would be reluctant to trust software which bombs regularly.

If methods that depend on R-method rules as imposed through multiple linear regression are eliminated, we are left to choose between CFA and PCA. CFA is based on the common factor model and PCA is not. When the choice is thus narrowed, Brown's oft-quoted comments about CFA being preferred on theoretical grounds become clear. CFA's "unique" indeterminacy (in the author's second sense, that all common factor approaches share), if taken in the context of a comparison between CFA and PCA, are completely sensible. The discussion of indeterminacy beyond this sense could be dismissed as a misunderstanding of this context if it were not for a deeper misconception that must be dealt with specifically.

It is important not to conflate accurate results with optimization based on statistical criteria in intermediate steps. In Q, "results" come much later than anything that happens in the factor extraction stage, and take the form of a useful interpretation of shared points of view represented by (rotated) factors. The search for meaningful results in this sense is indeed aided by avoiding the *cul de sac* of inappropriate reliance on determinacy in the form of a mathematical optimization to a preliminary step on the way to that result. In addition, our author's argument against indeterminacy in CFA also applies to PAF and ML, according to his own explanation. In arguing against mentioning indeterminacy as a theoretical reason for choosing CFA, he is arguing against the common factor model.

Let us turn to our author's suggestions regarding factor rotation. Rotation is taking a multidimensional arrangement of points in factor space and looking at this fixed arrangement from various angles, ultimately deciding which combination of axis angles reveals the most useful information. Our author proposes two improvements to the typical tools used by Q methodologists for this step: he would have us add additional rotation options borrowed from R-methodology, and deprecate the use of the manual rotation technique recommended by Stephenson, on the grounds that he believes manual rotation to be subjective, lacking in reliability, and unscientific. My response to the author's suggestions offers no resistance to the additions he suggests but focuses on addressing the misconceptions behind his deprecation of manual rotation.

The objections raised to manual rotation betray a lack of intuition about what is happening in rotation. Perhaps there is an assumption that different rotations could change the results of research and essentially contradict each other. This is a misconception. Rotations of the same data can always only reinforce each other from another perspective, as the arrangement of points in factor space is not changed at all in the rotation process, and this is as true for manual rotation as for any other method.

I think most of the inability to conceptualize what is happening in rotation comes when we deal with more than three dimensions, so let us limit ourselves to three for the moment. Suppose we have an insect encapsulated in a transparent ball of glass, and we can turn this ball along any axis to better view whatever portion of the insect we are most interested in at the moment. Does turning the ball change the insect in any way? Does one viewer's interest in looking at the back pair of legs do damage to the insect's proboscis? Why then should anyone's nose be out of joint over manual rotation? Is there a wrong way to look at it? No, there is no wrong way to look at the insect unless we fail to turn the glass ball to best reveal *the part we want to examine*.

Is a particular manual rotation valid? Mathematically, all rotations are equally valid, though not all *interpretations* are valid, so the right rotation, the most scientific rotation, is the one that is most useful to aid interpretation. This is a question for the judgment of a researcher who can actually look at the data, not a question to be decided beforehand by a mathematical algorithm.

Is manual rotation reliable? That is, are the solutions repeatable? In the hands of a novice, that may seem unlikely, but in the hands of someone with a bit of experience, and if the dimensionality is not high enough to make the problem intractable, manual rotation is readily repeatable. (If our author thinks otherwise, this is a question amenable to experimentation. Such a basis in data is missing from his own assertions.)

Let us turn now to the logic of adding machine rotations but dropping manual rotation. Suppose someone adds oblique and quartimax rotation options to PQMethod. Researcher 1 and Researcher 2 are asked to analyze independently the same data set using the updated software. Researcher 1 chooses the new oblique rotation, Researcher 2 chooses the quartimax option, and the two solutions don't completely match up. Does this mean these rotation options are unreliable and unrepeatable? No, it only means that different researchers made different decisions based on their goals in interpreting the results. In the same way, different researchers using manual rotation may make different decisions. If these differences count as a lack of reliability, then it is also a lack of reliability to choose different machine rotation options. In that case, it decreases the reliability of the software to add the new rotation options! Let us back away from this absurdity, and at the same time lay to rest the misconception that making one's own problem-guided and theory-guided decisions as a researcher counts as inappropriately subjective, unreliable, or unscientific.

The reforms in this article remind me of those in Shaw's *Pygmalion*. In the same way in which Henry Higgins "helps" Eliza Dolittle without ever becoming aware of his own bias in the matter of whether his help was needed, our author counts himself a true friend of Q and works hard at reforming it. Like Higgins, he seems unaware that he is applying categories brought from elsewhere that have limited value where he is applying them. It is unclear whether Q will be actually helped by them, but the suggested improvements might serve to make Q more presentable in certain (R-methodological) neighborhoods.

Though it is rational for Stephenson's students to resist faddishness as a reason to change statistical methods, they must still be concerned that Q methodology not be seen as an interesting antique rather than a vital enterprise. In this connection the current dialog is helpful. The needed vitality, however, does not depend on moving away from CFA or manual rotation. Adherence to these methods is not just a matter of loyalty but a matter of learning. Where these "old" methods are most thoroughly understood they are also most fully appreciated and most fruitfully applied.

References

Cardoso-Cachopo, A., & Oliveira, A. (2006). Empirical evaluation of centroid-based models for single-label text categorization. *INSEC-ID Technical Report*, *7*, 2006.

Chu, M. T., & Funderlic, R. E. (2002). The centroid decomposition: Relationships between discrete variational decompositions and SVDs. *SIAM Journal on Matrix Analysis and Applications*, *23*(4), 1025–1044.