

Editorial

The anatomy of interpretation: Coming to terms with the analyst's paradox in professional discourse studies

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1. Introduction

The act of interpretation has an anatomical ring to it (Abercrombie 1969)—in terms of ways of seeing and the instruments used to facilitate the seeing. Categorization and interpretation are central to all human activity, but within discourse studies, they are both the object of study and the process through which we make sense of others' interpretive practices. The constructs and categories that underpin discourse analytic interpretation can reveal forms and functions of micro-phenomena, but at the same time they can blind one to alternative modes of description. Burke (1966) characterizes such interpretive practice as 'terministic screening', which may be a consequence of our 'trained incapacities': 'every way of seeing is also a way of not seeing' (Burke 1965 [1935]: 49). 'Terministic screening'—or what one might call 'deterministic seeing' or following Woolgar and Pawluch (1985) 'ontological gerrymandering'—amounts to 'directing the attention' into some channels rather than others:

Any definition of man in terms of specialised scientific nomenclatures would necessarily be 'over-socialised', or 'over-biologised', or 'over-psychologised', or 'over-physicised', or 'overpoeticised', and so on, depending upon which specialised terministic screen was being stretched to cover not just its own special field but a more comprehensive area. (Burke 1966: 52)

The circumference of analytic activity is a matter of limiting one's observation/interpretation. Arber (1964) provides the example of an average biologist who may take his/her microscope for granted and simply use it as a *tool*, whereas a maker of the optical instrument would critically appraise the microscope itself as an *instrument*. Likewise, a philosopher may question the theoretical assumptions underlying the biologist's observations without necessarily appreciating the findings. Arber (1964) argues:

The biologist, if asked *why* he narrows down his procedure by taking both his microscope and the fundamentals of his thinking for granted, might reply that life is

so short that, if he attempted to become an expert in scientific instruments, and also a trained philosopher, no time would be left in which to pursue his own *métier*, in which neither the scientific instrument maker nor the philosopher could replace him'. (Arber 1964: 80, emphasis in original)

There is thus an appeal to division of expert labor, while acknowledging the pitfalls of infinite reflexivity.

In the professional discourse setting we may assume that experts inhabiting a specialized domain share the same interpretive schemata. This is not to overlook the possibility of differential professional viewpoints, say in the healthcare setting, mirroring the fact that patients with the same disease condition may provide different narratives of illness experience. This raises a set of interesting issues about the positioning of the analyst of professional-client encounters and the limit to their interpretive license. What kind of data sources are required to form the basis of informed interpretive practice? What relationships do obtain between the researcher and the researched? What are the safeguards against over-interpretation? How do analysts go about validating their interpretive practices in such settings? These are some of the issues that have preoccupied Aaron Cicourel throughout his professional career, which I wish to elaborate selectively in the context of this special issue of *Text & Talk* to celebrate his work. Charles Briggs' guest-introduction and the article by Philip Davies and Hugh Mehan (this issue) offer a comprehensive overview of Cicourel's contribution to the field. A composite list of Cicourel's publications to date is also appended to this special issue.

Let me begin with Cicourel's reflexive orientation towards the activity of interpretation. An illuminating example—in the spirit of Burkean logology, i.e., 'words about words'—is his interpretation of Goffman's interpretation of everyday life. Cicourel (1974a: 23–24) writes:

Implementing Goffman's perspective is difficult because:

1. Goffman's assumptions about the conditions of social encounters are substantively appealing but lack explicit analytic categories delineating how the actor's perspective differs from that of the observer, and how both can be placed within the same conceptual frame.
2. All of Goffman's descriptive statements are prematurely coded, that is, interpreted by the observer, infused with substance that must be taken for granted, and subsumed under abstract categories without telling the reader how all of this was recognised and accomplished.

Consider the following:

When an individual enters the presence of others, they commonly seek to acquire information about him or to bring into play information about him already possessed. They will be interested in his general socio-economic status, his conception

of self, his attitude toward them, his competence, his trustworthiness, etc. (1959, p. 1)

How the actor acquires information (the interpretation of external symbols, the use of language categories) or utilises information already possessed so as to link the presumed knowledge ‘appropriately’ to a particular setting, requires explicit reference to inference procedures and a theory of how the actor assigns meaning to objects and events. But Goffman’s model of the actor does not reveal how the actor (or observer as actor) negotiates actual scenes, except through the eyes of an ideally situated and perceptive ‘third party’.

