REPLICABILITY OF RESULTS WITH THEORETICAL ROTATION

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The ability of two or more independent investigators to replicate the results of an experiment is one of the touchstones of scientific method. In what follows, I will present an example of such replication in which Q methodology was employed, using for illustrative purposes a study in which 29 subjects were presented with 33 statements on the issues of nuclear weapons, national security, and the peace movement. My purpose is to clarify the role of the investigator's subjectivity in Q methodology and to show how, when properly employed, that subjectivity can facilitate rather than hinder the attainment of replicability.

There are at least two points in Q methodology where the issue of replicability of results arises--data collection and factor rotation. At the point of data collection, the replicability problem involves the reliability of the measurement instrument itself. In other words, if an independent investigator used the same concourse of statements to collect data from a second, similar set of subjects, would the data he/she collected be sufficiently similar to the original data set that we could say he/she "rep-

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licated the results"? The answer to this cannot be decided simply by performing a second collection of data, since the question then arises by what criteria we are to compare the two sets. This, in turn, cannot be answered without factor analyzing the data and rotating the factors, which itself raises questions of replicability. Thus the replicability of data collection (reliability of the measure) cannot be established without first establishing the replicability of factor rotation. This paper will limit itself to replicability at the point of factor rotation.

To put the problem in another way, two independent investigators have no basis for agreeing that two data sets are similar unless they can first agree on what factors are contained in a single data set. Note that this problem is especially acute for social scientists; natural scientists (e.g., biologists, who frequently employ numerical taxonomy and Q cluster analysis) can often decide whether two data sets are similar by objective factor rotation procedures such as varimax. As Brown (1980: 40-43) has pointed out, however, in the case of Q methodology such procedures often give misleading results. Given the need for theoretical (judgmental) rotation, can investigators using such subjective procedures replicate one another's results? If not, the scientific claims of Q methodology would be dubious.

Evidence indicating that theoretical rotation is indeed replicable was gathered in the course of work on the abovementioned nuclear weapons study. After the data were collected, a duplicate copy was sent to another investigator, who rotated the factors using a very different theoretical framework from my own. The theory that guided my rotation was drawn from Lifton and Falk's (1982) *Indefensible Weapons*. The other investigator's framework was drawn from the theories of the legal theorist Myres McDougal (1983). In spite of these different frameworks, and in spite of the personal and political differences between the factor analysts, both analyzed the data into three factors with very similar factor arrays.

One crude measure of the degree of similarity between the Lifton and McDougal factor solutions is the

number of statements that appear in common at the tails of the respective factor arrays. In the quasi-normal distribution, there are two statements in the ±4 categories and three statements in the ±3 categories, which is to say five statements in each of the tails. On the first factor, the Lifton and McDougal arrays contained three out of five statements in common in one tail of the Q-sort distribution, and two out of five in common in the other. On the second factor, the arrays contained four out of five in common in both tails. On the third factor, the arrays contained three out of five in common in both tails. In more qualitative terms, the Lifton and McDougal rotations converge on three common factors, roughly recognizable as doves, ideological hawks, and non-ideological hawks.

Thus, with a single set of data, an investigator using one theoretical framework replicated the factor solutions of another, independent investigator using a different theoretical framework. A simple interpretation of this correspondence would be that both theories facilitated the discovery of patterns inhering in the data. Some would say that this confirms the truth-value of the theories. But at the very least we can say that the patterns belong to the data, and are not read into the data arbitrarily by the investigator in his efforts to prove his own preferred theory.

Given this equivalence of the two theories with respect to the data at hand, is there any basis for choosing one over the other, and thus one variation on the common factors over the other? In order to answer this, the differences between the Lifton and McDougal rotations should be examined. The most obvious difference concerns the dove factor. The dove factor constituted by the Lifton rotation most strongly agrees (+4) with the statement, "We need to remember that the Russians are human beings like us--men, women and children." The McDougal dove, on the other hand, is moved less by existential than by pragmatic political considerations, giving a +4 to the statement, "The ones who profit from the arms race are people with careers in the military or in military produc-

tion, but not ordinary citizens."

If this were the only difference between the two rotations, the choice between them would be difficult, since one rotation does not yield an advantage of simplicity over the other. To decide such cases, some methdological criterion should be put forth or else the decisions will be made by each investigator on arbitrary grounds. Although such cases would still be judged differently by different investigators, their judgment would at least be guided by, and accountable to, a publicly stated norm.

The development of a criterion for deciding between rotations of equal simplicity is, by its very nature, a function not of pure reason but of practical reason. A possible criterion would be as follows: When two rotations are equally simple with respect to the data at hand, the rotation which best represents larger social cleavages should be chosen. Assuming that all persons of good will are committed to the just and lasting resolution of social conflicts, social scientists can best serve this common good by orienting their research to an illumination of what cleavages are actually at issue in these conflicts. Authentic communication and peaceful resolution of conflict will only be possible if the depths of social cleavages are explored, and the knowledge shared publicly.

