

Subjective Science: Normal and Abnormal (Continued)

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ABSTRACT: Q methodology's role and status is appraised in light of the 12 intervening years since the keynote speech carrying the same title was read at the first (1985) Q conference. The pervasiveness of subjectivity is stressed, as is Q's unique role in measuring and conceptualizing it. Evidence is provided that Q methodology has achieved certain characteristics of a normal science (as defined by Kuhn), and an inventory is made of conceptual errors and of the kinds of resistances to Q's implications. Summaries are also provided of the variety of projects in which Q methodology has a central role. The conclusion is reached that Q remains outside the mainstream, but that there have been marked improvements in its status within the past decade. These comments constituted an address to the Thirteenth Annual Conference of the International Society for the Scientific Study of Subjectivity, Syracuse University, October 23-25, 1997.

Well, as I was saying, our goal is to bring subjective science into the realm of normal science; that is, to transform it from an oddity, an abnormality, into the normal way of studying humankind. The idea of a normal science we of course owe to Thomas Kuhn (1970), who defined it as "research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further

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practice" (p. 72). And these scientific achievements, Kuhn went on to say, shared two essential characteristics: (1) They were of sufficient significance as to have attracted a community of followers away from alternative pursuits, and (2) they left problems behind for that community to resolve.

Those of us assembled here tonight constitute the core of that community, which we formally institutionalized as the International Society for the Scientific Study of Subjectivity. This we did at 8:40 on a Saturday morning, during a business meeting held at the University of Missouri School of Journalism on October 28th, 1989—which will be eight years ago this coming Tuesday. Many here tonight were present then when we unanimously selected Don Brenner as our first Chair and adopted *Operant Subjectivity* as our journal. I don't know that we could all say that we have been attracted away from alternative pursuits in any *substantive* sense, as Kuhn required: The psychologists among us are still interested in the problems of psychology, the political scientists are still interested in politics, and the journalists are still interested in journalism. On the other hand, I think it's safe to say that once we were introduced to Q methodology, from that point onward the way in which we conducted our professional lives in pursuit of our substantive interests probably was permanently altered.

And what problems have been left behind for our community to resolve? Take political science. In his "Foreword" to my book on *Political Subjectivity*, Stephenson (1980a) wrote that "all studies of political theory in the past are now subject to recall, like the thousands of automobiles recalled every year by their manufacturers, because there was no way, up to now, to qualify the subjectivity invariably at issue in them" (p. xi). Were political science to take this challenge seriously, it would have its work cut out for it simply going around and qualifying the subjectivity that has always been involved, but has heretofore been neglected—in voting, decision making, conflict resolution, interest articulation, and all other forms of political goings-on.

Now, in issuing his challenge to political science, it was not Stephenson's intent to exchange all existing political studies with the same studies done over again but this time with Q sorts. His point was that science heretofore had marginalized subjectivity and had shunted it off to the side, due largely to an inability to deal with it in any rigorous way. But with Q methodology it is now possible to provide precise measurements so that subjectivity can now be brought back in and made a part of what is normally taken into consideration. There was a time when conventional medical practice did not require blood

tests—the theory and technology were just not there—but now blood-work is a normal part of appraisal. And there was a time when astronomy did not take velocity into account when measuring the timing of events, but since Einstein this is now an ordinary part of calculations.

One of the immediate problems left for us to resolve, therefore, each in his or her own substantive field, is to qualify the subjectivity involved in all pursuits in which it has heretofore been neglected. Almost everyone throughout history has agreed that subjectivity is important—for example, that values matter in decision-making, or that how a child *thinks* something works is an important consideration in teaching that child how something *really* works, or that *how* medical care is delivered depends in part on the character and outlook of the care-giver. But it has only been in the past half century that we have learned how to get a handle on subjectivity in a measurement sense. With Q methodology, we now know how to do this, and so it is now incumbent upon science to bring subjectivity back into the fold so that science can proceed on its normal way with subjectivity included rather than excluded.

Q and R methodologies represent fundamentally incommensurate ways to examine different aspects of human conduct. On the one hand there are those things that are objective about us in a certain sense: our intelligence, for instance, or our ego structure, or our memory for a string of random digits. Either I can solve a block puzzle within a minute and a half or I can't, and that's that, and no amount of wishing on my part will help me go faster. In a certain sense, my IQ is independent of my desire.

