Q Methodology, Textuality, and Tectonics

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ABSTRACT: Along with the paper from Rex Stainton Rogers, and that from Simon Watts and Paul Stenner, this paper seeks to explain and illustrate how and why the Q work we are doing here (mainly in the UK, but also with some colleagues elsewhere) differs from the work with a much longer history which has been undertaken by Q scholars in the USA and their students and protégés (again, this includes work outside the primary geographical locus). While our work has much in common with the latter, there are significant differences both in our approach to carrying out Q studies and in our objectives. Our interest in Q arose from a theoretical location within a "climate of perturbation" framework, drawing from "French Theory." Q met our need for an idiographic methodology, and offered us a highly effective means to conduct discourse analytic work. We use Q to address issues of power and knowledge, and their interplay, using analytics of textuality and tectonics. These terms are defined and an illustration is provided to show how Q can be used in this way.

Introduction

At the recent conference in Durham celebrating the life and work of William Stephenson, it was very evident that the Q work we are doing here (mainly in the UK, but also with some colleagues elsewhere) differs in a number of respects from the work with a much longer history which has been undertaken by Q scholars in the USA and their students and protégés (again, this includes work outside the primary geographical locus), most of whom were colleagues or students of Will

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Stephenson himself. This paper, alongside others in this special edition, examines some aspects of these differences, the reasons for them, and their implications and consequences.

Different Histories

The first thing to note is that the two approaches have rather different histories. The Brits came to Q much later—it took, I believe, until the 1980s before the first UK doctorate based upon Q research was completed by Celia Kitzinger (Kitzinger, 1984). This appeared as a book some three years later (Kitzinger, 1987) and was highly influential in informing scholars in the UK about Q methodology in general, and Stephenson's work in particular. Before that, in psychology at least, Q was seen as a minor aberration of the "correct" use of factor analysis, and generally, where mentioned at all, was misattributed to Butler and Haigh (1954) or Block (1961). Stephenson's work—indeed, his very existence—was virtually unknown in the UK up until then, outside of the dim recollections of a few people who had worked with him before he left for Chicago.

What this meant was that while our approach has obviously been informed by Stephenson's writing and, subsequently, by the direct contact we were able to have with him in the last few years of his life, it was not Will Stephenson's work which led us into Q, but Steven Brown's (1980) *Political Subjectivity*.[1] The timing was crucial, since we got to read it just at the point (the early 1980s) when a lot of other intellectual "moving and shaking" was going on. It is something of a paradox that in the UK, despite the antagonism of the Thatcher government towards the Universities—indeed, towards scholarship generally—it was an exciting time. For those of us who had long felt disquiet about the positivist paradigm which had dominated social science throughout the 60s and 70s it was a time of liberation.

Michael Mulkay (1991), a sociologist of science, has offered a neat analogy where he describes the course taken by the relationship between science and social science in terms of a "love affair." At first, enamored and bowled over by science's potency and power, social

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[1]Its discovery was sheer serendipity—a chance meeting in a corridor with a colleague in political science, who happened to have been watching an educational television programme in which we had misused Q, and who had a copy of Brown's book to lend us. Celia Kitzinger was, at that time, looking for an appropriate method for exploring lesbian identities and, "the rest," as they say, "is history."

science did all she could to emulate her hero. But slowly she became disenchanted, as it became ever more obvious to her that his dogmatic preoccupation with objectivity and "hard data" made it impossible to engage with what was most salient and interesting about the social world.

The 80s marked, for us, at least, the time at which social science found the courage and the means to liberate itself from its stifling fealty to "hard" science[2], and to begin to carve out for itself a theory-base more suited to its subjects of inquiry. Of course (as Rex Stainton Rogers' paper stresses) this was always a minority movement. In psychology, at least, positivism retained (and continues to retain) its sovereign position and dissent was (and continues to be) strenuously policed.[3] Yet for all that, those of us who had "fallen out of love" (or had never been infatuated in the first place) were able to begin to move out of the too-cozy relationship and "rebuild our lives" afresh—no longer *Stepford Wives*, but dissidents and rebels, living on the fringes and "doing our own thing."

The Climate of Perturbation

European psychology has had a long history of such dissent from positivism, which is evident, for example, in the theorization around social representations instigated by Serge Moscovici (1961)—work almost unknown in the US—which has much in common with Q theorization (see W. Stainton Rogers, 1991, for a fuller treatment of this resonance).

