On the Mid Range: An Exercise in Disposing (or Minding the Gaps)

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Many efforts to establish concepts and theories of the middle range have sought to find an appropriate balance between theoretical abstraction and the desire to remain faithful to the empirical complexity of phenomenon. As with other forms of expertise, those analyzing socio-technical life face acute tensions in attempting to reconcile the general and the specific in a manner which is regarded as credible. Through a consideration of the self-referential implications of STS critiques of traditional notions of science as well as attempts to establish rules for the acceptability of physical force, this article develops a sense of what is at stake in attempts by actors and analysts to grasp the general and the specific together. Instead of seeking to identify a point of resolution for the two, it advocates and exemplifies the need to attend to the dilemmas associated with the movement between the general and the specific. The middle range concept of 'disposal strategies' is offered both as an aid to the study of socio-technical life and as an example of attending to the movement between the general and specific.

Key words: reflexivity; arms control; disposal strategies; middle range theory

Generalizations

Generalization is pervasive in inquiry and theorizing; whether one defines that as moving from a single instance to speak about many or as bringing together multiple instances to identify what is common between them. Central theories in Science and Technology Studies (STS) consist of abstracted generalizations based on the empirical study of a limited number of areas which purport to be applicable for understanding others. So, the social construction of technology (SCOT) was originally offered as the social construction of *technology*, not the bicycle, let alone the Penny-farthing in 19th century Britain. Taken individually, notions such as closure, stabilization, relevant social groups, and interpretive flexibility provided conceptual handles for studying a wide range of socio-technical developments. Taken together, such concepts provided the preliminary basis for a now well established program in STS.

For those that seek to devise programmatic frameworks or inform theoretical debates through empirical investigation, the extent of generalization – the shift from the specific to the general – is a central tension. On the one hand, one-off, throwaway, irreproducible claims about the world are of limited explanatory relevance. On the other hand, any generalizations must be faithful in some meaningful way to the objects or acts under question. Generalizations as generalizations always abandon much richness and messiness in favor of providing (relatively) tidy pictures. Intellectualizing, analyzing, and theorizing might be indispensable aspects of how understandings are formed, but they are also questionable in terms of what understandings they supply.

At the risk of presenting a Whiggish history, if I were say how I became interested in proposing a middle range concept, I would begin in early 2000. At the time I was studying so-called ‘non-lethal’ weapons, devices said to allow for the relatively harmless incapacitation of individuals. In this work I became concerned about a recurring disjuncture: while frequent claims were made by those in police agencies that formal policies were in place to ensure the controlled use of such weapons, when things ‘went wrong’ those that once touted the strictness of the rules often contended that it was unrealistic to expect that officers could act mechanically ‘by the book’ in conflict situations. These issues about formal rules and informal practices were particularly pertinent for non-lethal weapons because they must be used in

controlled ways to result in relatively little harm -- not too closely, not on certain individuals, not too much, etc.

In this situation, those charged with devising operational rules face acute tensions. By seeking to offer guidance, formal rules about what is and is not acceptable by the way of force entail reducing the complexity of socio-technical practice by establishing abstract, 'frozen in time' prescriptions. Through doing so, rules attempt to classify as similar situations that might otherwise be regarded as distinct. A central (but still rather fuzzily conceived) tension for me at the time was this: highly general rules offering only the vaguest standards risked being regarded as *ad hoc* and as a sign of lax policies, however precise stipulations would be inflexible and potentially open officers up to litigation for 'breaking the rules'.

As I was mulling over this predicament, Nick Lee (1999) sent me a publication regarding the challenges adult institutions face in adopting generalized policies about the institutional status of individual children. As he argued, rules often defer the resolution of difficult issues of status to some later date, a practice that can place considerable intellectual and emotional burdens on children.

After reading this article I began to think about deferral as a way of characterizing the processes for securing legitimacy in police policies. The 'rules' given to officers in England & Wales that left it for individual officers to decide on and justify the appropriateness of the use of the sprays on a case-by-case basis provided a vivid example of how the institutional tensions associated with generalizing were 'resolved' by deferring resolution (Rappert 2001). From my research I concluded the provision of such rules was important in two contingent dynamics: first the organizational devolvement of responsibility for the appropriate use of the sprays down the organizational hierarchy to individual street-level officers. An inter-related second dynamic was how in cases of serious injury what one maintained about the officers' adherence to (and ability to adhere to) operational stipulations affected determinations made about the safety of the sprays 'themselves' (as opposed to their 'unadvised use'). In other words, assessment of health effects, officers' competency and operational conditions were mutually constituted in relations of accountability.