On the other side of the coin is my subjective life, and here we have to be very careful because there are all kinds of intellectual vested interests that are bound and determined to misunderstand us on this point. The term *subjective* is much more in vogue now than was the case 20 years ago, but it's been stretched to cover all manner of mental conditions, such as biases, feelings, predispositions, and so forth. But in almost every case in which a social scientist is using these terms, reference is being made—at least implicitly—to inaccessible mental events thought to be responsible for our attitudes and behavior.

This is absolutely not what Stephenson had in mind when he invoked the term *subjectivity*. In fact, it was just the reverse: Not mentalistic processes, but communicability captured his interest. People communicate, to themselves as well as to others, and for the most part all of this communicating is on the surface for the scientist to measure and

observe. And the fact that it is *I* who am communicating is what makes it subjective: *My* communication is from *my* standpoint, not *yours*, and I am therefore uniquely positioned to provide a measure of that subjectivity, which is why the Q-sort cards have to be handed to *me*. An external observer, such as a psychologist, may try and second-guess what my point of view is. This, in effect, is what the survey researcher does when providing me with a set of statements that are presumed to carry liberal, conservative, or some other kind of meaning: It is assumed that if I were to utter these statements on my own, this would be because my viewpoint is liberal or conservative, and this in turn would be because other liberals and conservatives in the past have said these sorts of things. Now, the researcher might be right, but it is apparent that all this sitting back and speculating is no royal road to understanding my point of view, especially if I am standing right there in front of the researcher and am capable, with perhaps a little bit of assistance, of providing it myself. It is just not sound science to speculate when direct interrogation is entirely possible.

The idea that we have to approach human subjectivity indirectly—e.g., by asking a person to respond to something, like a scale, and then, on the basis of the response, making shrewd inferences about what is going on inside—this strategy is a carryover from the objective sciences. A physician, for instance, might ask me if I have been feeling more tired lately, or about the color of my urine, or about my eating habits, and this information might be supplemented with laboratory testing of one kind or another. Based on my responses, the doctor may start putting two and two together and conclude that I am hypoglycemic or that my pancreas is malfunctioning.

But facts such as these have little to do with my viewpoint about anything. My blood sugar is above or below normal readings, or I am consuming too much starch, or I am getting too few anti-oxidants and that's just the way things are. And if the psychologist wants to know if I am depressed in the same sense that I am hypoglycemic, I might be given the MMPI or the Beck Depression Inventory in the same way that I would be given a blood test. This is the model of science in its objective mode, and so it was only natural that when science started turning to matters such as self and subjectivity that it tried to deal with them in the same way, which was the only way it knew how—i.e., indirectly, from observed effect to a presumed antecedent cause.

Subjectivity, however, is not quite like hypoglycemia or even depression, both of which are technical terms understood only by experts. A person's point of view about something is a different matter:

It is typically about rather ordinary matters, as in Pablo Neruda's *Odes to Common Things* (1994), in which he bursts into verse about scissors, salt shakers, chairs, oranges, and dogs. This is what Stephenson (1980b) means by *consciring*, which involves shared meanings, hence requiring no special training or knowledge; moreover, the only expert is the person whose view it is. The subjectivity is in the viewpoint itself, and Q methodology was invented to provide a basis for the scientific study of that subjectivity as such, and not as mere verbal report on the basis of which to infer objective antecedent conditions.

A pervasive feature of the objective material world is form: In fact, it is hard to imagine in what way matter might exist without having form. Stephenson was quite interested in form, and frequently cited D'Arcy Thompson's book *Growth and Form* (1917/1942; cf. Gould, 1971). Matter has form, but can form exist without matter? This might sound like an airy philosophical inquiry to the typical empiricist, but it is of rather central importance in Q methodology since factor analysis consistently presents us with all manner of forms and structures in the absence of anything material. Although they may not express it in this way, it is bothersome to most empiricists that there is nothing *there*, i.e., nothing material in Q methodology as a basis for the form which the factor analysis documents, and this is perhaps what prompts some to posit brain-based consciousness, cognitive structures, and substantive selves as lurking in the shadows and providing a quasi-material basis for the factor structure.