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[2]It is important here to stress that this was not a rejection of science, *per se*. So long as it is seen as technological endeavor directed at informing practical concerns (such as the design of machinery or industrial processes, or assessing the *physiological* efficacy of pharmaceutical agents) science clearly has enormous pragmatic value. We would also accept the definition of science as a "systematic and disciplined inquiry." What was being discarded was *scientism*, the pretence that human concerns, interests and values can be objectified, measured and judged just as if they were lumps of clay or physical forces like gravity, unaffected by issues of ethics or ideology.

[3]For example, the *Research Assessment Exercise* introduced into UK Universities in the 1980s, favors psychology publications based on positivistic research. Theoretical and critical papers are less easy to get published, and, when they are, count for less. The direct consequence of this is that such work "earns" considerably less money for the University. There are direct consequences for the employment and career development prospects of those who attempt to do alternative work, in psychology departments at least.

The major impetus for our own liberation came, however, from what is often, loosely, called "French Theory," encompassing the work of people like Foucault, Derrida, Deleuze, and Kristeva. We have termed this intellectual movement minimally, as a "climate of problematization" (cf. Curt, 1994) or "perturbation"[4] (R. Stainton Rogers *et al*, 1995), rather than, say, "postmodernism" or "social constructionism," in order to avoid the sense that there is some kind of new "Grand Theory" being proposed. Since one of the heretical challenges this movement puts forward is that there can be no "Grand Theory"[5], it seems more prudent to concentrate upon what does hold the various elements together—a troubling of taken-for-granteds; a literal stirring up of doubt and perturbation around the way that social science has sought to gain legitimacy and credibility by adopting a scientized approach.

Taking ideas from this movement, our trouble-making got "teeth," as we gained a language and, through it, a set of potent concepts and analytics, which enabled us to directly begin to pick away at the stranglehold that scientism was holding over social science in general and psychology in particular. Possibly the best known US advocate of this development in psychology is Ken Gergen, who described it recently in terms of "interrelated developments in social constructionism, ideological critique, discourse analysis, ethogenics and social pragmatics," developments which he argues are, in turn, "intertextual with far more sweeping transformations in the academy—adumbrated by such terms as post-structuralism, post-empiricism, and postmodernism." Together, he says, these portend "a new social psychology" (Gergen, 1996, p. 1196). Others (see, for example, Ibáñez and Íñiguez, 1997; R. Stainton Rogers *et al*, 1997) have adopted the term "critical social psychology."

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[4]Our original formulation, the "climate of problematization" was intended to convey the idea that we saw mainstream theorization as highly problematic. We were picked up on this by colleagues at the Autonomous University in Barcelona, who pointed out that the notion of "a problem" (in that it implies that there is a solution) is, itself, inconsistent to postmodern theorization. We have therefore subsequently substituted the term "climate of perturbation," since its implications are more minimalist, inferring no more that there is a "stirring up of trouble and doubt."

[5]Stephenson himself was very disparaging about such theories. In *The Study of Behavior*, for example, he writes: "General theories ... lie about everywhere in psychology, lazy, like cats in the sunshine" (p. 3).

The Attractiveness of Q Methodology Within the Climate of Perturbation

It was within this context that we first read *Political Subjectivity*. We discovered in it a number of ideas and arguments which fitted very neatly into our "problematizing of scientism" agenda. Specifically, we were drawn by Steven Brown's arguments against the way that "social scientists have generally adopted a strategy of conceptualizing attitudes, feelings and other relevant human events as internal states or traits, with certain properties that can only be measured indirectly through devices, such as attitude scales, said to be operational definitions of them" (Brown, 1980, p. 2), and how "[b]y defining ahead of time what a response is to mean, the observer is imposing his will on reality" (Brown, 1980, p. 3). We agreed with his statement that "when a subject responds to a scale item ... the meaning and significance his response has for him may differ in major respects from the meaning assumed by the observer" (Brown, 1980, p. 3) and that "[l]anguage-in-use- is by its nature symbolic ..., with each combination of words being capable of carrying a wide range of meanings. For an investigator to regard his own understanding as in some sense objective or correct is therefore pretentious in the extreme" (Brown, 1980, p. 3).