Very often, however, factor rotations appearing to be equally simple with respect to the data at hand on closer examination are found to be not equally simple. Less obvious differences are often more significant in the end. The Lifton and McDougal rotations, for example, most obviously differ in their construction of the dove factor along existential versus pragmatic political lines, and this difference does not give an advantage of simplicity to either rotation. A less obvious difference regarding the hawks, however, is the tip of an iceberg--a structure, found only in the Lifton rotation, which organizes the data with great simplicity.

As constituted by the Lifton rotation, the non-ideological hawks strongly agree (+3) that "If the U.S. seriously wanted to stop the arms race we could

convince the Russians to accept bilateral reductions," which places them in polar opposition to the ideological hawks, who strongly disagree (-3). In the McDougal rotation, on the other hand, both hawks give the same score of -2 to the statement, thus collapsing the polarity.

A bipolar opposition between the hawks on the issue of negotiation is the key to a tripolar structure which dynamically relates all three factors. This structure emerges when the polarity between the hawks is superimposed orthgonally on the polarity between the doves and the non-ideological hawks on the issue of mass killing. This latter polarity is found in both rotations in the scoring of the following statement: "If leaders are willing to kill millions of people in the name of national security, they cannot be called responsible." On this, doves strongly agree (+4 or +3), non-ideological hawks strongly disagree (-4), and ideological hawks are neutral (0 or -1).

The Lifton rotation, by introducing a bipolarity between the hawks, sets up a tripolar relational structure among the three factors as follows. The doves agree with the non-ideological hawks that we can negotiate, but are in utter, polar disagreement with them regarding the legitimacy of mass killing. The doves disagree with the ideological hawks about negotiation, but can at least communicate with them regarding the legitimacy of mass killing. The hawks agree among themselves that the U.S. needs to keep up the arms race, but for conflicting reasons. All these relationships, while implicit in the one data set, are only constituted by the Lifton rotation.

The differences between the two rotations regarding this tripolar structure are shown in Table 1 which gives the alternative factor scores produced by the Lifton and McDougal rotations.

In summary, this case study provides evidence that factor solutions arrived at by theoretical rotation are replicable by independent observers. Deviations from perfect replication occur, however, corresponding in part to differences in the theories used to guide the rotations. In such cases, a single rota-

TABLE 1
Comparison of Factors

	Statements* A	B
Lifton Rotation		
doves	+4	+2
ideological hawks	0	-3
non-ideological hawks	-4	+3
McDougal Rotation		
doves	+3	+3
ideological hawks	-1	-2
non-ideological hawks	-4	-2

*A. If leaders are willing to kill millions of people in the name of national security, they cannot be called responsible.
B. If the U.S. seriously wanted to stop the arms race we could convince the Russians to accept bilateral reductions.

tion can often be selected if it yields an advantage of simplicity over any of the alternatives. If no alternative can be preferred on the grounds of simplicity, however, other criteria will be employed. In the interests of sound methodology, these criteria should be made explicit.

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COMMENT BY WILLIAM STEPHENSON

A recent letter from a correspondent in Israel asked for references to papers which deal with Q's methodology, as distinct from papers which merely use Q technique. Brian D'Agostino's is such a rare event, even though it limits itself to "replicability at the point of rotation." The outcome is a happy one for Q. But suppose it had been otherwise, that the rotation was not replicable: Would all be lost for Q?

The answer is no, because there are two very different kinds of "statements" involved in advancing knowledge, one is "statements of fact," and the other "statements of problems." The one concerns discussions to establish the truth or falsehood of facts. The other is for discussions to explore the range of meanings, the variety of facts, to which the "statements" apply. It is a distinction made by the late Professor Richard McKeon in a paper entitled "Scientific and Philosophic Revolutions" (1967), and lies behind the opinion of many that Q is most useful for arriving at hypotheses, not for testing any. The search in D'Agostino's paper is with respect to "statements of fact," for testability and falsifiability. It is certainly a way in which knowledge advances, though never with complete certainty; and, besides, the facts may be purely categorical, that is, "structural information" (MacKay, 1969).

Factor structure, involved in the rotation problem, serves the second of McKeon's purposes: Factors present *problems* for our regard, and this is of primary significance in the advance of knowledge. If two researchers arrive at different solutions in rotation, it could be for different "statements of problems," both of which may be worth pursuit.

Thus, in my paper "Methodology of Trait Analysis" (1956), it was shown that R.B. Cattell's solution to the rotation problem in his experiment (resulting in

twelve factors, after the equivalent of one man-year of calculations) could be matched by a different solution, resulting in five factors only, requiring only a few hours for the calculations. My solution was predicated on the statement of a problem: All of the traits used by Cattell were for temperament, except one, for aesthetic sensibility; I argued that this, aesthetic sensibility, was likely to be largely cognitive, not temperamental, and therefore would be orthogonal in the rotation to most of the traits under investigation. But I also had a sophisticated theory at issue, about "reductive" inference, and a formal model which could detail how *Roget's Thesaurus* could be explained. On these grounds I provided a solution to the rotation problem totally different from that reached by Cattell.