Cicourel (1974a: 25) goes on to summarize: ‘Even though Goffman provides rich “third party” descriptive accounts of the course of social exchanges, he does not tell us how the social analyst as observer and/or participant translates the “logic-in-use” of his field work into the “reconstructed logic” of his theorizing’. This critical, reflexive stance very much anticipates how Cicourel would set out to repair Goffman’s ‘third party’ accounts with a commitment to two key analytic constructs: actors’ reliance on ‘interpretive procedures’ (Cicourel 1974a) and researchers’ sensitivity to ‘ecological validity’ (Cicourel 1996, this issue). In what follows I comment on both these constructs vis-à-vis the notion of ‘analyst’s paradox’ (Sarangi 2002) in the context of professional discourse studies—a site that is close to Cicourel’s own research agenda.¹

2. The many faces of professional conduct

The recent years have seen a communicative turn in the study of professions and their clients, comprising different analytic frameworks—e.g., discourse analysis, critical discourse analysis, conversation analysis, interactional sociolinguistics, narrative analysis, rhetorical analysis. As discourse/interaction analysts we are more likely to be geared towards interpreting manifest performance (mainly language, but also to include visual, non-verbal and paralinguistic features), although professional knowledge and experience which inform professional conduct may not always be explicitly visible. In a critical vein, Lynch and Sharrock (2003: xxxix) suggest: ‘Although the sequential procedures that make up what conversation analysts call “talk in interaction” are evident in, and important for, the organisation of practices in a variety of social institutions, it is not enough to say that, for example, a jury deliberation or a medical diagnosis is an “organisation of talk”’.

Additionally, given the complex inter-relationship between language and context, what may be visible is not easily interpretable, because the phenomena we are observing can hide more than reveal. As Heath (1979:

108) rightly points out, professionals have been ‘socialised to have certain perceptions of their role in communicative tasks, and they have been trained to use language as an instrument to maintain that role and to accomplish ends often known only to them in interchanges’. Consider the study by Schefflen (1966), labelled ‘context analysis’, where he draws attention to the systematic step-wise lighting of a cigarette by the therapist as a critical moment in the psychotherapy session. The occasion of ‘cigarette-lighting and looking up’—rather than being a ‘side-involvement’ in Goffman’s (1967) terms—marks the client’s transition to patienthood.

Throughout his career, Cicourel has not only drawn attention to the interpretive challenges facing the researcher, but he has also routinely engaged with them in a sustained manner. As a point of departure, he critiques the general measurement trend in social sciences as he declares his inclination towards descriptive/explanatory aims and the search for ‘ecological validity’. According to him:

If the ‘rules’ governing the use of language to describe objects and events in everyday life and in sociological discourse are unclear, then the assignment of numerals and numbers to the properties of objects and events according to some relatively congruent set of rules will also reflect a lack of clarity. (Cicourel 1964: 15)

What Cicourel alludes to here as ‘lack of clarity’ can be linked to his concerns about survey research more generally, which is evident in his (this issue) strong endorsement of W. S. Robinson’s remark: ‘in social science research, reliability increases with sample size, but validity decreases’. By a similar token, he warns against viewing validity ‘as an inherent quality of “naturally occurring” and formal and informal “institutional” talk’.

In the context of professional discourse studies, a starting point is how professions are constituted in differentiation of specialized knowledge and the extent to which such knowledge is discursively articulated (Goodwin 1994) or remains tacit in the form of ‘knowing in practice’ (Schon 1983). This leads us to wonder which of the disciplines in social and human sciences have the necessary craftsmanship and the critical sensibility to unpack professional conduct without being constrained by Burkean ‘terministic screening’. Professional conduct, by definition, is complex, which defies disciplinary pigeon-holing, let alone different analytic frameworks within a given discipline claiming a privileged interpretive status. In their seminal work on interpreting psychotherapy, Gottschalk and Auerbach (1966) list as many as nine disciplinary contributories: human anatomy (e.g., what part does the patient’s anatomy, including genetic inheritance, play in his personality problems?); human physiology and biochemistry (e.g., how have physiological and biochemical factors affected the patient’s life history?); learning theory (e.g., how does the patient learn his