Stephenson was a scientist, an empiricist—he referred to himself as a positivist—and he wished to base his subjective science on real events: not necessarily material things, but events for which operations could be provided and that could be measured directly. This is why he was so taken by Kantor's interbehavioral psychology, because it provided a conceptual foundation for the study of subjectivity as a purely natural science (Kantor, 1938; cf. Brown, forthcoming [a], forthcoming [b]). A note was found on a night stand next to Kantor's bed the morning after he died, and it reads, "No spirits, wraiths, hobgoblins, spooks, noumena, superstitions, transcendentals, mystics, invisible hands, supreme creator, angels, demons."¹ Inaccessible mental events that could not be directly confronted were unacceptable to Kantor as a basis for any science, and this was true for Stephenson, too. Subjectivity, on the other hand, was not spooky or transcendental, but was entirely

¹Available at the J.R. Kantor website (URL <http://funnelweb.utcc.utk.edu/~wverplan/kantor/kantor.html>).

empirical.

I mention these matters because all too often Q methodology attracts self-styled postpositivists who see in Q an ally in their war against science. Crusaders of this sort would have been surprised I think to learn that Stephenson shared many of their complaints against science, but that he did so because he thought it was bad science and not because he thought science itself was bad. His life-long task was to correct the record. Many postpositivists whom I have encountered are very much the better scientists than the scientists they criticize—they realize that human experience is somehow important and should be afforded the significance which it deserves—but rather than stand and help correct the record, many have taken the easy way out and have fled to found new churches. The result in too many cases has been that science has been left to the bad scientists while those who could have done something about it have washed their hands and declared themselves to be humanists or postmodernists of one sort or another and have taken their revenge by reintroducing spirits, spooks, and other in-dwelling and inaccessible agents of causality. As a consequence, we have the worst of both worlds: Bad science and bad anti-science.

But enough pontificating for the moment. One needn't preach to the choir.

This evening is for enjoying being back among kindred spirits and celebrating this, our 13th annual get-together. It scarcely seems possible that a dozen years could have passed since I stood behind this podium to address this group, which had gathered for the first time in the summer of 1985 at the University of Missouri (Brown, 1985a). The occasion was to mark 50 years since William Stephenson wrote his famous 1935 letter to the British science journal *Nature*. Will and Maimie were in the audience, and I recall being somewhat apprehensive at the thought that he might interrupt me at any time during my presentation and point out to everyone assembled where I had gone wrong. The feeling was like that controlled panic which we have all presumably experienced in dreams in which we are in a crowd without a stitch of clothing and are trying to act nonchalant. Those who knew William Stephenson know what I mean. Whether we were making a formal presentation or merely sitting in the audience, I think we were all somewhat relieved to have Maimie present since she seemed to be the only one able to exercise any kind of control over him.

Those who were present for the second Q conference in 1986 will probably never forget Will's interruption of the banquet speaker—a man named Elias Porter, a long-time friend of the Stephenson family dating

back to Chicago days, and who had come all the way from California to give the banquet address. Dr. Porter was going on about something rather obscure and faintly R methodological when Stephenson interrupted and challenged him on some point I've now forgotten. My adrenalin shot up in unison with everyone else's I'm sure, and so my memory is sketchy, but I do recall part of Will's argument being that Americans had so many counselors and psychotherapists, way more than would ever be found in England. To which Maimie reminded him, in that tiny voice of hers that could nevertheless be heard like an oboe rising above a symphony, "But Will, it's such a small country." That, of course, brought the house down, relieving everyone except the distraught speaker. I have never been totally clear as to whether there was any connection, but poor Elias Porter died within a year, and ever since I've carried an irrational twinge of guilt that we were somehow partly to blame. On the other hand, I remember Wendy Stainton Rogers remarking that we all recognized that what Porter was saying was nonsense, and that while we were all busy being polite, Will quite properly interrupted to confront the nonsense. How I miss that intellectual hyperactivity and uncompromising honesty. I think all miss it who knew him, and we benefited enormously from having been exposed to it.

He died three years later, in June 1989, a few months after our fourth Q conference and just two months following a Q conference in Reading, England. It was a heavy blow to us organizationally, although our Society was formally instituted in part as a reaction to that loss, and for that we can be grateful. But we survived the doldrums and were able to "get on with it," as Will might have said, and there are many people here tonight—in spirit if not in body—who are responsible for helping us get on with it. I would mention at least two of them:

- First, our honorable host and program chair Dennis Kinsey, whose interest in Q methodology goes back 20 years. In his wake are converts not only at Syracuse, but at Stanford, Boston University, and Decision Research Corporation in Cleveland, where he toiled before taking his vows of poverty and returning to academia.
- Second, I would mention our debt of gratitude to Dan Thomas for having taken over the reins of *Operant Subjectivity* and bringing it back on schedule from the publication backlog which he inherited. Those who are unfamiliar with the time constraints imposed on faculties in small colleges which emphasize the teaching mission will not fully appreciate Dan's accomplishment.