But what attracted us to *Political Subjectivity* especially was the promise of a method which would allow us to conduct empirical investigation from an idiographic rather than nomothetic perspective. Critical approaches, including critical social psychology, were good at criticism, but rather less capable when it came to empirical enquiry. Such iconoclasm was one of the reasons why many social scientists were less than enthusiastic about the turn to criticality. The discovery of Q methodology, at that precise point in time, was like striking gold. In order to see why it is necessary to look in a bit more detail at the nomothetic/idiographic distinction.

Nomothetic and Idiographic Methods

As Lamiell (1998) has recently pointed out, "nomothetic" inquiry is generally seen to be matter of looking for systematic differences among people, whereas "idiographic" inquiry is seen to focus on individual experience. Cloninger, for example, says of the nomothetic approach that "[g]roups of individuals are studied, and the people are compared by applying the same concepts (usually traits) to each person," whereas "the *idiographic* approach studies individuals one at a time without

making comparisons with other people" (Cloninger, 1996, p. 5, emphasis in the original). Lamiell (drawing on the work of the German philosopher Windelband) argues that this is an over-simplification. The distinction is not between studying populations or studying individuals, but in the nature of the sought-after knowledge.

In this analysis, the nomothetic approach seeks to discover the lawful properties of enduring human qualities which transcend culture and history; to gain objective knowledge about human "essences"—such as intelligence or personality. The idiographic approach, by contrast, is interested in finding out about what people *make* of a particular issue or topic—their opinions, judgements, understandings, and so on—which are recognized as reflecting the cultural, social, and historical contexts in which this knowledge is constructed. This reading of the meaning of "idiographic" fits well with Stephenson's own formulations, where he saw the "single case" as either an individual or "a single group of interacting persons" (Stephenson, 1953, p. 2).

One of the main challenges to orthodoxy of "climate of perturbation" theorizing is that this is a false distinction. *All* knowledge is seen to be local and contingent; that is, a product of its time and of its cultural, social, and ideological location. There is no epistemological superiority of "fact" over "fiction"—both are products of human meaning-making, the representational labor which mediates between what Mulkay (1985) calls "the world" (i.e. the physical objects—including people—and their action and inter-action which constitute the world-out-there in which we humans live) and "the word" (i.e. our knowledge about and understanding of that world). From this perspective all enquiry is idiographic, however much it is purported to be nomothetic. So-called phenomena like "intelligence," "attitudes," and "personality" are as much inventions of human imagination as are opinions and beliefs. Steven Brown encapsulates this elegantly in his analogy between testing for, say, intelligence and telling time:

> The correlation and factor analysis of scale responses leads not to a taxonomy of behavior as commonly thought, but to a taxonomy of tests. ... This misconception might be compared to that of a physicist who, if upon discovering a high correlation between the measurements of his watch and his wall clock, assumes he has measured time. All that he has really shown is that the two measuring devices are related, which says nothing about time. (Brown, 1980, p. 5)

From a "climate of perturbation" perspective, nomothetic approaches are steeped in this misconception. The apparent validity and reliability

of scales of intelligence, personality, attitudes and so on arise not because there are naturally occurring phenomena "out there in the world" that these scales are measuring accurately, but for two simple reasons. First, the scales themselves often consist of asking the same question in slightly different form, over and over again. And second, the samples used for testing comprise people who share a common world-view.

An example here can illustrate the point. In a study of explanations of health and illness (W. Stainton Rogers, 1991), responses to an established scale (the Multivariate Health Locus of Control or MHLC scale) were subjected to both R and Q analyses. The R data confirmed the scale's design properties—responses were divisible between three main "types":

- Belief in *internal control*, viewing health as depending on one's own actions and lifestyle;

- Belief in *external control*, viewing health as outside of one's own control, just a matter of chance and fate;

and

- Belief in the control of *powerful others*, attributing the state of one's health to the actions of the health professionals and others, such as one's family.

But the Q analysis offered a rather different interpretation, as it allowed fine-grained scrutiny of alternative patterns of response. For instance, one factor so identified was notable because it discriminated between responses to items about "chance" (which were all strongly rejected) and items about "fate" (which were all strongly endorsed). The person whose response set most strongly exemplified this factor was a Hindu, for whom these two words had a radically different meaning from each other, unlike the similarity between them generally assumed by the majority US population on whom the scale was originally developed. In other words, the MHLC scale only "works" with a population which shares the world-view of the scale designers. There is no enduring "locus of control" trait which transcends culture and history, and which a nomothetic approach is able to reveal. All that can be accomplished is an idiographic explication of a local and contingent version of common-sense knowledge.