symptoms or unlearn his deviant behavior?); communication theory (e.g., how effectively do the patient and the therapist communicate?); theory of psychopathology (e.g., what theory of neurosis and psychosis does the therapist use?); theory of small groups (e.g., how does the patient establish interpersonal relationships with the therapist and others?); sociology and cultural anthropology (e.g., what are the cultural and social group characteristics that the patient has internalized?); value theory (e.g., does the patient have conflicts of values and are these relevant to his personality problems?) and measurement theory (e.g., to what extent are quantitative measurements appropriate and are directly observable phenomena distinguished from inferred entities?). Such a list foregrounds the need for professionals—in the healthcare domain more generally—to acquire interdisciplinary knowledge and apply it in their professional practice and/or defer expertise to their colleagues within a collaborative multiprofessional framework. This is a noticeable shift from the conceptualization of professional knowledge in terms of narrowly defined specialism that can identify and solve clients' problems.

Here a parallel can be drawn between the conduct of professional practitioners and the positioning of the analyst in professional discourse studies. In Garfinkel's (2002) sense, how does the analyst fulfill the 'unique adequacy requirement of methods' in order to be able to provide a description from the point of view of the observed? The scenario becomes further complicated when the observed—i.e., professionals and clients in a given encounter—do not share the same perspective. In particular, if the observer-analyst is not armed with adequate interdisciplinary, multiprofessional sense-making practices, it is likely to result in what might be called the 'fly-on-the-wall' syndrome. As O'Hanlon and Wilk (1987) observe:

A 'fly on the wall' who did not know we were doing psychotherapy would not necessarily suspect that that was what we were doing: he would see and hear only an ordinary conversation. What defines the conversation as psychotherapy is simply our goal in conducting the conversation. (O'Hanlon and Wilk 1987: 177)

This echoes Moerman's (1988) point about interpretive challenges in an intercultural setting. When the conversation analyst steps out and studies the communicative practices in another culture, gaps in interpretive practices may surface. As Moerman (1988) puts it:

All natives take their native knowledge for granted, take it to be nothing other than the nature of the world (Geertz 1975). But how could the conversation analyst recognise an utterance as a pre-invitation, for example, without trading on covert native knowledge of dating practices and the special significance for them of Saturday night? (Moerman 1988: 4)

Moerman here highlights the potential gap between participants' and analysts' interpretive practices and the extent to which this gap can be bridged in order to facilitate a better understanding of the phenomena under study.

3. Aligning actors' and analysts' 'interpretive procedures'

Based on Schutz's (1964) 'common scheme of reference' and Garfinkel's (1967) 'practical reasoning', Cicourel (1974a: 30) proposes the notion of 'interpretive procedures' which 'provide the actor with a developmentally changing sense of social structure that enables him to assign meaning or relevance to an environment of objects'. Following Schutz and Garfinkel, he identifies the following properties of 'interpretive procedures' (Cicourel 1974a: 52–56):

- Reciprocity of perspectives
- The *et cetera* assumption
- Normal forms: common knowledge, 'what everyone knows'
- Retrospective-prospective sense of occurrence
- Talk itself as reflexive
- Descriptive vocabularies as indexical expressions

More generally, Cicourel draws our attention to the relationship between interpretive procedures and normative rules—the latter are not easily accessible except via interpretive procedures participants deploy in a given setting. According to him: 'The interpretive procedures, therefore, have reflexive features linking their properties to actual scenes such that appropriate surface rules are seen as relevant for immediate or future inference and action' (Cicourel 1974a: 55).

Schutz (1964) alludes to members' sense making practices through creating loose equivalence classes, rather than through clear cut, either/or, true-false categories. Such a perspective is echoed in Garfinkel's (1967) conceptualization of the vagueness of norms as the '*et cetera*' property, which accompanies every norm. Everyone has to adapt the norm, or *ad hoc* the norm, to actually produce conduct in a given situation. So, all norms are essentially vague because they are partially open to adaptation in every situation of use.