Towards the Middle Ground

Just how those in the social sciences ought to respond to the tensions with generalizations has been recurring topic of commentary. One response has

been to propose middle range theories and concepts situated between abstract statements and complex empirical evidence. In relation to the wide ranging themes in this special edition of *ST&HV*, this article poses two questions: What have analysts sought from such 'a' middle ground? What might have frustrated its establishment in STS as posited by Wyatt and Balmer in the "Introduction" to this special issue?

Middle range theorizing has a long pedigree across a number of disciplines. An initial appreciation of the diversity in thinking can be gained from recounting classic works in sociology by Robert K. Merton and C. Wright Mills.

For Merton (1968, 39), middle range theories rested between 'the minor but necessary working hypotheses that evolve in abundance during the day-to-day research and the all-inclusive systematic efforts to develop a unified theory that will explain all the observed uniformities of social behavior, social organization and social change'. Role-sets and dissonance theory were presented as concepts and theories that were neither a collection of empirical observations made of a certain culture at a certain time nor the sort of totalizing theories of behavior proffered by Parsons. Writing in the late 1960s, for Merton middle range theories were perhaps best characterized as *intermediate* theories: that is, they fulfilled a necessary next step in the progression of sociology to a total system theory. Until sociologists assembled a number of middle range theories to build a total theory, they would never be accorded the legitimacy given to the physical sciences.

C. Wright Mills highly influential book *The Sociological Imagination* provides a similar but also contrasting appraisal of the mid range. Although he did not call for 'middle range theories' *per se*, Mills stressed the need to bridge focused empirical studies and 'grand theories'. Required were 'viewpoints that are simple enough to make understanding possible, yet comprehensive enough to permit us to include in our views the range and depth of human variety. The struggle for such viewpoints is the first and continuing struggle of social science' (Mills 1959, 148). Mills sought to span the lofty obscurantism of Grand Theory (e.g., Parsons' *The Social System*) and ahistorical, astructural analyses of Abstracted Empiricism. Whereas the grand theorists could never 'get down to the facts', the abstracted empiricists never got beyond Data and relied on an outdated conception of the Scientific Method. In contrast to both these programs, Mills advocated translating 'private troubles' into 'public issues'.

While employing Lee's deferral analysis generalized its import by showing its relevance for another substantive area, my initial interest in it was limited to aiding my analysis of 'non-lethal weapons'. As I thought about deferral though, I began to regard it as a rather pervasive dynamic in the operation of organizations. Agreeing to agree at a later date in some unspecified fashion was one way for moving forward in a complex or contested world since, as far as I saw, in neither their formulation or application did rules often serve as the definite guides to action often attributed to them.

It was good fortune that at this time that a friend suggested I read Heritage's 1984 *Garfinkel and Ethnomethodology* which makes a 'finitist' case for the insufficiency of rules to definitively determine action. The reasons for this insufficiency are many: a case must be made for the relevance of general rules for particular instances, rules must be applied in situations which are never in every respect identical and so their relevance must be worked out anew, rules never completely speak to all possibilities within the domain they are supposed to cover, and any rules about how to interpret rules must themselves be interpreted.

While these points are directly applicable to studying how organizations such as the police 'follow' rules, I also became interested in the two ways Heritage said ethnomethodological studies furthered general rule 'finitist' commentaries. One of these was linking a finitist approach to rules with a finitist approach to description. A key starting point for this is Schutz's characterization of language as 'typifying'. He argued language typifies through employing approximate terms for objects and actions that by some metric might be regarded as distinct. In other words, the trade-off between abstraction and specificity is built into language. In practice, the meaning of concepts and categories are constantly elaborated and what counts as a precise enough or too general a description is a matter for negotiation. Yet, despite the potential for communication to go awry, what is remarkable is the manner in which approximate, indeterminate vague concepts (when considered in the abstract) are clear enough for practical purposes. Paraphrasing Heritage I came to sum up a basic concern in ethnomethodology as this: What are the practical and situated ways in which social interactions make definite sense out of indefinite resources?

By tying my concerns to a much wider set of 'theory', I re-interpret my particular substantive concerns about rules in relation to much more general dynamics. Rather than deferral being a possible strategy

adopted in certain circumstances, it was a widely prevalent phenomenon. Treating descriptions as being partial, incomplete, and in need of contextual elaboration (Heritage 1984, 156-7) means the deferral of meaning is a pervasive feature of language and social interaction. How individuals span the gaps between rules and actions, words and objects, or texts and texts and what is accomplished in such active processes became significant concerns for me.