Advantages of Q as an Idiographic Methodology

This illustration and, more broadly, this review of the nomothetic/idiographic distinction should make it clear why it was that the discovery of Q was so exciting for us. Disillusioned as we were with psychology's "wild goose chase" after nomothetic knowledge, we felt we had hit upon a very powerful methodology which would enable us to pursue an idiographic approach, which overcame many of the drawbacks of the methods which were coming to be used within the "critical social psychology" movement for this purpose: either grounded theory methods (cf. Glazer and Strauss, 1967) or discourse analysis (see, for example, Potter and Wetherell, 1987; Parker, 1992). Both of these suffer from limitations arising from the relationship between the participants in a study and the data produced and the researcher. As Kitzinger (1987) has pointed out:

> Research reports of interview studies often describe in detail the social context and conduct of the interview itself, while glossing over the coding and analysis of interviews as a mere technical matter. ... This has the effect of giving the investigator's interpretation of the interviews an air of objectivity and inevitability, as if anyone faced with the same set of interview transcripts would produce a similar analysis. (Kitzinger, 1987, p. 76)

The criticism which is so often directed against "soft" (i.e. qualitative) methods by "hard" scientists is that the data that they generate are simply the products of the interpretative skills and cultural fluency of the researcher, and thus, inevitably, will always reflect the researcher's own prejudices and limitations. Such criticisms usually assume that it is objectivity which is at stake, and the worry is that the researcher's biases may yield inaccurate data. Accuracy, *per se*, is not a problem from an idiographic approach (given that it does not assume there is some "benchmark" of fact that can be accurately or inaccurately interpreted). But what is of concern is that it is the *researcher* who decides what is salient, what are the crucial dimensions along which accounts vary (or understandings, images or whatever the subject of study). In other words:

> The analyst handling interviews or texts select out those which *appear* to be significant when listening to the tape or reading the document. The great danger here is that the researcher making selections will simply mirror his or her prior expectation. In this situation data can be used to simply buttress the favored analytic story, rather then being used to

critically evaluate it. (Potter and Wetherell, 1987, p. 42)

By contrast, while the researcher in a Q study provides the "raw material" from which accounts are to be constructed (in the form of the items comprising the Q set), it is the *participants* in the study who determine the lineaments of meaning, salience, connectedness, and so on. It is their response patterns alone which yield the factors, and it is the factors which determine the pivots around which variability is operationalized. With Q the data are much more able to "speak for themselves" as it were. In a Q study the source text provided by participants is an already organized and articulated whole. In conventional forms of discourse analysis, though, organization has to be imposed by the researcher. He or she has to construct a coherent account of, say, the different discourses in play around a particular topic, out of large numbers of isolated elements dispersed through the text under scrutiny.

Thus while other idiographic methods are ways of "reading" text, Q analysis is more like solving a conundrum. In most Q studies there will usually be factors where the solution is more or less "obvious." What is being conveyed by a sorting pattern is immediately recognizable—one can simply draw on one's experience and cultural knowledge to identify the position being taken, or portrayal being depicted, or whatever. But most Q studies will generate one or more factors which are, at first sight, quite puzzling and indecipherable. Much effort may be required to gain insight into what is being expressed, including going back to the participants whose Q sorts were exemplificatory, and getting them to explain. With other idiographic methods it is much more difficult to be genuinely surprised by the outcome. It happens, of course—skilled analysts prepared to really "work on" their raw data can come up with innovative insights by treating texts as puzzles to be solved. But with Q it is a common feature; some factors require considerable detective work in order to be articulated. It is this inbuilt abductive quality of Q which makes research so satisfying. Again Steven Brown is good at conveying what is on offer:

Q samples provide the launch pad for an investigation, an entrée into a phenomenon, the scientist's best initial guess as to how a particular administrative situation, social consciousness, or whatever operates. The data gathered with the Q sample may lead in quite different directions ... There is never any guarantee, in other words, that splash-down will occur in the same area as the point of departure. (Brown, 1980, p. 39)

This is not to say that we never use anything else other than Q methodology. We have worked-up a number of other methods of enquiry (see Curt, 1994 for more details) and generally use Q in conjunction with other methods, including various forms of cultural, textual, and discourse analysis (see W. Stainton Rogers, 1996, for a fuller explanation of difference between our use of discourse analysis and that adopted by Potter and Wetherell). However, this augmentative approach is not what discriminates our use of Q. Rather, our "climate of perturbation" theory-base means that our conceptualization of subjectivity is different.