Against this backdrop Cicourel argues that human socialization equals 'acquisition of interpretive procedures'. However, it is quite possible for members of a given society to face difficulties in operationalizing everyday concepts. In his study of Argentine Fertility, Cicourel (1974b) warns us against anthropomorphizing abstract collectivities or phenomena such as

'fertility', 'abortion', 'family size', 'tolerance'. He asks: How do social researchers interpret the meanings associated with key common concepts such as status, role, fertility both within the context of a research interview and when analyzing the research interview data? The problem of interpreting, for Cicourel, lies in the 'tacitly assumed meanings that are not clearly indexed'. He shows how different interpretations of the 'same' materials were mediated through researcher bias and the contingency of the setting.

This tension is particularly manifest when we shift our analytic lens to the professional/institutional domain. The position that participants' perspective allows one to categorize and interpret events unproblematically becomes untenable, especially when the knowledge gap between analysts and participants increases, as is the case in some sites of professional and organizational communication. While both Schutz and Garfinkel draw attention to the problem of analytic reasoning vis-à-vis the occasioned nature of interpretation, Cicourel attempts to approximate, if not solve, such analytic problems by a recourse to long-term ethnography as a way of acquiring actors' 'interpretive procedures'. Elsewhere I characterize this sustained involvement in a research site as 'thick participation' which constitutes a form of socialization in order to achieve a threshold for interpretive understanding (Sarangi 2006).

Cicourel (this issue) argues against the viewpoint that social interactions have 'a life of their own'—this reminds us of the 'fly-on-the wall' syndrome I alluded to earlier which presumes transparency of social interaction. Instead he emphasizes the 'socio-cultural and cognitive/emotional processes that constrain and shape discourse'. He is particularly cautious about equating participants' reasoning with analytic reasoning:

Forms of reasoning are viewed as central to the researcher's understanding of the way speakers and hearers presumably understand each other. The forms of reasoning we attribute to the participants of discourse parallel the reasoning we employ as researchers in making sense of the speech acts we record and listen to in arriving at some form of analysis. But as researchers we can, of course, specify formal aspects of discourse, produce systematic descriptions, and note emergent properties of the interaction. Yet we cannot attribute such properties unequivocally to the knowledge base of the participants. (Cicourel 1980: 101)

This echoes the broader tensions between analytic vs. everyday reasoning on the one hand, and between treatment of natural scientific and social scientific phenomena, on the other. The interpretive gap between the actor and the observer-researcher, according to Cicourel (1974a: 39–40), is part ideal and part academic: 'In actual practice, however, the actor's everyday theorizing is probably not much different from the observer-

researcher. Both employ the same interpretive procedures and similar typification . . . The observer-researcher must rely upon interpretive procedures when subsuming “recognized” behavioural displays under concepts derived from his scientific vocabulary’.

4. The interface of ‘interpretive procedures’ and ‘interpretive ethnography’

Cicourel’s notion of ‘interpretive procedures’ aligns with Geertz’s (1973) perspective on ‘interpretive ethnography’—which translates, following Gilbert Ryle, into ‘thick description’ of a social group’s ‘interworked systems of construable signs’, its ‘structures of meaning . . . and systems of symbols’ and ‘local knowledge’ (‘mutually reinforcing network of social understandings’). According to Geertz (1973), the analysts’ job is to take the informants’ own indigenous, locally-produced concepts (‘experience-near’) and ‘place them in illuminating connection’ with the ‘concepts theorists have fashioned to capture the general features of social life’ (‘experience-distance’).

Many discourse analysts, with their descriptive agenda, aim for ‘thick description’, although not based upon ethnographic insights. Labov and Fanshel’s (1977) study of psychotherapeutic interaction involves rules of discourse production and interpretation at the levels of ‘what is said’ (proposition) and ‘what is done’ (interaction)—in relation to what they call ‘cues’ and their ‘expansion’ surrounding A/B events. In a sense, the analytic strategy of ‘expansion’ seems to be drawing upon insider insights, which is facilitated by the collaborative partnership—along the lines of Geertz’s ‘experience near’ and ‘experience distance’ phenomena.