Turning Back to Face the Middle Ground

Merton and Mills thus suggested different appraisals of what constituted the needed middle ground as well as what the mid range was 'between'. This diversity suggests that those seeking to fill the gap between empirically rich cases and theories should attend to the possibility for multiple conceptualizations of the issues at stake.

What might account for a possible relative scarcity of middle-range theories in 'STS' was one of the concerns that motivated this special issues of ST&HV. As a starting point in considering why, it can be noted that many of the 'Core Concepts' that motivated the search for middle range theories in the 1960s are not prevalent in STS today. Perhaps most pertinent is how the conceptualization of social science and science has changed. Instead of adopting some model of Science, the contingencies and conventions of science-in-the-making have been central to the sociology of knowledge (SSK) and STS more generally. Indeed, it is opening up the historically situated and socially conditional practices of producing the general (or non-local) from the specific (or local) that is the central topic of many studies (e.g., Thackray 1998; Timmermans & Berg 2003). To be sure, there are defenders in STS of what loosely might be termed 'realism' (van Zwanenberg & Millstone 2000; Abraham 2002). Yet with the importance attributed to understanding how scientific knowledge is constructed, contested and contingent, much research today starts with presumptions involving particularity, flexibility, fluidity and multiplicity rather than generality, fixity, solidity and singularity.

If the conceptual and methodological issues that figured as central to classic debates about the middle range are not prevalent in STS today, then any lack of calls for such theories in terms of the former is hardly surprising. Yet as maintained in the first section, the extent it is advisable to abandon the complexity of empirical study so as to offer abstracted theories and concepts is a recurring tension in the social sciences. Therefore, even if it is unrealistic to expect debates in STS to be congruent with the concerns of

Mills, Merton or others, any lack of middle range-type theorizing demands attention.

One way to proceed is to ask what particular to STS could explain the relative lack of middle range generalizations. Arguably STS overall, and especially those lines of work associated with SSK, differ from other areas of social science in the type of the questioning of the adequacy of explanations. Whereas much of social science theorizing explains by substituting one object (e.g., religious beliefs) for something else (e.g., its social functions), 'such a substitution cannot be accepted so easily about a topic – science, objectivity, universality – which *alone* is not like *all* the other objects of study in the social sciences. This is because it is something to be looked down and explained, but also something that is to be looked *up* and the *ultimate* source of explanation' (Latour 2000, 109). In this way, the opening up of 'black boxes' not only questions the basis of scientific knowledge, but self-referentially the basis for the very analysis that purports to enable this opening up. The accusation of self-contradiction is never far from being leveled against those (somehow privileged) individuals that claim to reveal truth formation as a contingent process.

Despite the long time acknowledgement of the self-referential implications of SSK (e.g., Bloor 1976), just how analysts should respond has been a source of unease. Just when and how doubting should end is continuing problem, even if it is not often an overt topic for analysis. Gieryn and others, for instance, have developed perhaps the most well known candidate for a middle range concept in STS in the notion of 'boundary work' (though see Geels' and others' contributions to this special issue for further possible candidates). It is middle range in being a fairly easy to appreciate abstracted concept based on a limited number of empirical studies that provides analytical handles applicable for understanding a diverse range of cases. Moreover, it provides this without making totalizing systems claims. Yet in examining how lines are drawn between science and politics, expert and lay knowledge, and the social and the technical so as to secure the credibility of some claims over others, those employing the notion of boundary work are exposed to cries of self-referential foul in the way they draw boundaries so as secure the credibility of their own analyses (see Gieryn 1999, Epilogue).

Previous responses to the reflex to turn back have been varied, but arguably each raises questions about their adequacy. Conveniently setting such complicating issues to the side would appear the most common response. Latour (1988) elaborated a type of (infra-) reflexive 'story telling' that blunts

the potential sting of (meta-) reflexivity by appropriating reflexivity within ANT. However this was done only as part of offering novel definitions of 'science', 'social science', 'explanation' and 'objectivity'. Still others have chosen to revel in the 'methodological horrors' of reflexivity. The late 1980s and early 1990s, in particular, witnessed a flourishing of analysis that sought a more thorough going engagement with the theme of reflexivity. These questioned the limits placed on self-referential questioning in SCOT or the Strong Program. Ashmore (1989), Woolgar (1988), Mulkay (1985) and others sought to break with the conventions of empiricist writing through 'new literary forms' that disrupted the interpretation of texts and the acceptance of their correspondence to a reality out-there. And yet, however engaging this line of work, self-avowed reflexive forms of writing are now fairly rare in STS.