Subjectivity

Throughout *The Study of Behavior*, Stephenson took great pains to argue (at a time when to do so was highly unpopular) that subjectivity is amenable to scientific study. It is clear from his writing that his use of the term "scientific" was quite the opposite of the scientizing positivism refuted above, but rather he meant the rigorous and systematic study of behavior, in the form of subjectivity-made-operant. He was absolutely clear that this was not a form of phenomenology or mentalism. But equally he rejected the idea that subjectivity is somehow not amenable to scientific study:

> It seems [un]profitable to draw any distinctions between what is objective and what is not, except in terms of dependable *operations*. According to logical analysis ... so-called "subjectivity," when it is not confused with "mind," is merely and indication of undependability, variability and the absence of "constant relations." The scientist is engaged in separating what is dependable from what is unstable or variable, and "subjectivity" is the name we give to the latter, the untamed horses of the scientific ranch. Similarly it is unsound, except on the grounds of convenience, to distinguish between "inner" and "outer," "internal" and "external," frames of reference; certainly there should be no imputation of the kind that one is scientific and the other not. (Stephenson, 1953, p. 88)

Hence, not surprisingly, the adoption of the term "the scientific study of subjectivity" is what holds Q scholars together. However, among those of us using Q theory and methodology within a "climate of perturbation" framework, our interpretation of what this means is somewhat different. Like Stephenson we agree that "'experience' and 'mind,' in any existential sense, are fictions" (p. 90), and with his analysis that "[i]ntrospection was brought into disrepute because it was

used as the means to an impossible end, namely, to discover 'sensations' and 'experience' as existential psychic matters, in a world apart from things; and certainly it is time to put this 'experience' out in the cold ..." (p. 92). So, if it is not some mentalist "psychic inner world," what, then, is this "subjectivity" that we are scientifically studying?

Our work is profoundly influenced by "French Theory" formulations around the notion of *discourse*, particularly the ideas of Michel Foucault about the relationship between "knowledge" and "power." Thus while we are interested in how certain ideas are made to "hang together" in the different accounts (or whatever) explicated through the factors identified in a Q study, our main concern is less with content than with purpose. I will come on to the implications of this a little later. More fundamental to our approach is the rejection of the possibility of any kind of "Grand Theory" about how subjectivity may be structured (for example, how many or how few factors there "really are"). We view "subjectivity," just like Stephenson did "experience" or "mind," as a convenient fiction, a device, no more, for making thinking easier. Equally, as Stephenson did, we view factor analysis as "merely a tool" (p. 339)—not as a means of revealing some psychic "essence of subjectivity" but as a convenient technique for gaining access to the way ideas, arguments, explanations, and representations may be "knowledged into being."

What all this boils down to is a conviction that we can never have any independent knowledge about "the world" of things and events. There is no possible "hot-line" to the truth about "real things," no means of knowing things-as-they-really-are. All knowledge is a human product; all the apparent phenomena, processes, events, and things that we observe around us and happening to us and to others, exist only in the sense that have been "knowledged into being."

It is this agnosticism over the world of "real things" that most people find difficult to accept about postmodern ideas. Possibly the most notorious example is Jacques Derrida's claim that the Gulf War is a fiction. Often this is taken to mean that Derrida is suggesting, preposterously, that it was no more than a figment of our imaginations, a virtual-reality peep-show set up in order to have us glued to CNN; that nobody was killed and no damage was done. This is not what he is saying. Rather he is using the shock-potential of such an outrageous statement to make a point—that the concept of "a war" is a matter of an arbitrary designation of a set of events and actions and consequences in an arbitrarily defined location, over an arbitrarily defined period of time. Before the Gulf War, for example, many thousands of Kurds

were massacred in Iraq. Since that time, who knows what horrors have been perpetrated there. These were "not-the-Gulf-War," but only because of a decision not to define them so (see W. Stainton Rogers and R. Stainton Rogers, 1997 for a more detailed exposition of this argument).