Ethnography generally claims to privilege the native’s point of view, i.e., it orients to participants without an outsider observer bias. Kleinman (1988) captures the ethnographic orientation as follows:

The ethnographer, first, tries to get things right from the native’s point of view. To accomplish this, he practices an intensive, systematic, and imaginative empathy with the experiences and modes of thought of persons who may be foreign to him but whose foreignness he comes to appreciate and to humanely engage. The ethnographer does not seek to go native—to become a Masai warrior, a Kung Bushman gatherer, a Mbuti Pygmy hunter, or a Yanomamo shaman—but rather he struggles to learn to see things the way natives do, to enter into their experiential world. (Kleinman 1988: 230)

This is similar to discourse analysts’ attempt at acquiring actors’ ‘interpretive procedures’, but not becoming actors themselves. Approaching

something from 'a native's point of view' is not the same as 'going native', although in some situations a close alliance between the two forms of knowing and experiencing is possible. Kleinman—a psychiatrist by profession and an anthropologist by training—draws a useful comparison between clinicians and ethnographers:

Master ethnographers and clinicians, though their work is quite different, nonetheless tend to share a sensibility. They both believe in the primacy of experience. They are more like observational scientists than experimentalists. Like the poet and the painter, they are strongly drawn to the details of perception. They struggle with the precision of communication to render percepts authentic, but they also have firsthand experience of the hiddenness of intimate meanings and of the disguising of experience that comes from social convention and personal defenses. The core truth of semiotics—namely, that everything can be a sign and that the relationships among signs are codes of broader and deeper meanings—is as available to the seasoned practitioner as it is to the anthropologist. The anchoring of the individual's attitudes in the local politics and economics of social relations is a given for both. (Kleinman 1988: 231–232)

As 'mini-ethnographers' (as opposed to 'master ethnographers'!), clinicians can place themselves 'in the lived experience of the patient's illness', and 'this experiential phenomenology is the entrée into the world of the sick person'. Kleinman (1988: 232), however, warns that unlike the master ethnographer, the clinician-turned-mini-ethnographer has 'a therapeutic mandate demanding that he must choose and act in behalf of his patients'.

Ethnography as a method of inquiry is not without problems—both in terms of its epistemological foundations and in its application. Malinowski's (1967) *A Diary in the Strict Sense of the Term* lays bare the implausibility of anthropological project *per se*. As Geertz (1983: 56) puts it: 'The myth of the chameleon fieldworker, perfectly self-tuned to his exotic surroundings, a walking miracle of empathy, tact, patience, and cosmopolitanism, was demolished by the man who had perhaps done most to create it'. Geertz goes on to critically evaluate the very notion of the 'native's point of view' and forcefully suggests:

The ethnographer does not, and, in my opinion, largely cannot, perceive what his informants perceive. What he perceives, and that uncertainly enough, is what they perceive 'with'—or 'by means of', or 'through' ... or whatever the word should be. In the country of the blind, who are not as unobservant as they look, the one-eyed is not king, he is spectator. (Geertz 1983: 58)

Fine (1993), among others, has listed as many as ten lies of ethnography, ranging from friendly ethnographer to honest ethnographer to observant ethnographer to unobtrusive ethnographer.

This is where the marriage of ethnographic insights with actors' 'interpretive procedures' in social encounters would be significantly helpful, as Cicourel (1987) has made abundantly clear:

Inasmuch as sociolinguistics tends to address various kinds of everyday events in which the analyst can 'pass' as an expert because of his or her background in language use and social interaction, the analysis of content is often viewed as fairly straightforward. By calling the reader's attention to the role of multiple ethnographic and/or organisational settings and informants, I underscore the complexity of the term 'context' or 'local' and more abstract sense of culture or social organisation for understanding language use and social interaction. (Cicourel 1987: 223)

In professional discourse studies, we, as language and interaction experts, may identify patterns of use (register, genre, action sequence, etc.), but to what extent can we claim expertise in our interpretation of context, or for that matter, content? How do we move from being language/interaction experts to becoming context and content experts? By extension, how do we deal with the apparent paradox that utterances can create their own contexts?