There are, of course, erstwhile I4S presidents and program chairs: Don Brenner, Al Talbott, Rob Logan, Irv Goldman, Karen Dennis, Bob Mrtek, and many others, all of whom have put in many hours hosting, mentoring, and bringing many new faces to our meetings. And there are many of you who have loyally attended over the years and have contributed stimulating presentations to our proceedings, and all of this has added strength and vitality to our organization and has kept people coming back for more. This is the stuff of which traditions are made, and it has contributed to the transformation of Q from an abnormal to a normal science by providing continuity and clarity.

Every science needs its geniuses—its Galileos, Newtons, and Einsteins; its Freuds, Skinners, and Spearman. These unusual individuals are somehow able to see through surface impressions to deeper realities beyond, but they only come around once in a great while, and the insights which they provide are invariably troubling to the world. And these troubling ideas would remain abnormal and generally unacceptable were it not for those who are capable of understanding the insights and of making them comprehensible to others. Stephenson was of course the genius of our science: How subjectivity could be rescued from the realm of speculation and placed on a scientific footing was his insight, and I daresay that none of us in this room could have innovated Q methodology in a way even remotely approaching the way in which Stephenson did. By the same token, his idea of a subjective science would likely have remained largely stillborn had it not been for the precious few in this room who were fast learners, who grasped the insights even though they could not have come up with these insights on their own, and then were able to explain these central and oftentimes difficult ideas to others. It is this ability of ours to recognize something important in advance of everyone else that serves to constitute us as the kind of community which Kuhn said was indispensable for a normal science.

But what now seems normal to us is still considered a curiosity in some circles, and there is always a built-in resistance to abnormal ideas, to ideas that do not fit in with what people already know. Nothing is quite as counter-intuitive as the proposition that the earth moves: Our unaided senses do not support this. Were the earth actually moving, we would expect the surface winds to be constantly turbulent, like when you stick your head out of the window of a moving car, only several hundred times worse. If the earth is spinning, then why isn't the centrifugal force throwing us off the planet and out into space? Initially, Copernicus' idea of a moving earth was experienced as abnormal. But technological advances, especially the telescope, extended the senses

and exposed them to new influences, and eventually humankind adapted to Copernicus' idea and it came to be experienced as a more and more normal way of thinking.

Stephenson's Q technique is our telescope, and through its use we are able to see things in different ways that others experience as abnormal. Galileo faced the same difficulty: When we look at stars, they appear to twinkle, and so Galileo had to explain why it was that stars did not twinkle when viewed through his telescope. His critics were using this fact as evidence against Galileo's findings and against the Copernican hypothesis. As it turned out, the twinkling of stars was not something intrinsic to the stars, but was due to the refraction of starlight in the moisture on the surface of the eyeball: It was the naked eye that was providing the distorted information (for details, see Brown, 1985.) The same thing applies to Q methodology: We often see things that others do not, and so have to resolve whether what we see is correct in some sense, or merely a function of our measuring procedure. In this regard, I remember a panel critic once commenting that the high correlations which we reported could only be obtained at Kent State University, which was a clear expression of methodological incredulity.

The reaction to our endeavor has sometimes been hostile: Those who rely on Q methodology are sometimes referred to as hero-worshippers (Stephenson, of course, being the hero), or as obsessed with method. At least we don't have to wait until we die before we dare publish our findings, as Copernicus did, nor do we have to face being burned at the stake. Galileo considered being burned at the stake to be a great annoyance, and so he recanted. And even though these kinds of annoyances are a thing of the past, the 20th century has produced annoyances of its own.

But there is evidence that the tide is beginning to turn, and that Q methodology is being extended greater respect and is gradually achieving the status of normality. I received a phone call a year or so ago from a researcher at Harvard Medical School. She had apparently written a paper using Q technique, but the journal to which she had submitted it had turned it down because, the reviewer said, she had not used Q technique properly, and it was suggested that she contact me. I have no idea who the reviewer might have been—perhaps someone here tonight—but voluntarily suspend your disbelief for a moment and visualize this situation: Here is someone in the Psychiatry Department in Harvard's famed medical school calling a political scientist at Kent State University for advice about how to get her paper published. I have no idea whether she ultimately succeeded, but even if she did I'm

sure I got more out of our phone conversation than she did. I gave up smoking more than 10 years ago, but following this phone conversation I was sorely tempted to light up a cigar and puff contentedly while watching the sun go down. It was a moment pregnant with meaning, and there have been others since. And I am sure we will experience additional moments such as this in the years ahead.