Derrida's claim, however, was not just a matter of semantics. Its purpose was to make a political point—that what we call things, how we conceive of them, has far-reaching consequences. The often-used example is calling an aggressor either a "freedom-fighter" or a "terrorist." Closer to Derrida's rhetoric, deliberate use was made of terms like "co-lateral damage" in the Gulf War, in order to detract attention away from the many people who were being killed and maimed as the "smart-bombs" were dropped on their targets.

It should then begin to make sense why we use Q as a form of discourse analysis, to gain insight into what is being "knowledged into being." Our primary concern is to find out about the purposes to which text (this is usually language but may sometimes be another symbolic form of representation such as a picture) is being used: what ideas are being peddled? What ideologies are being promoted? What is being covered up and who is being silenced? Who gains and who loses? It is this highly *politicized* agenda which marks our Q work off, we suspect, as different from much (although by no means all) of that being undertaken elsewhere.

Textuality and Tectonics

In order to go about this task we have appropriated the analytic of *textuality* from others such as Mulkay (1985), and devised another—*tectonics*—in order to adopt Q methodology for use in conjunction with Foucauldian discourse analysis (see W. Stainton Rogers, 1996, for a review of different forms of discourse analysis)—that is, one which addresses these kinds of politico-ideological questions. These ideas are treated in detail in a book we wrote as part of a collective, Beryl Curt[6] *Textuality and Tectonics: Troubling Social and Psychological Science* (Curt, 1994), including three chapters based upon Q studies.[7] Here we

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[6]The name of our corporeal author is an ironic almost-anagram, our own mischievous homage to Will Stephenson by way of lampooning his adversary.

[7]This book can be ordered direct from the publisher via orders@openup.co.uk. This is not a crude sales pitch, but a recognition that it is difficult to obtain outside of the UK!

will simply outline the main features of these analytics, and how they can be applied to Q research.

Textuality is an analytic which:

- Is agnostic over a singular "true" reality and asserts that, for any topic or matter of concern, there will be a diversity of alternative stories being told; for every object a diversity of alternative representations; for every set of alternative actions, a range of alternative pre- and pro-scriptions.

- Acknowledges no boundaries between different kinds of knowledges as textualized in different discursive locations (e.g. lay and expert knowledge; science and art).

- Dispenses with any need to or interest in establishing truth claims for any text.

- Encourages us to consider, instead the purposes to which texts are put (i.e. the action or praxis they enable, and/or the ideology they promote).

- While regarding all texts as epistemological equivalent, is an ideological endeavor requiring us to take moral responsibility for our analyses.

- While regarding all texts as local and contingent in their operation, focuses attention on the historical, political, and cultural contexts of textual production, maintenance, and interaction (i.e. their tectonics).

Thus the adoption of textuality as an analytic encourages us to explore how, where, why, and out of what certain texts are "knowledged into being" in particular circumstances and social ecologies, and are made to function in particular ways at particular periods of time.

Textuality as analytic also opens up questions about how, given there are always multiple texts concurrently in play, they affect each other coevally. However, it is insufficient in itself for exploring the interplay between discourses. We have therefore adopted tectonics as an analytic which is specifically concerned with the ways that the different stories and representations from which texts are drawn impinge upon each other as they are being produced, molded, activated, and archived across time and social space.

Central to the concept of tectonics is an acknowledgement that, once produced, stories can only endure if they are actively maintained. Like the pattern made by iron filings in an electric current (where the pattern dissipates as soon as the current is switched off), no story can retain its world-making ability without being continually rehearsed against

arguments and ideas which constitute our social world, or at the least, within the active musings of somebody thinking about them. Tectonics, then, is as much about the various ways stories and representations are marketed, mongered, driven underground, muted, adapted, reconstructed, and disposed of as it is about their production. It is about the rhetorical skirmishes of one against another; their rivalries and their allegiances; the playing out of dominance and submission.

Overall, tectonics, as we define and use it, is an analytic which:

- Assumes that new stories and representations never arise entirely spontaneously, but are crafted out of existing ones or the discursive "spaces" between them.
- Is concerned with the discursive practices whereby stories and representations are produced, maintained, and promoted.
- Asserts that no story or representation operates in isolation, but always in dynamic interplay with others.
- Focuses our attention on the forms that interplay may take, and the consequences that may ensue.
- Is concerned with the discursive practices involved in this interplay, in which one text is applied to another.
- Encourages us to explore the consequences of interplay, such as when one particular story gains dominance over (and thereby mutes) others.