If it takes a context to map an interpretation onto an utterance, how can we extract a context from an utterance before interpreting? The idea that utterances might carry with them their own contexts like a snail carries its home along with it is indeed a peculiar idea if one subscribes to a definition of context that excludes message content. (Levinson 1997: 26)

Consider, for instance, the interactional structure of an encounter between a professional and a client which may be easier to map analytically. But it is more difficult to interpret the content of interaction and its relational dimension without a situated understanding of clients' lifeworld trajectories and the professional habitus in a given institutional ethos. As an example, the genetic counseling context of familial breast cancer requires us to understand what underlies a categorization of 'at risk' status (Sarangi 2005). Assessment of risk, following linkage analysis as opposed to DNA testing, crucially depends on the co-operation of first degree family relatives, in itself fraught with ethical, moral and psychological implications. A discourse analyst has to grasp the significance of 'genetic content' in order to be able to interpret how risks and responsibilities are formulated in a given counseling session. In general, discourse devices take on a special meaning in the professional setting and their interpretation would require a level of professional literacy (Sarangi in press).

Cicourel's (this issue) notion of 'ecological validity' is pertinent here. For him, 'ecological validity focuses on how we seek to convince others

of the viability and authenticity of our claims and can be understood by our use of primary and secondary data sources such as official statistics, demographic distributions, sample surveys, structured interviews, open-ended or unstructured interviews, and recorded discourse during social interaction'. In reflecting on a selection of his prior studies and in accounting for how he cumulatively approximated 'ecological validity', Cicourel calls for going beyond the convenience of marriage of meaning, i.e., not reducing ethnography to taking down background details as field notes and assigning them factual status at the interpretive stage.

5. Three paradoxes: Premises and consequences

Let me introduce the notions of observer's paradox, participant's paradox and analyst's paradox (Sarangi 2002), although I do not wish to suggest that observers, participants and analysts are necessarily separable in a given research setting. By a similar token, observation, participation and interpretation do not constitute distinctly different activities. For purposes of clarity, however, I will treat these constructs as separate.

Labov (1972) proposed the notion of 'observer's paradox' to draw our attention to how the act of observation itself can contaminate the data being gathered. By extension, we can only get authentic data when we are not observing. This pronouncement no doubt has severe consequences for ethnographic research where participant-observation constitutes the core data collecting activity. Labov succeeds in minimizing the observer's paradox by asking informants what he calls 'danger of death' questions in research interviews or by opting for covert recording of 'naturally occurring' interaction. In ethnographic studies there are examples of covert observation/participation involving marginal communities such as heroin users (Stoddart 1974); homosexuals in public places (Humphreys 1970); convicts in the halfway house (Wieder 1974). Researchers' attempt to overcome the observer's paradox (as in Stoddart's description of the use of the argot 'pinched' by heroin addicts) is indicative of problems associated with participant-observation and subsequent interpretive practice. *The Tearoom Trade*, where Humphreys takes on the voyeur role of watch-queen to announce the arrival of police or intruders, is beset with ethical problems as well.

The observer's paradox, it seems, is primarily concerned with collecting 'pure' data, with the assumption being interpretation of such data is unproblematic. In other words, analytic expertise pre-exists data collection. A further dimension of observer's paradox is the assumption that human behavior is fully tractable in its 'pure' form with or without the observer's

presence, as is the case with the study of phenomena in the natural sciences. Following Schutz, Luckmann (1983: 29) reminds us:

1. The data of social science are pre-interpreted. Interpretation of experience (and action) is a constitutive element of the data; we do not have 'raw' data to which are added common-sense interpretations which are to be discarded by means of some 'purifying instrument', if only we could find one (this is the old cosmology!).
2. Interpretations are made in, and bound to, ordinary historical languages. The data of social science are therefore from the outset irrevocably part of the historical worlds of everyday life: they are constituted in human action and experience as historically specific contexts of significance and motivation.

'Participant's paradox'—which can be seen as an extension of observer's paradox, but specifically concerned with 'naturally occurring' data settings—refers to the activity of participants observing the observer against a set of expectations. Participant's paradox can of course be extended to capture the participation structure more broadly in terms of researcher-researched relations. Bosk (1992) suggests that when the researcher is an 'invited guest' as opposed to 'an uninvited intruder', the role-relationships work differently, with differential expectations of usefulness—which will have ramifications for the researcher's methodological determination to remain detached and objective. Returning to our 'fly-on-the-wall' metaphor, the researcher-observer in a clinical setting may conceive of his/her 'involvement obligations' (Goffman 1967) in a manner which is not shared by the clinician-participant (cf. Goffman's 'sphere of participation' vs. 'sphere of focused interaction'). As Clarke, a clinician colleague, recounts:

The seating arrangements are important in promoting this naturalness—the observer should fit in as part of the circle of the doctor, counsellor, and family members. She should perhaps set herself back very slightly, so as to be out of the high-ways of looks and glances but yet able at times to participate in this at a low intensity. For the observer to sit completely outside the circle, perhaps at the back of the room or behind one of the professionals or clients, would be much more intrusive. Even small movements and sounds in a person's peripheral zone of awareness are registered and can be very unsettling to the primary participants—an observer may be less intrusive/noteworthy if they are easier to observe. (Clarke 2003: 381)

Participation in a situated activity is a two-way street, so cooperation and trust between the researchers and participants are central to the research process and outcome. Clarke (2003: 382) continues: 'Just as an observer trying to behave as a fly-on-the-wall is in fact more intrusive than one who behaves as a natural participant in the clinical setting, so health professionals are likely to behave more naturally if they are welcoming in

the observer and are actively engaged in the research process rather than being “subjected” to observation by the human fly’.

Finally, by ‘analyst’s paradox’ I refer to the activity of obtaining members’ insights to inform our interpretive practice, especially in light of tacit and layered embeddings of professional conduct. ‘As discourse researchers, we remain, for the most part, peripheral but legitimate participants, eager to rely on our subjects’ insights so that we can align (rather than transform) analyst and participant perspectives’ (Sarangi 2002: 122). This echoes Cicourel’s (1974a) attempt at bringing together ‘logic-in-use’ and ‘reconstructed logic’, in the same way that Geertz tries to reconcile phenomena which are ‘experience near’ and ‘experience distance’.

A key concern here is the extent to which actors’ ‘interpretive procedures’ are entirely recoverable from talk-in-interaction. Garfinkel (1974: 16) draws attention to the analytic conundrum which requires a balanced steering between “‘what we’re entitled to say” and “what the evidence shows” and “what can be demonstrated” and “what actually was said” as compared with “what you only think he said” or “what he seemed to have said””.

A ‘thick description’ without ‘thick participation’ may lead to a fly-on-the-wall account. In this sense, the analyst’s paradox can be linked to Burke’s ‘terministic screens’ and ‘trained incapacity’ that underpin the analyst’s ways of seeing and interpreting data. Fillmore’s (1984: 74) interesting analogy concerning data segmentation and cutting a pig’s carcass from the perspective of either a butcher or an anatomy student is instructive. While the butcher ‘makes his segmentations of the animal in accordance with the prevailing practices of local meat-eaters and his own inner urges’, the anatomy student ‘in disassembling the pig, traces a muscle from one of its moorings to the other’. By extension, the level of analysis and its embeddedness in the given context is also something to take into consideration. It is arguable, following Martin and Bateson (1993), that the fine-grained analysis characteristic of some interaction research paradigms lacks context:

A fine-grained analysis is only appropriate for answering some sorts of questions, and a full understanding will not necessarily emerge from describing and analyzing behaviour at the most detailed level. While a microscope is an invaluable tool in some circumstances it would be useless, say, for reading a novel. (Martin and Bateson 1993: 9)

Rethinking the microscope metaphor, Clarke (2005) suggests:

The analyst must steer between the Scylla of decontextualisation and the Charybdis of over-generalisation. A microscopist would remind us of the need to

use a lens of appropriate magnification—neither too high power (removing essential context) nor too low power (revealing insufficient detail). (Clarke 2005: 189)

Such analytic adjustments, far from distorting our ‘interpretive procedures’, may help us cope with, and reflect on, potential differences surrounding categorization of actions and events when working with professionals and clients as co-researchers. This may be seen as a step towards minimizing the analyst’s paradox.

The use of key informants in ethnography and interactional sociolinguistics has been forwarded as a solution to the analyst’s paradox. According to Gumperz (1997):

One solution is to have a local assistant who works with you on the analysis and who will tell you what it is in what he/she hears or perceives that leads to the interpretation. That gives you information at two levels, content and form. The more one works with such interpretive analysis, the more native-like one’s interpretations can become. (Gumperz 1997: 20)

This suggests that actors’ ‘interpretive procedures’ cannot be taken for granted, but must be worked at. It is probably more relevant in the context of professional discourse studies, given the distance between the analyst, the client and the professional practitioner.