In light of the argument in this section about the rather exceptional type of questioning in SSK/STS regarding the basis for knowledge, instead of carrying on discussing in general why STS might be lacking in middle range theories, a more focused approach would be to turn some of the issues on their head. Considering why the long time preoccupation with reflexivity and associated new literary forms has not led to the development of a program of research is one way to reflect on the challenges offered to generalization within STS. So, in a stark form then, why isn't there a (even modest) Theory of Reflexivity in STS?

But to tell the story in this way is to get ahead of myself and foreshadow later developments. During the time my concern with rule following and organizational deferral was developing, so too was a preoccupation with the implications of a finitist approach to description for my own analysis. One of the reviewers of my CS spray article commented to the effect that it was performing various deferrals, this by displacing to the reader the troubling theoretical questions central to that article regarding the merits of relativist and realist approaches in STS. As well, the typifying language used about 'the effects' of 'CS sprays' was lapsing into conventional realist language that the article was committed to eschewing. As such, it could be questioned along similar lines regarding how it sought to credibly manage problems of generality it examined elsewhere. In brief, the request was made for me to attend to the self-referential implications of my argument that had been conveniently set aside.

Responding to such an invitation was not a simple undertaking. The pervasiveness of the tensions associated with managing the general and

the specific are at work in the very language used to describe these tensions.

The offering of accounts inevitably entail leaving elaborations unfinished and leaving certain issues aside to talk about the world 'out-there'. Just what picture our analyses provide, and whether a picture-representation is indeed what we want, are questions that can be asked and asked. Reification is a constant danger in offering (generalized) accounts.

This applies, for instance, to analyses such the one I offered above that attempt to summarize what heterogeneous fields of study like STS are really about and what is unique about them. Consider as well my previous discussion of rule-following. A good deal of debate has taken place about whether the problem of rule indeterminacy is really a problem of the application of rules or a deficiency by certain analysts in understanding what it means to apply rules (see Lynch 2005). Much of this is couched in relation to what constitutes a proper reading of Wittgenstein's description of rule-following. As a colleague, Nigel Pleasants, conveyed to me though, perhaps the very starting move to treat a rule as a *something-which-can-be-found-to-be-either-deficient-or-sufficient-through-conventional-analytical-argumentation*, provides a particular and questionable picture for thinking about the operation of rules. So too would (general) arguments about what Wittgenstein really meant in his later writings as if that should be neatly specified.

Those asking how to 'go on' (Wittgenstein 1967, §446) given the 'reifying', 'literalizing', 'typifying' tendencies of analysis face some thorny questions. The questioning of one's own accounts can never be total. This has not been achieved in the sections above and it will not be achieved in the rest of this article. At least one reading of the later writings of Wittgenstein provides some counsel though. As I understand it, he advised supplanting traditional philosophical analysis with a sort of *therapy*, a way of describing that fostered new sensibilities regarding the status of arguments and countered literalizing tendencies. Through this, many traditional philosophical problems would disappear because they would be seen to be nonsense.

(Following) Through the Looking Glass

Asking why the varied strains of reflexivity associated with SSK/STS never developed into a generalized program of research then, is offered in this article as one way of understanding the wide-ranging difficulties of conceptual generalization in STS.

At least one reason for the lack of a reflexivity program is that, in the never ending quest to query the conventions and dichotomies upon which analyses rest, self-identified reflexivists repeatedly and openly criticize the conventions, categories and dichotomies underlining in their own arguments. Steve Woolgar (2005) articulated the sort of destabilizing analysis sought in making the case for a form of writing where:

One is simultaneously a member of and a stranger to one's (own) argument, so that one produces (what looks like) a "conventional" text that at the same time develops and advances its own critique. The aim here is to see if we can produce an unstable argument which needs to be taken seriously, an argument which also constitutes its own ethnography, a text which is simultaneously at rest with and critical of itself. So let us be clear that this whole project is nuts.

Of course, to take this call 'seriously' would require not taking too seriously its face value distinction between 'conventional' and 'critical' texts; which then could be extended to not taking the call itself too seriously. Following 'reflexive' formulas is anything but formulaic.

A consideration of self-critical potential of reflexivity can be furthered by examining a presentation given by Steve Woolgar (2000) at the 2000 EASST/4S conference in Vienna. The presentation consisted of reading a letter from the industrial research user "Brian Peabody PhD OBE FEng" to Woolgar about the potential for reflexivity to further researcher-user interactions. Peabody recounts one reason for the correspondence as:

...an inquiry from a research student. An "Andrea Buccholz", wasn't it, who came to you at some point in late 1998. She had just discovered two of the classic reflexive texts of the late 1980s... Her simple, devastating question was: "what happened?"...Your interlocutor wasn't so tactless as to make her concern explicit: how come you weren't still writing all those dialogues and experimenting with form [?]