From this incident, by the way, we can see why it is that abnormal sciences are so often resisted: They typically turn things upside down and otherwise threaten the status quo. Q methodology in particular can be viewed in this way because its topsy-turvy world is in a certain concrete sense the very opposite of convention: In Q methodology, we correlate persons instead of tests (Stephenson, 1935), and this is widely understood to mean that the world as it is conventionally known is turned on its head, with the rows of data matrices becoming columns and columns becoming rows. This is apparently tantamount to telling the churchmen that the earth is not at rest, that the way the world works is the very opposite of the way it seems to work, and this of course startles the horses.

But as we all know, Stephenson never said that in the world of Q methodology, the rows become columns and the columns become rows: This is a myth and a gross distortion of what Stephenson actually said, and since he went out of his way to say that this was definitely *not* what he meant—this appears not only in *The Study of Behavior* (Stephenson, 1953) but in several other of his early publications, as well as in the writings of a few others who managed to catch on—since he repeatedly denied that this was his viewpoint, we cannot help but ask why this falsehood has gained such wide currency.

I for one do not see this as particularly mysterious. By holding onto the view that Q is merely the transpose of R, R methodology was able to retain its world pretty much intact: It was merely rotated 90-degrees so that the rows became columns and the columns rows. But it was still the same world: It was still the same familiar data matrix, only turned on its side. Cyril Burt's (1972) reciprocity principle—to the effect that Q and R factors are merely different routes to the same reality—was simply a last-ditch attempt to normalize Stephenson's innovation in the sense of retaining it as a part of normal science, thereby saving the old reality. Einstein did the same: His special and general theories of relativity to a certain extent kept the old Newtonian universe intact and made it more comprehensible, but when quantum theory suggested an entirely different kind of reality, Einstein could not accept it, and it is for this reason that he is often regarded as the last of the great

classicists, and Neils Bohr as the first of the modernists.

There is another reason why R methodology has a vested interest in continuing to conceive of Q methodology as simply an exchange of rows and columns of the same data matrix, and that is to be able to retain Q as a straw man which can be easily ridiculed. For instance, by holding to the view that Q and R are simply reciprocal matrix transpositions, R.B. Cattell (1978) can write knowingly about "The Misspent Youth of Q Technique," which is the title of a chapter fragment in one of his many books, in which he discredits Q for its many defects, none of them salient. In this way, Cattell can easily dispense with Q as trivial, but he would no doubt be surprised to learn that Stephenson was in agreement with his assessment of "Q technique," that is, Q technique as Cattell understands it: Stephenson once commented to me that so-called inverted factor analysis was good for practically nothing. Incidentally, Stephenson and Cattell were contemporaries at the University of London in the early 1930s, but Cattell chose to defend Burt's position. On a couple of occasions when I was editor of *Operant Subjectivity*, I invited Cattell to summarize his views about Q, but he exercised good judgment and never responded.

Or consider what Peter Gould (1985) took, at least initially, to be his devastating critique of Q methodology. Gould was a professor of geography at Pennsylvania State University, and he regarded Q-factor analysis as merely a degenerate form of some other mathematical procedure: Q forces linearity on the data, he said, imposes orthogonal structures, and generates an artificial text for interpretation; moreover, he seemed to detect in Q some kind of throwback to Descartes' distinction between objective and subjective. Gould's critique was skilled and deadly, but it didn't have anything to do with Q methodology as we know it; and when this was demonstrated to him, he quietly rode away into the gathering twilight, never to be heard from again.

The demonstration, incidentally, consisted of a Q study in which the views which Gould had expressed in his article were included with the views of others; this Q sample was then used to represent Gould's standpoint for contrast with Q sorts representing Stephenson, Burt, and some others. (For details, consult Brown, 1985b.) The demonstration cannot have helped but reveal to Gould that the Q methodology which he had just seen demonstrated was not the same as the Q technique which he had just criticized—and this probably explains why we never heard from him again.