Textuality, Tectonics, and Q

This brief outline of textuality and tectonics condenses a great deal of theory which we have developed elsewhere. I hope, simply, that it gives some "flavor" of what we are trying to do, and what we aim to achieve. I will end this paper, then, with a brief illustration of how they can be applied to a Q study on understandings of chronic pain in which I participated (Eccleston *et al*, 1997). Q method was used here for its capacity to examine "professional" and "lay" understandings using a single Q set, and thus to explore similarities and differences between them. Included in the study were three main groups of participants:

- Medical professionals without specific expertise in this field.
- Medical professionals with specific expertise in the field.

- Sufferers from chronic pain.

The factors identified demonstrated interestingly different accounts from each of them. Non-specialist medical professionals tended to account for chronic pain as a result of mismanagement of an acute condition in the past. Two psychologists' (one a health psychologist and one clinical) Q sorts were exemplificatory, and what is interesting is that the cause of the pain is given less emphasis than the *rejection* of anything that comes across as "unscientific," particularly psychodynamic explanations (such as pain being a consequence of unresolved emotional problems).

By contrast medical practitioners with specific expertise in the field (including five anesthetists, four psychologists, and a nurse, all working directly with pain patients) accounted for chronic pain as a product of dysfunctional learning. Chronic pain is seen to arise from patients having lost control and to have developed bad habits, allowing pain to become "a way of life." An original cause may well, in this account, be physical (for example, damage or simple decay with age) but the problem of the pain becoming a chronic condition is seen to arise from the patient not accepting that the pain is inevitable, and constantly seeking for a "cure" that is not available.

This account locates the "problem" within the patients themselves—in dysfunctional responses to earlier episodes of pain. By contrast patients located the "problem" in the medical profession. Seven chronic pain sufferers (together with one psychologist and one partner of a chronic pain sufferer) provided exemplificatory Q sorts for a factor which stressed that chronic pain *always* has a physical cause, which, they felt, had either been failed to be diagnosed in their particular case, or for which medical science more generally had been, as yet, unable to provide an explanation.

Looking at the way in which these three accounts tended to be articulated by different people, it becomes possible to speculate about why a particular explanation is offered. For pain sufferers the insistence on a physical cause absolves them from blame and reinforces the case that their symptoms deserve to be treated seriously—as real illness. In order to protect themselves from accusations of malingering or of imagining their symptoms, they pass on responsibility to the medical profession, arguing that it is medicine which is at fault (either individually or collectively) because of its inability to discover the physical cause.

For specialist pain practitioners, the explanation they give is that required by the operating theory which underpins their work. Currently

pain management involves various forms of cognitive therapy, in which patients are taught new ways of dealing with their pain. Clearly such a strategy can only be credible if one assumes that the problem is one of dysfunctional learning. Equally it is necessary to deny that a cure is possible, in order to absolve oneself from the responsibility to seek a diagnosis of the physical problem or a cure for it.

Finally, the non-specialists' preoccupation with rejecting any explanation that smacks of "psychobabble" can be read as another, but rather different, defensive strategy. Similar results have been found in other studies (for example, Leenders (1995) study of explanations of enuresis). What appears to be happening is that people who are in less prestigious levels of the medical hierarchy (in Leenders' case it was nurses who gave this account) feel the need to establish their credentials as "real scientists" by strongly rejecting items which are at all "unscientific" (in Leenders' case the strongest rejected item in this case was: *People born under water signs of the zodiac are particularly vulnerable to suffer from enuresis*).

We can see, then, that in each case not only does Q offer a means to access the alternative understandings being adopted, but it also provides a stimulus to conjecture about the reasons why different groups may adopt different explanations. These weave together a diversity of functions, including self-presentation, legitimation of professional or other roles, and so on. The Q study therefore provides an opportunity to examine issues around identity, professional power and authority, the attribution of blame, and the way action or inaction can be justified.

Inconclusions

Will Stephenson could be wickedly ironical at times. In *The Study of Behavior* he acerbically observed that "psychology, teeming as it is with investigators, is not altogether overburdened with important scientific discoveries" (p. 151). His words are even more opposite today, for certainly while the number of investigators have multiplied enormously, there is little sense of enormous progress on the scientific discovery front! It can be incredibly disheartening to browse through any journal devoted to the nomothetic "rush of fact-finding" (p. 89) that Stephenson complained about, though these days it is not so much a quest for facts, *per se*, which is at stake as publication records, job security, and promotion. In our view, Q studies stand out against these as both interesting and informative, and as giving a real sense that something

worthwhile has been, if not discovered, brought to our attention.