As Peabody goes on to write though, maybe even posing the question of why reflexivity has not taken off as a genre presumes indicators of success that self-respecting reflexivists would deem as 'inappropriate, precisely because they are not trying to emulate the usual form of institutionalization' and formalization of research central to defining a Theory.

Aka Peabody goes on to mention varied considerations that might explain the waning of reflexivity in STS. One, self-referential analyses often act as conversation stoppers, inviting cries of derision such as 'come off it' and 'oh for god's sake'. Also reflexive performances often reduce audiences to a silence 'because (some) ...seem not to provide any rules for how to respond'. Perhaps (though perhaps not) as Peabody contends, what has happened is that '[t]hrough a process of institutionalization and assimilation, the original concepts have become denuded of their radical connotations, the dangerous destabilizing tendencies become calmed, threats to established academic and intellectual organizations [...] neutralized.'

Lest anyone take the letter read too seriously, Peabody later goes on to say he will 'follow the reflexive formula' by suggesting a destabilizing form of writing to Woolgar that could aid in exploring question of researcher-user interaction. Writing could take the form of a text wherein:

you might have one of your main character(s) suddenly realize that s/he/they could perhaps fashion an entire piece which itself experimented with the analyst-user couple, perhaps by making the whole narrative into a letter latterly recently received by the author. But, as I've already intimated, I think this would be just too clever by half. And nobody is going to be convinced that the putative user voice is anything other than your own invention. On the other hand, I suppose this does at least open up the dynamic of the user relation, even if I appear as a grotesquely stilted figure in your narrative.

What hope then for a 'Theory of Reflexivity' with this sort of ever undermining, ever ironic self-referentialism?

Of course, some have criticized the extent of the self-turning back achieved as partial and selective. Pinch contended the reflexive approach of Woolgar had become formulaic (giving it the accusing programmatic label of reflexive sociology of knowledge [RSK]). For Pinch (1993, 512), Woolgar conveniently failed 'to attend to the lack of reflexivity in his own text'. Among the list of realist conventions Pinch identified included the unforgivable practices of attributing determinate meanings to texts cited, quoting bits of text out of their contextual complexity, lapsing into loose generalizations about what fields such as SSK were about, and using individuals to represent a way of thinking in a field. Pinch advocated one way to avoid formulas (as part of 'escaping from under the shadow of epistemology') was 'to stay close to the object of analysis and to treat that object seriously in its own terms' (ibid., 518).

In relation to the questions of this article, whatever the differences between the two authors, both their analyses could be read as recognizing the difficulties with generality and then purportedly siding with the specific. Pinch in his (ironic?) call (as co-founder of a program) for the importance of staying close to what is studied rather than relying on formulas. Woolgar in his concern with the specific textual accomplishments that build a picture of the world-out-there. However much both gesture towards the specific, though, their arguments also, inexorably and with varying performances of acknowledgement, rely on generalizations about the field and what can be gained from their approach.

These then were some of the predicaments associated with conceptual analysis as I began to appreciate them. Yet noting how studies of technology exhibit the tensions with generality and deferral identified elsewhere is one thing. Translating that into a fruitful research agenda is another.

One line of research I started was to examine how governments, NGOs and others justify assessments of the humanitarian standing of weapons. In recent decades, a variety of national and international conventions have been agreed that limit the development and use of certain weapons – from leg-irons to biochemical agents – because of their exceptional inhumanity. Trying to establish clear rationales for what weapons might be more or less appropriate begs a variety of questions: What makes killing and maiming by one method any worse than another? Why, for instance, should chemical weapons be deemed morally abhorrent as they widely are today, when in certain scenarios high explosives cause much greater harm? At a basic level is not there something slightly perverse with attempting to decide on the acceptability of some means of killing in any case? In short, the project of formally classifying the humanitarian standing weaponry might well be regarded as nuts.

Much of the literature in STS, of course, cautions against extracting a technology from its socio-technical assemblage to establish its acontextual qualities. Yet, while it is often recognized as problematic by those involved, such a move is undertaken in many attempts to justify humanitarian limits. My initial aim was to examine the recognized ‘practical’ and ‘analytical’ problems associated with making general claims about the humanitarian status of technology. Through offering my own general analysis of such problems I could then consider the similarities in the problems faced by ‘actors’ and ‘analysts’

in trying to advance authoritative claims about the world despite the problems of doing so.