Or take the use of Q-factor analysis as used in the study of organizations by Danny Miller and Peter Friesen of McGill University (Miller

& Friesen, 1984). In conventional fashion, Miller and Friesen asserted that "Q technique is merely R technique using a transposed raw-data matrix" (p. 47); and, as is also typical, they cited Stephenson in support of what they had done. When this error was pointed out, Miller (1985) took umbrage and commented that he and his co-author had not misunderstood Stephenson, but instead had decided to use a variant of Q—but his so-called "variant of Q" was what we would recognize as the more conventional inverted factor analysis in which the matrix of objective scores was transposed and reanalyzed. A variant of Q, indeed! This is the height of misunderstanding. In the course of his exposition, incidentally, Miller explicitly aligned himself with numerical taxonomy, hypothesis testing, and the works of R.B. Cattell. As the saying goes, there are none so blind as those who will not see.

These old stories could be supplemented by many more recent ones, but the point of retelling them is not to reminisce, but to serve as a reminder of the strength of convention. In the world of intellectual exchange, the default condition is set on *slumber*, and no one appreciates being roused from a somnambulant state.

The Intellectual Establishment is completely outfitted with devices for protecting itself, and ignoring or purposely misunderstanding the tenets of Q methodology is only the most frequently employed means. At least two others suggest themselves, and one of them is simply to dismiss Q as at best an interesting curiosity. This is the *power response*, for only the powerful can summarily dismiss without being held accountable.

I encountered this response first-hand recently when a decision was made in my department no longer to permit the offering of a graduate seminar on Q methodology. (I might add that this decision may be reversed, but for the moment it stands.) Our department had recently taken the bold move of refocusing the graduate program on the area of public policy, and the ostensible reason for the decision to remove Q methodology from the curriculum was that it was not in the policy mainstream. This is of course true, but if attention is directed away from the mainstream and toward the cutting edge, the situation changes dramatically, for there is all kinds of evidence that Q is increasingly being utilized as a means to gain leverage on pressing public problems. Allow me to parade a few recent examples in review.

First, I would mention studies conducted by Marten Brouwer and Michel van Eeten, who are in the audience and on the program for this conference: their studies are on the public's views about new and expanded airport facilities in The Netherlands. Marten Brouwer reported his results this past month at a meeting of the World Associa-

tion of Public Opinion Research in Edinburgh, Scotland (Brouwer & Binnendijk, 1997). Michel van Eeten (1997) reported his results on a panel this afternoon and has been invited to present them again next month before a policy group at the U.S. Federal Aviation Administration in Washington. Q methodology is profitably utilized in this instance to reveal public sentiment with regard to this issue. Incidentally, one of the reasons Michel van Eeten has been invited to Washington is because of an FAA policy analyst there by the name of Ann Hooker, who is also here this evening. Ann presented the results of her study at the I4S meeting in Chicago a couple of years ago, and she authored a fine policy-related dissertation on forests at Yale University in 1992 (Hooker, 1992). And I might add a further coincidence: Ann Hooker's dissertation is subtitled "A Framework for Listening"; and I hope I am not letting the cat out of the bag by noting that a forthcoming manuscript by Michel van Eeten is tentatively titled "Dialogue of the Deaf in Public Policy." The idea that what public policy requires is more sensitive listening and that Q methodology helps us hear better is an easily overlooked concept in policy making.

Q methodology is the centerpiece of a \$1-million-plus grant to examine public support for brownfield redevelopment in Oklahoma. Brownfields are those areas decimated by past hazardous-waste disposal practices, and the cost of reclaiming these areas is substantial. Are people willing to pay the cost? Geologist Will Focht of Oklahoma State University has attended previous Q conferences, and he is the point man for this project, preliminary results of which were presented a year ago at a meeting of the Association for Public Policy Analysis and Management in Pittsburgh (Focht & Lawler, 1996). Will Focht's paper was entitled "The Use of Q Methodology in Policy Dialogue," and it shows the value of Q in helping to clarify the perspectives of major stakeholders on an issue. Unless all significant decision making groups can be included in a problem's solution, and unless some basis for consensus can be found, it is doubtful that major problems can be solved. Q is of course ready-made for the clarification of perspectives and for locating bases for consensus, if they exist.

Similar studies are in-progress or in various stages of completion elsewhere in the world. Let me quickly mention a few:

- Bruce Bratley (in progress) in the Geography Department of the University of Surrey is completing a Q dissertation on the problem of solid-waste disposal in England. In this study, it has been discovered that the environmentally-popular concept of recycling has sometimes gotten in the way of public acceptance of solid-waste burning as capable of

producing more and cheaper energy.