Finding Steven Brown's *Political Subjectivity* was, indeed, a life-line at a time when we were seriously demoralized–if it had not been there, could we ever have invented it? I doubt it. So let me leave Will with the (almost) last words: "Q-technique ... is a modelling device of great beauty, elegance and pregnancy" (p. 28). For all our differences, I think we can all agree with that. For some of us the pregnancy has given birth to something of a "gruesome child." We hope, for all her rebelliousness and trouble-making, she can still be welcomed into the family of Q scholars. Certainly, we promise, she will make life interesting.

References

Block, J. (1961). *The Q-sort method in personality assessment and psychiatric research*. Springfield, Ill.: Charles C. Thomas.

Brown, S.R. (1980). *Political subjectivity: Applications of Q method in political science*. New Haven: Yale University Press.

Butler, R. & Haigh, T. (1979). Changes in the relation between self-concepts and ideal concepts consequent upon client centered counselling. In C.R. Rogers & R.F. Dymond (Eds.), *Psycho-therapy and personality change*. Chicago: University of Chicago Press.

Cloninger, S.C. (1996). *Personality: Description, dynamics and development*. New York: Freeman.

Curt, B. (1994). *Textuality and tectonics: Troubling social and psychological science*. Buckingham: Open University Press.

Dryzek, J. (1990). *Discursive democracy: Politics, policy and political science*. Cambridge: Cambridge University Press.

Eccleston, C., Williams, A.C de C. & Stainton Rogers, W. (1997). Patients' and professionals' understandings of the causes of chronic pain: Blame, responsibility and identity protection. *Social Science and Medicine, 45*, 699-709.

Gergen, K. (1997). Social psychology: The emerging vision as text. *Contemporary Psychology, 41*, 1196-1197.

Glaser, B.G. & Strauss, A.L. (1968). *The discovery of grounded theory*. London: Weidenfeld and Nicolson.

Ibáñez, T. & Íñiguez, L. (1997). *Critical social psychology*. London: Sage.

Kitzinger, C. (1984). The Constructing of Lesbian Identities. Unpublished Doctoral dissertation, University of Reading.

Kitzinger, C. (1987). *The social construction of lesbianism*. London: Sage.

Klein, P. (1988). *Psychology exposed: Or the emperor's new clothes*. London: Routledge.

Lamiell, J.T. (1998). Nomothetic and idiographic: Contrasting Windelband's understanding with contemporary usage. *Theory & Psychology, 8*, 23-38.

Leenders, F. (1995). An examination of alternative explanations of enuresis: A Q-methodological study. Unpublished MA dissertation, South Bank Polytechnic.

Leventhal, H. & Hirshman, R.S. (1982). Social psychology and prevention. In G.S. Sanders & J. Suls (Eds.), *The social psychology of health and illness*. Hillside, N.J.: Erlbaum.

Moscovici, S. (1961). *La psychoanalyse: Son image et son public*. Paris: Presses Universitaires de France.

Mulkay, M. (1985). *The world and the word: Explorations in the form of sociological analysis*. London: George Allen and Unwin.

Mulkay, M. (1991). *Sociology of science: A sociological pilgrimage*. Buckingham: Open University Press.

Parker, I. (1992). *Discourse dynamics: Critical analysis for social and individual psychology*. London: Routledge.

Potter, J. & Wetherell, M. (1987). *Discourse and social psychology: Beyond attitudes and behavior*. London: Sage.

Stainton Rogers, R., Stenner, P., Gleeson, K. & Stainton Rogers, W. (1995). *Social psychology: A critical agenda*. Cambridge: Polity.

Stainton Rogers, W. (1996). Critical Approach to Health Psychology. *Journal of Health Psychology, 1*, 65-77.

Stainton Rogers, W. (1991). *Explaining health and illness: An exploration of diversity*. Hemel Hempstead: Harvester Wheatsheaf.

Stainton Rogers, W. & Stainton Rogers, R. (1997). Does critical psychology mean the end of the world. In T. Ibáñez & L. Íñiguez (Eds.), *Critical social psychology*. London: Sage

Stephenson, W. (1953) *The study of behavior*. Chicago: University of Chicago Press.