With my interest in controversy rather than consensus, I was not so much concerned with the ‘taken for granted’, and ‘uninteresting’ methods by which people made definite sense with indefinite resources but how indefinite resources (e.g., treaties, policy statements) were used to manage the ever present problems of cutting through disorderly and complex socio-technical assemblages. The deferring of meaning in agreed limits is just one way of doing this (or actually two since my initial formulation conflated delaying in time and moving the site for ‘resolution’ around organizational hierarchies). Also of note are the practices of deflecting away responsibility for making a resolution, deterring others from judging how this gets done, and establishing relations of deference so that awkward questions are never posed. Each is a way of notionally determining what needs to be done without determining. Each is a way of coping with, rather than resolving, the persistent problems and dilemmas of substantiating the status of technology. In an article I coined the term ‘disposal strategies’ to refer to such practices because:

First, if there is no final, definitive, in principle resolution to the dilemmatic matters outlined, then a key question is how actors ‘rid themselves’ (or attempt to rid themselves) of the need to have to make resolutions. Second, ‘disposal’ also points to the active ordering, adjustment, bestowing, attributing, and regulating taking place in accounts. Each of the partial and provisional ‘resolutions’ ... is inextricably bound up with determinations of who or what is the determinant of causality, the bearer of responsibility, or the source of blame. Third, the accounts given are meant to persuade others about (or disposing them to) certain ways of seeing what needs to be done. Finally, the strategies are offered in relation to certain practical matters on specific, local contingent occasions and thus themselves somewhat disposable (Rappert 2005a, 234).

The notion of ‘disposal strategies’ thus speaks to the way classifications are supposed to say what is going on and what needs to be done despite the fragility, disputability, and contestability of doing so.

As with deferral, these other strategies are relevant for the way in which my own analysis varyingly passes on the problems of ‘resolving’ the never ending questions that can be raised about how it imposes a sense of order. I commented on this condition in a book length analysis of weapons prohibitions when I suggested that:

discussions about the merits of force and the prohibition of weapons are characterized by a complex inter-play between moments of

treating the world as fixed, determinate, and known and alternatively treating it as fluid, indeterminate and unknown. While attempts to capture some definite understanding of what is taking place and why that might underpin control measures are ever elusive, attempts to devise prohibitions necessitate trying to do just that. As argued, the fundamental and (in many respects) inescapable disagreements and controversies associated with specifying the acceptability of weaponry should serve to alert us to the pervasive problems associated with the analysis of this subject. As this book considers the contentions of devising and enforcing prohibitions, it seeks to work out something the problems of associated with analyzing the acceptability of force (Rappert 2006a, 4-5).

In asking what is at stake in debates about humanitarian prohibitions, I wanted to work out something of the intellectual problems of setting about to address this subject. So, the analysis in this book queried the 'craving for generality' (Sharrock and Button 1999) but, as it must have, relied on general claims in doing so; oscillated between realist and relativist stances towards knowledge claims; and marshaled categories while questioning the basis of making categorizations. Taking background inspiration from 'new literary forms' analysis in STS, the book sought to exemplify the dynamics it attributes elsewhere and through doing so appreciate the richness of what is accomplished in descriptions.

So What?

Doubts about the utility of those types of self-referentialism associated with SSK could also be offered to explain 'what happened to reflexivity?'. During the heydays of reflexivity in STS, arguments were made that such analyses provided little basis for either contributing to public debates or fostering analytical growth.

For instance, Winner contended that the methodological preoccupation with interpretive flexibility in constructivism undermined its relevance for public issues of day. Woolgar in particular comes under criticism, not for questioning what makes certain interpretations of the world persuasive, but in contending that this matter was 'simply not decidable' (Winner 1993, 374). Rather than staring into a reflexive hall of mirrors, Winner advocated that scholars should 'offer coherent arguments about which ends, principles, and conditions deserves not only our attention but also our commitments. At that point, one ceases interpreting interpretations of interpretations and, for better or worse, takes a stand on choices to develop or limit the technologies' (*ibid.*).

Collins and Yearley (1992) criticized the usefulness of SSK lines of reflexivity. Whereas Winner was concerned with political conservatism, Collins and Yearley were concerned with analytical conservatism. The basic trouble for the latter two was that SSK reflexivists 'would, if they could achieve their aims, know nothing at all' (*ibid.*, 302). Without a certain taken-for-granted social realism about the objects under study, reflexivity provided no basis for doing analytical work; 'nothing is realigned, nothing trembles, nothing falls' (*ibid.*, 302). Thus together, Collins, Yearley, and Winner urged an end to the reflexive regress in order to provide a basis for saying something about the world and getting something done in it. Such concerns with saying and doing have been central to arguments about the need for middle range theories in the social sciences.