It is perhaps useful to think of analyst’s paradox as a continuum. The analyst’s paradox is at its most extreme when interpreting core professional practices, that is the backstage activities, e.g., case records, peer-centered talk/text as in case presentations, case conferences etc. Front-stage professional-client encounters are easier to interpret, because we can put ourselves in clients’ shoes and draw parallels to similar experiences—in Geertz’s terms we have access to ‘experience-near’ insights. It is worth noting, however, that when a professional practitioner becomes an analyst of their own practice, they may find it difficult to maintain a distance from the object in order to appreciate it. This would be equivalent to the native bias that imposes taken-for-grantedness. Instead, informal, long-term involvement by an analyst, aided with collaboration, may minimize the analyst’s paradox, and thus maximize ecological validity.

In the following example, taken from the Foreword of a field-work based novel *The Bookseller of Kabul*, by Åsne Seierstad (2004: 6–7), the observer, the participant and the analyst are rolled into one:

I was spared having to adhere to the Afghan woman’s strict dress code, and I could go wherever I wanted. Nevertheless, I often dressed in the burka, simply to be left alone. A western woman in the streets of Kabul attracts a lot of unwanted attention. Beneath the burka I could gaze around to my heart’s content without being stared at in return. I could observe the other family members when we

were out, without everyone's attention being directed at me. Anonymity became a release, the only place to which I could return; in Kabul quiet places were in short supply.

I also wore the burka to discover for myself what it is like to be an Afghan woman; what it feels like to squash into the chock-a-block back rows reserved for women, when the rest of the bus is half empty, what it feels like to squeeze into the boot of a taxi because a man is occupying the back seat, what it feels like to be stared at as a tall and attractive burka and receive your first burka-compliment from a man in the street.

How in time I started to hate it. How it pinches the head and causes headaches, how difficult it is to see anything through the grille. How enclosed it is, how little air gets in, how quickly you start to perspire, how all the time you have to be aware of where you are walking because you cannot see your feet, what a lot of dirt it picks up, how dirty it is, how much in the way. How liberated you feel when you get home and can take it off.

I also wore the burka as a matter of precaution, when I travelled with Sultan on the unsafe road to Jalalabad; when we had to spend the night in a dirty border station; when we were out late at night. Afghan women do not normally travel with a bundle of dollar bills and a computer, so highwaymen usually leave the burka-clad women alone.

The burka—the veil—effectively allows Seierstad a sense of native-like involvement as well as access to first-hand experience of Kabul's everyday life. It is far from being 'covert' participation in the ordinary sense of the term. Her observations seem to approximate 'ecological validity' in Cicourel's sense.

6. Conclusion

The core of my discussion above centers on the problem of accessing professional conduct through interpretation and the extent to which our interpretation is, by default, *ad hoc*. The study of professions other than our own, is like interpreting another culture. Implied here is the call for discourse researchers to become 'mini-ethnographers' as a form of socialization is needed for aligning analysts' 'interpretive procedures' with those of the participants under study. In coming to terms with the analyst's paradox and by collaboratively interpreting *ad hoc* categories of professional practice, we can strive towards 'ecological validity' as Cicourel urges us. In my view, the more aligned actors' and analysts' 'interpretive procedures' become via 'interpretive ethnography', the more likely it is to approximate ecological validity—and what is currently being called 'respondent validity'—thus underlining our 'discriminatory expertise' if our research were to attain practical relevance. A gesture towards approximation of

analyst's paradox in professional discourse studies strongly coincides with Geertz's (1983) interpretive ethnographic experience:

Understanding the form and pressure of, to use the dangerous word one more time, natives' inner lives is more like grasping a proverb, catching an illusion, seeing a joke—or, as I have suggested, reading a poem—than it is like achieving communion. (Geertz' 1983: 70)

Note

1. Parts of my argument have previously been presented at the 9th International Pragmatics Conference (IPrA), 10–15 July 2005, Riva del Garda, Italy, in the panel honouring Aaron Cicourel's work (which forms the basis of this special issue) and at the 14th AILA Congress, 24–29 July 2005, Madison, Wisconsin, USA.

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