I regard my "Methodology of Trait Analysis" as one of the most significant of my papers, not because I expected anyone to believe in the truth or falsity of my theory as to how language proliferated, but because it held within it the core ideas for advancing knowledge along theoretical lines, in terms of "statements of problems."

What, then, is a "statement of problem"?

Factors are admissible in Q if they are operant, and this is often provided by a varimax solution for centroid factor analysis. This is due in part to the averaging procedures used, and to Q samples designed on Fisherian "balanced block" lines. Added to this, however, there are two subjective criteria for operancy, as to how far the factor structure is *schematical*, and how far each factor array has one *feeling-state* running through it from one end to the other. The researcher's aim is to reach an *understanding*, not facts of matters.

The concern with "statements of problems" takes several forms in the above connection. It can replace the use of questionnaires applied to large numbers of respondents when the purpose is to see "what goes with what." Thus, with respect to Buchanan and Cantril's study on *How Nations See Each Other* (1953) in which 1,000 persons in each of 14 nations were surveyed, I used nine subjects each performing one Q sort.

Duly factored and rotated, the result was what I had anticipated, that the most obvious difference between men and women in this country was that, given the choice, women were committed to peace, and men to war. Buchanan and Cantril had missed, in the density of their data, a matter of primary significance. My purpose was simply to qualify an understanding I had reached from several earlier studies, all pointing in this direction. From their study, Buchanan and Cantril could only recommend the obvious--to give people of the world a sense of security and independence, and to improve communication facilities all over the world, so that everyone could act in the common interest (all to be achieved by UNESCO for which body the study had been undertaken--and we now know to what ends this advice has led that body). My advice would have been to foster commitment to international well-being by way of women's commitment to peace. It is all very complicated, no doubt, but chapter 9 of my *The Play Theory of Mass Communication* (1967) is well worth re-reading in this connection.

In short, one is not exactly looking for facts, to be proved or not, but for development of a concept, which might involve years of further experimenting. In this endeavor one treats factors, and factor structures, as statements of problems still to be tackled.

There is also a precise form of "statements of problems" in any "single case" study, for example, in judging whether a person's actions in an incident have been ethical or not. Now one would design studies to bring possible lawfulness into the situation: Could James' law be at issue, and Rogers', and the rest? The rotation problem then depends upon prior suppositions, known to the experimenter. Nor can the suppositions be formalized, as hypotheses to be tested in the *apriori* manner of the hypothetico-deductive framework. The subjectivity of the experimenter's *understandings* is always at issue.

In none of the examples I have used is replicability of a solution to the rotation of factors a purely objective matter. But neither is it without necessary and sufficient controls--one cannot take liberties with data. There are reasons for what one does

in rotating. And each study has a measure of control within it: Thus, in my form of the Buchanan and Cantril study, the Q sample was composed of the questions used in their questionnaire.

Brian D'Agostino may well still be puzzled, wondering whether I have done anything to upset his quest: My point is merely that the be-all and end-all of studies for the advancement of knowledge is not in fostering "statements of fact," even though this is by far the most prevalent way in which current science functions, especially in the psychological and social sciences. The special possibility for Q is in the other direction, a quest for concepts of importance.

This leaves open the door to much that is subjective to the investigator, ultimately to his or her understanding entirely. But this is not to pander to "whims and wishes": To the contrary, it involves every trick of the scientific and methodological game. One does not play footloose with data. In the final analysis, however, one lives by Moritz Schlick's (1935) dictum:

> If anyone should tell me that I believe in the truth of science ultimately because it has been adopted "by the scientists of my culture circle," I should--smile at him.... I assure you most emphatically that I should *not* call the system of science true if I found its consequences incompatible with my own observations of nature, and the fact that it is adopted by the whole of mankind and taught in all the universities would make no impression on me. (pp. 69-70)

This is not an imperious demand for one's own concepts, but testimony to long-continued effort to develop them out of the intransigencies of nature. An example of what I have in mind is provided in essays by Freeman Dyson, in *The New Yorker* (of February 6, 13, 20, 27, 1984) on the subject of "Nuclear Weapons." Dyson, physicist, expert on nuclear weapons, from a life-time of involvement in the military scene and clearly a year or two to write elegantly on his sub-

ject, set out to do what he could to ameliorate the current Iron Curtain impasse on nuclear threat to the world. He provides a concept, "Live and let live," to be set against existing concepts, of Assured Destruction, Limited Nuclear War, Destruction Unlimited, Non-violent Resistance, and Counterforce. He would have added Freeze, but eliminated it because it involves actions, and not the conditions of elaborate diplomacy and treaty-making inevitably at issue. He chose a profoundly-important matter to which to address his special knowledge of weaponry. His "Live and let live" is redolent of what was expressed in *The Play Theory of Mass Communication*, for example in chapter 8. It is to such possibilities of concept formation, in terms of our special knowledge in subjective science, that we have to attend. And the way to this is the way of "statements of problems."

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Stephenson expands on the concept of "statements of problems" in the first in a series of *Perspectives on Q Methodology*, beginning with the next issue. Subsequent *Perspectives* will include "Monistic Protopostulate of Communicability," "A Creative Nexus," and "Behavioral Worlds."