- In e-mail correspondence with Sarahi Angeles Cornejo of the Autonomous University of Mexico I have recently learned of another pocket of policy analysts who are actively involved in probing the views of citizens and stakeholders concerning Mexican climate policy. Again, Q methodology is at the heart of a project that is apparently nationwide and is apt to be utilized as input into national decision making.
- John Dryzek of the University of Melbourne is the coordinator of a multi-national effort to probe emerging national identities of Eastern European countries which were, until recently, under the domination of the former Soviet Union. The ways in which nations conceive of themselves are significant features of the policy process inasmuch as they can facilitate or impede formal policy initiatives. A separate Q sample has been fashioned for each of several countries singled out for inspection—the Czech Republic, Poland, Hungary, and others—and a national coordinator has supervised its application to a P set representative of significant groups in each country. Within the next year, John Dryzek will meet with his coordinators for a comparative audit of the new national self concepts that are currently being formulated.
- Not far from here, at Cornell University, a group of researchers in the Division of Nutrition Science are coordinating a massive project on food security in six northern New York counties. This initiative is being led by David Pelletier, Vivica Kraak, and others, and Q is again center-stage and is being used to provide clarity as to the main issues dividing and joining participants of diverse perspective.

In addition to applications of Q to matters of immediate policy consequence, efforts are also going forward on the more theoretical aspects of policy. Dan Durning of Georgia's Vinson Institute of Government is taking a leading role in this—for example, in his essay on "The Transition from Traditional to Postpositivist Policy Analysis: A Role for Q Methodology" (Durning, 1996), which he read at last year's annual research conference of the Association of Public Policy Analysis and Management. Dan has been committed to introducing various post-positivist concepts and practices into an overly-econometric field of policy analysis, and, with the help of Q, has begun sensitizing practitioners to the role of values in policy (Durning & Osuna, 1994). Along with co-workers Natalia Gajdamaschko, Valeriy Bebyk, and Sally Selden, who is with us this evening (having just moved to Syracuse), he has also been slipping some of these ideas into the emerging policy sciences in the Ukraine (Durning, Gajdamaschko, &

Selden, 1997). The connection with Valeriy Bebyk is particularly fortunate, incidentally, since Valeriy has a position within the Administration of the President of Ukraine.

I could go on, but I hope it is clear that there are now too many policy initiatives which have been launched for those who might be tempted to dismiss Q to head it off; moreover, once the results start pouring in, enthusiasm cannot help but wax. Q methodology holds a mirror up to individuals, groups, and societies and provides them with a clear reflection of themselves, and there is something very uplifting and liberating about that. About three or four years ago, my bedraggled and war-torn department got together in a brain-storming session, generated a lot of ideas about steps that might be taken to improve our lot, and then had the wisdom of putting these suggestions into a Q sample for Q sorting by the faculty. I happened to be out of town at the time and so cannot be accused of having instigated this, but the upshot was startling: Three quite different and in many ways incompatible factors emerged, but a few topics of consensus did also. The surprised faculty, which had been unable to cooperate on anything for several years, immediately passed three pieces of legislation associated with its new-found consensus, and without a single dissenting vote. Such dramatic results cannot be promised every time, of course, but they are often within reach and merely require something like Q methodology to make us aware that they exist.

I began by noting that there are three main ways in which the Intellectual Establishment can avoid having to confront the tenets of Q methodology, as a way to keep things normal and to protect itself from the abnormal—the first being to ignore or purposely misunderstand Q, the second being simply to dismiss it (e.g., by declaring it to be outside the mainstream and deserving of no further consideration).

I would like to move toward closure this evening by considering a third intellectual defense mechanism: *rejection through partial incorporation*. This is a phrase that was introduced by political scientist Harold Lasswell (1963, p. 151), and it refers to a way of restricting the impact of a practice or an idea by adopting some part of it (and usually some trivial part), and thereby creating the impression of having embraced it. Transposing a data matrix, correlating and factoring persons, and calling that *Q methodology* is, of course, a sterling example. Stephenson's central idea was in many respects revolutionary, and by adopting some superficial and peripheral aspect of it and then citing Stephenson as if his approval could be vouchsafed creates the impression of having accepted the main idea without actually having to

do so. Through partial incorporation, the whole can be restricted by accepting a part.