In addition to considering reasons for the descent of reflexivity though, "Brian Peabody PhD OBE FREng" spoke to 'its' utility for fostering research interaction. Herein, the issues at stake were initially framed as ones where:

the problem of connecting reflexivity and interaction is a particular case of the more general problem of relating deconstruction/relativism etc with utility/intervention. [...That is] how can we intervene, act (politically), be useful, engage in outreach, interact with users etc while maintaining skepticism and anti-essentialism.

Yet, Peabody contends (he, but not Woolgar, might have said 'following the reflexive formula') such a formalization itself should be questioned in terms of its categories because which might inappropriately dictate practice. Yet, a countervailing consideration to this one noted in the letter is that while 'in principle, every truth claim, or even gesture, implication, hint, innuendo about reliable knowledge should be rooted out and have its constructed origins displayed in public. Yes, but one is tempted to say, what an effort, what a cost and for what?' It is all presented as rather confusing for the industrial collaborator, but he concludes by noting that '...it's precisely by thinking more about the social basis for interactivity that you can inform your practical interactions with users like myself.'

The question might well be asked though what all of this has to say about how to go on with the study of weapon prohibitions. Dwelling in the problems of resolution is all well and good, but where does it lead? With my interest in how actors (and analysts) try to rid themselves of the difficulties associated with drawing lines as well as

how such strategies are bound up with determinations of blame, my concern has been in asking this: what alternative ways are possible for handling the problems of classification?

I have since taken up this question in relation to cluster munitions. These weapons consist of containers that disperse several to a few hundred sub-munition 'bomblets' over a large area. Humanitarian concerns have centered on this large area (particularly when clusters are used near civilian populations) and the failure of many of sub-munitions to explode during their initial use. The latter means the sub-munitions become volatile ordnance that can be set off if disturbed. Just how many sub-munitions initially fail, with what consequence, and what the overall effects of cluster munitions have been on civilian populations are matters of significant disagreement.

Almost all of the existing debate in formal arm controls forums about the relative merits of this technology is framed in terms of whether it falls foul of the rules of international humanitarian law (IHL). Key in this is the notion of proportionality, that being a consideration of whether the anticipated losses of civilian life and objects from cluster munitions exceed the military advantage expected. Concerns about what should count as part of 'collateral damage' (e.g., are these only immediate consequences, possible long term injuries, loss of farming land, fear?) and military advantages (e.g., the tactical gains made from specific attacks, the overall contribution to the war strategy?) have been central to international discussions. Various questions recur in assessments of the advisability of banning cluster weapons: Is it acceptable to lump all 'cluster munitions' together under a general category? To what extent is it possible to modify certain versions or use them in certain ways so as to fix the source of the worst humanitarian problems? What is an acceptable rate of failure for sub-munitions to not initially detonate? Are civilian deaths in combat situations regrettable but unfortunately inevitable?

It would seem reasonable to me to characterize much of the argument and counterargument as involving disposal strategies of deferral, delay, deference and deterring alluded to in the previous section. For instance, the meaning of military advantage and civilian damage in IHL are hardly clear-cut, let alone the calculus by which they should be balanced. The frequent proposal by user and stockpiler governments that cluster munitions should be evaluated in a case-by-case manner by these ambiguous notions in light of the understanding held at the time of use by those in the field is highly consequential. It means that only those who are privy to the detailed, use-specific military deliberations

are able to judge how assessments should be conducted and what should be (and might have been) done.

My agenda has been finding ways of intervening in this debate that are not solely based on weighing facts, attributing essences, or searching for more precise rules. Rather, I want to be mindful of the (macabre) difficulties associated with trying to substantiate the relative acceptability of weapons, the necessary contingencies and conventions of accounts, and the limitations of formal rules as a means of regulating behavior. This does not spell out a neat formulaic answer for what ought to be done, but rather underscores the need for situated intervention.

With the persistent problems in analysis of attributing some definite and general character to objects and acts, my analytical strategy so far has been advancing forms of argumentation and intervention that shift the burden for substantiating what is and is not acceptable away from those primarily concerned with humanitarian consequences to those in positions of authority in user and stockpiler governments. So in one report written for the NGO Landmine Action (Rappert 2005b) I detailed the paltry effort made by the UK government to understand the humanitarian consequences of its use of the weapons over recent decades as well as the disposal strategies it adopted that were consequential in legitimating its use of cluster munitions despite this failings. A key aim here was to dispel the idea that decisions about the legality of weapons are made by the fictitious 'decision maker' posited under IHL who understands 'the whole picture' of the relative advantages and harms. A subsequent report then argued for jettisoning the dominant IHL case-by-case, notionally rationalistic approach for evaluating the acceptability of cluster munitions in favor a particular type of 'precautionary' approach (Rappert and Moyes 2006).