Several examples can be cited. To begin with, there was considerable discussion on the Q-Method network a couple of years ago about the so-called "California Way" of using Q technique. The California Way was so-named because its main practitioners were either at, or passed through, the Psychology Department at the University of California-Berkeley, the central figure being Jack Block (1961). The crown jewel in this intellectual tiara is the California Q Set, a standardized Q sample comprised of 100 items. Q sorts have been used at Berkeley for decades, and Stephenson has been religiously cited as the person who inspired it all, but little of what Stephenson was promoting is to be found there. The standard operation of the California Way is for the psychologist to use the Q sort to describe a patient—i.e., the psychologist does the Q sorting, not the client. The vantagepoint of the observer is therefore always from the outside. Now, it's not that Stephenson never utilized Q technique in this way: On the contrary, he left behind several examples (e.g., Stephenson, 1950). But this is only a single application drawn from Q's wider array of possibilities, and a relatively minor one at that, and it is surely a half bubble off from Q's main value as the basis for a science of subjectivity.

The "Minnesota Way," as we might christen it, refers to two unrelated but in many respects similar applications of Q technique. Among the political scientists, either at the University of Minnesota or who have graduated from there, there is a marked tendency to use Q technique to derive categories which are then entered into a large-sample operation, such as a survey (e.g., Theiss-Morse, Fried, Sullivan, & Dietz, 1991). Similarly, those Minnesotans in communication studies also strive for large samples, often gathering Q sorts by the hundreds for purposes of averaging and making inferences to the larger population (e.g., Cragan & Shields, 1981). It is very easy to spot Minnesota-inspired manuscripts when they are received for blind reviewing: They have a kind of birth mark.

The "Colorado Way" is associated with an application of Q that is centered at the University of Colorado. Studies influenced by this approach are easy to spot due to the fact that cluster analysis rather than factor analysis is inevitably employed (e.g., Brunner, 1983). The theory behind this expressed preference is that cluster analysis is mathematically more precise than factor analysis, which I am quite prepared to believe is true. What is not grasped, however, is that centroid factor analysis is preferred in Q methodology *because* it is

imprecise in the sense of its being indeterminant, and it is this indeterminacy that enables the factor analyst to be sensitive to context and to guide rotations in light of the specificities of context. This contextual sensitivity is missing in the a-contextual application of cluster analysis.

Other examples could be cited, but perhaps enough have been to support the conclusion that the various spin-offs from Q's intellectual center of gravity have arisen due to partial visions which have been mistaken for the whole, or which have been assumed to be compatible with the whole—even more, that the various partial visions have helped insulate certain users from the reverberations that might result from contact with Q's central insights. It is to be noted that with the sole and significant exception of Jack Block, not one other devotee of the California Way currently subscribes to *Operant Subjectivity*: One used to several years ago, but subsequently terminated his membership in disgust and returned all his back issues of the journal, claiming that he had grown beyond Q methodology and criticizing those left behind as doctrinaire hero-worshippers. And no one from the Minnesota or Colorado camps has ever subscribed, either to the journal or to the electronic discussion list—and all this despite the fact that they use Q technique in their studies from time to time. The resistances are strong.

I would like to close with a quotation from a recent chapter by Angela Febraro (1995), entitled "On the Epistemology, Metatheory, and Ideology of Q Methodology." Febraro states that

... as a device for conducting scientific research, whether positivist, behaviorist, or feminist, Q methodology ... has somehow obtained scientific legitimacy, by having convinced *all* the research "players," of varying epistemological, metatheoretical, methodological, and ideological commitments, of its value as rhetoric. (p. 149)

I'm not so sure about the "rhetoric" part; however, I am in agreement with Febraro that larger numbers of researchers from a wider and more diverse audience are finding something of value in Q methodology, and are increasingly turning to it as the method of choice.

This turn of events has its pluses and minuses. Twelve years ago I addressed this group and warned that in our dimly-lit future, it was quite possible that the falcon would not be able to hear the falconer. I was perhaps too pessimistic. It's true that we have since lost our most accomplished falconer, but the center seems to be holding anyway and things are not falling apart. Realistically, as long as we keep in close contact with our central ideas we should be able to continue to make

inroads and to win greater acceptance of the role of subjectivity in all human endeavors. At the same time, as we slouch toward Seoul, Korea, and our 14th organizational year, we need to be wary of the philistines milling around outside. We need, in sum, to be wary while celebrating, and on this cautious note I end with a toast: To the health of our enterprise, and death to our enemies. Put a bookmark here and I'll pick up on the story in the year 2009.

Thank you.

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