My basic aim in these reports and other forms of intervention has been to initiate a line of skeptical questioning attentive to the contingencies of analysis that can provide a basis for further questioning of conventional claims. I have sought through this to shift discussions away from legalist and empiricist forms of argumentation to instead focus on the social and political commitments made in situations of uncertainty and disagreement. The need to focus on *commitments* applies as much as does to governments determining their policies as it does to analysts deciding what to research. To be sure, this line of work has not sought the peace that Wittgenstein did from therapeutic analysis, in that the problem of analysis has not disappeared but instead

become the explicit topic of analysis in itself. Yet, as I have come to appreciate, arms prohibitions are rarely about peace.

The Middle Range?

On the basis of the argument given it might be said that both critics and advocates of reflexivity concur that it does not provide a basis for middle range theorizing: critics (and advocates) because if it did it would fail by sliding into formulism; advocates because of the espoused desire to continually question conventions rather than rely on them. Of course, though, such a statement could itself be taken apart for the way it marshaled given categories, terms and distinctions. As too could the manner in which the previous analysis employed simplified notions of STS, SSK, and new literary forms. As 'reflexive analysts' are forever employing generalizations, categories and dichotomies, the best they can hope to do is to simultaneously rage against and be at rest with their own accounts.

This article has not just been concerned with generalizing and reflexivity in their own right, but rather examined them to suggest something of the troubled basis for generalizations in STS overall owing to the self-referential implications of SSK. As argued, to the extent that those in STS seek to problematize the conventions and definitions of science, then the lack of any easy recourse to Science threatens the legitimacy of analysts' ability to make easy general claims to knowledge. The uncomplicated marshalling of categories and dichotomies that might provide the basis for theorizing is, at least notionally, to be shunned and those in STS that rely on simplistic categories risk criticism. In the sense, any 'failure' of reflexivity to offer such a basis generalizations is a 'failure' shared by an STS informed by SSK.

The question of how the traditions of relativism, indeterminacy and anti-essentialism in STS can mesh with the realism, determinacy and essentialism so as to offer generalizations has been and no doubt will continue to haunt (if not preoccupy) the field. The STS community conversation about how this might be done might have quieted down to a low murmur, but not because resolution has been found. While the course between the two poles is uncertain, let one guard against attempts to seek safe passage in a middle ground.

This is the sort of story that I could tell about disposal strategies looking back with a sense of the now. My use of the notion attends to

the problems of cutting up socio-technical assemblages to resolve the appropriateness of formal classifications of the relative unacceptability of force. In the intractable problems of doing so, alternative characterizations of the issues at stake alternatively locate and define responsibility, blame, and agency. 'Disposal strategy' as a concept is offered as nothing more (and nothing less) than a contingent generalization that suggests what is going on, but one in any case that leaves (disposes) much work to the reader in determining how to go on. It takes as its topic how generalizations as generalizations abandon much of the richness and messiness of empirical study in favor of providing (relatively) tidy pictures that fix the world in some partial and contingent manner that inevitably fails to capture an exhaustive understanding of what is going on. As such it is a way of speaking about and doing the processes of ordering and abstracting.

But if this notion is to be extensively embraced, then I need to illustrate its relevance for a wide range of substantive empirical areas; this rather than just limiting it to the narrow and rather sparsely populated research area of weaponry. One way to do this would be to enroll as resources other STS studies as exhibiting the disposal dynamic. So, for instance, this could include analyses of the way in which medical agencies are tasked with devising formal rules for appraising the quality-adjusted life-years pharmaceutical products for the purposes of rationing services (Sjögren and Helgesson 2004). Or analyses about how efforts are made to establish legitimate formal consumer systems for ranking the 'quality' of wine, which must define quality in an abstract and uniform sense despite the contingencies about what makes wine tasteful (Teil 2004). Aren't such cases of 'qualification' ripe with instances of 'disposal strategies'?

These are the sorts of translations necessary to propose abstracted generalized notions based on the empirical study of a limited number of areas that are applicable for understanding key dynamics across a wide range socio-technical development (e.g., as SCOT has done with concepts such as closure, stabilization and interpretive flexibility). If it is deemed persuasive, if 'disposal strategy' adequately spans the gap between abstract conceptual representations and empirical phenomenon, it might become a prominent, reflexively sophisticated middle range concept in STS. I don't wish to be crass, but do you think I can hold the general and specific together, dear reader?

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Notes

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